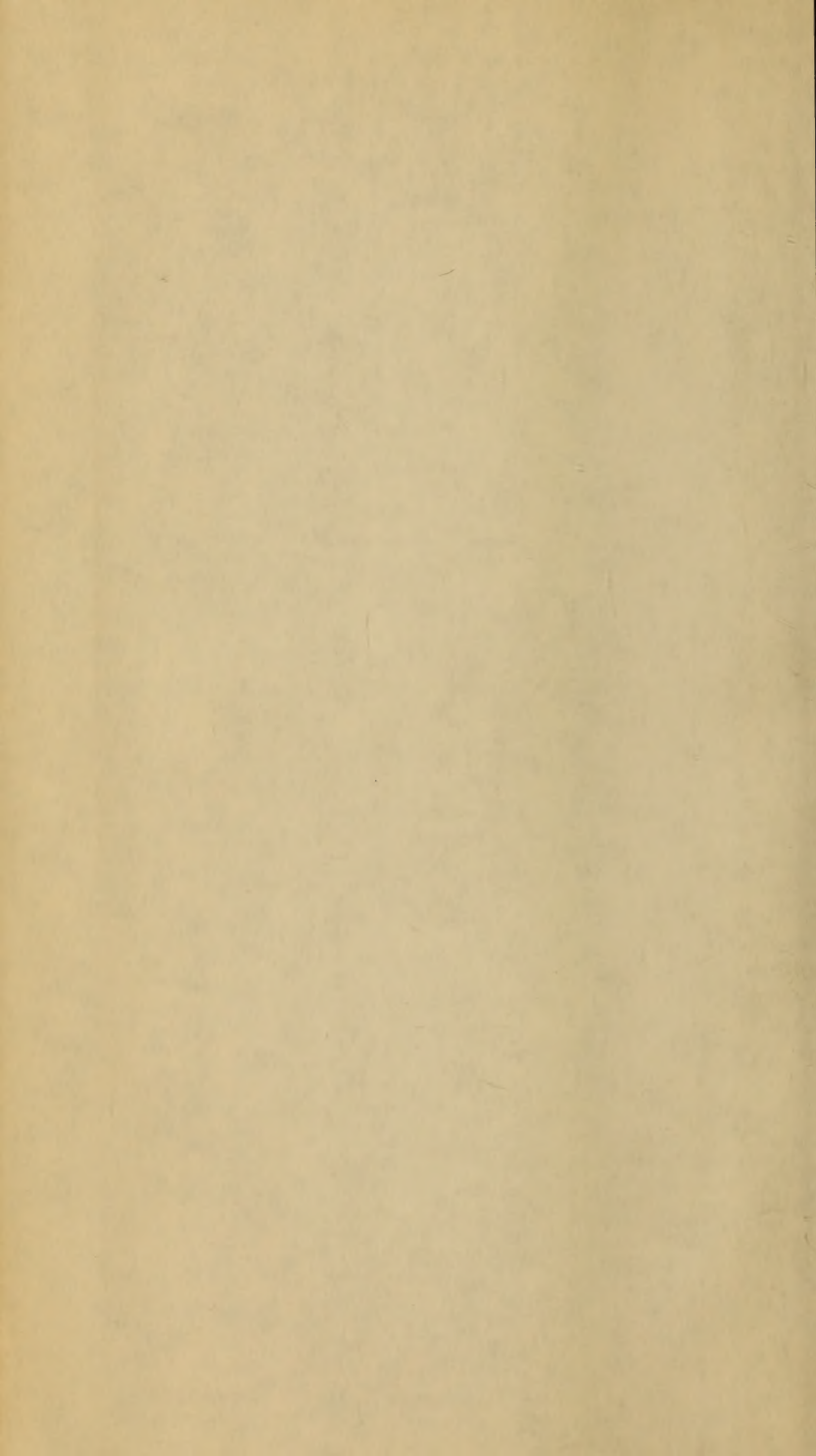


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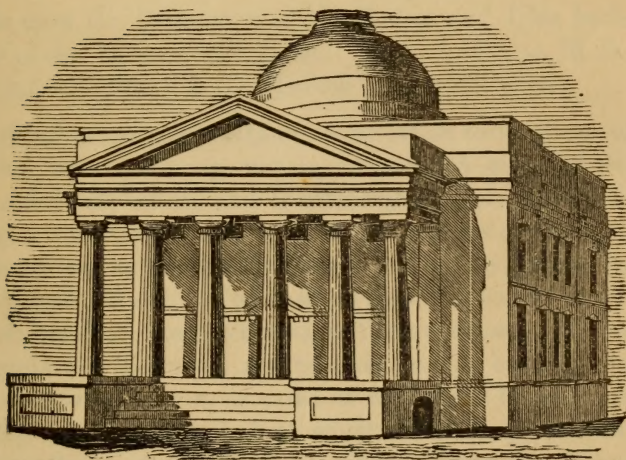


1851

SOUTHERN
MEDICAL AND SURGICAL JOURNAL.

EDITED BY

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Medical College of Georgia.

"Je prends le bien où je le trouve."

VOL. VII.—1851.—NEW SERIES.

Augusta, Ga.
JAMES McCafferty,
PRINTER AND PUBLISHER.

1851.

1392

SOUTHERN MEDICAL AND SURGICAL JOURNAL.

Vol. 7.]

NEW SERIES.—JANUARY, 1851.

[No. 1.

PART FIRST.

Original Communications.

ARTICLE I.

Remarks upon the Dislocation of the Radius and Ulna backwards at the Elbow—with Cases. By L. A. DUGAS, M. D.,
Professor of Surgery in the Medical College of Georgia.

The object of this paper is to direct attention to a species of dislocation, which, although not so common as some others, occurs sufficiently often, and is attended with consequences so serious, when upreduced, as to make its study a matter of great importance to every practitioner, and to the community who rely upon his skill. The cases to be subsequently reported will demonstrate the correctness of these premises. Errors of diagnosis are, however, not peculiar to our country, for Liston observes that, "many cases of unreduced luxation are met with; I have seen it in both elbows of the same person; and I have had a dozen of cases, in as many months, of unreduced elbows shown too late for attempts at reduction. The frequent occurrence of such blunders is the more lamentable, as it is almost impossible to replace the bones after three or four weeks; indeed I have been foiled at the end of two weeks."—(Elem. Surg.)

The bones constituting the elbow-joint are held in their normal position by the capsular, the two lateral, the coronary and the oblique ligaments, neither of which, individually, nor all combined, are sufficiently strong to resist the violence to which they are sometimes subjected. On the posterior aspect of the joint we find the great triceps extensor cubiti, inserted into the

olecranon process, and the anconeus, into the upper portion of the body of the ulna,—both of which muscles tend to strengthen the capsular ligament by their attachments to it. Anteriorly, the biceps flexor cubiti is implanted into the tubercle of the radius, and the brachialis internus into the coronoid process of the ulna and a portion of the bone below this point, being also attached to the capsular ligament. The displacement of such extensive articular surfaces, the rupture of the capsular and lateral ligaments, and the strong traction to which the biceps and brachialis internus are subjected, (the latter being sometimes lacerated,) will, especially when viewed in connection with the tension of the median nerve and brachial artery, sufficiently account for the painfulness of the injury and the seriousness of its character.

This accident is usually the effect of a fall from horse-back, from a vehicle, or from some other elevated position, during which the hands are thrown forwards for protection, receive the principal force of the shock, and transmit it to the elbow-joint. The bones of the fore-arm, being thus suddenly and violently forced upwards and backwards, rupture their attachments to the humerus and slip up behind the lower end of this bone.

The displacement is sometimes effected with such force as to cause the extremity of the humerus to protrude through the soft parts, and thus to constitute a compound dislocation, which is then readily recognized by the exposed bone in the bend of the arm. In simple dislocations, however, tumefaction takes place very soon, and becomes so considerable as not unfrequently to obscure very materially the means of diagnosis. It is true that, in the language of Sir Astley Cooper, "this dislocation is strongly marked by the great change which is produced in the form of the joint, and by its partial loss of motion." But when the case is presented to the surgeon, the form of the joint is often so much changed by the swelling, as to make it difficult to determine how much of the change belongs to this and how much should be attributed to a displacement of the bones. Indeed we find in the valuable work from which we have just quoted,* the narrative of a case under the Editor's

* A treatise on Dislocations and Fractures of the Joints. By Sir Astley Cooper. Edited by Bransby B. Cooper. Philad., 1844.

care, in Guy's Hospital, the true nature of which was not detected until the third day after its first examination, nor reduced until the fifth or sixth day. The patient was admitted on the 12th January, when "the tumefaction was so great as to prevent a very minute examination." * * * "Thirty leeches were applied, and afterwards the spirit lotion. The next day the swelling was still very great; twenty leeches were ordered; purgative medicine was administered, and the lotion continued. On the next day (Jan. 14th) the swelling was very much reduced, and the *nature of the injury became apparent.*" * * * * "15th. The swelling being much diminished, I made an attempt at reduction, but unsuccessfully, in consequence of the great pain it occasioned. Sir Astley Cooper corroborated my opinion as to the nature of the accident. A day or two after, I succeeded in reducing the dislocation." (Op. cit., p. 386.) We doubt not that every surgeon of experience has met with cases in which the diagnosis was obscured, as it was in this instance. It is therefore important that we possess ourselves of every means by which the nature of such accidents may be determined, even when the joint is swollen to the uttermost. Let us then dwell a little upon the symptoms and diagnosis.

The first circumstance calculated to awaken suspicion that the case may be one of dislocation backwards of the radius and ulna, is the position in which the limb is found. The fore-arm is in a state of semi-flexion and the palm of the hand turned upwards. Pronation is impracticable, save in a very slight degree. Any attempt to *extend* the fore-arm completely is attended with pain and great traction of the bicipital tendon; and if it be attempted to *flex* the fore-arm beyond a right angle with the axis of the humerus, pain will also be occasioned and the tension of the triceps be materially increased. The fore-arm may, however, be flexed and extended within the limits just mentioned, without much inconvenience. The wrist may be moved laterally, or carried from side to side, an inch or more beyond the axis of the humerus without much pain. If we now extend the fore-arm as much as may be, and examine the anterior aspect of the joint, a considerable prominence will be found in lieu of the depression which usually exists in the bend of the arm. This resisting tumour terminates abruptly at its

lower margin, is formed by the inferior extremity of the humerus resting in front of the radius and ulna, and may generally be easily traced by the fingers to be continuous with the shaft of the humerus. The brachial artery is found in front of the tumor, is unusually superficial and beats with great force. The tendon of the biceps is also very prominent and tense, as well as the belly of this muscle.

The lateral surfaces of the joint are much thicker than in the normal state, because of the great increase in the antero-posterior diameter. When the fore-arm is semi-flexed, there exists a depression on either side of the olecranon, occasioned by the increased prominence and distance of this process from the humerus.

If the joint be examined posteriorly whilst the limb is extended as much as possible, and a line be drawn across it from one condyle to the other, the olecranon will be found considerably above this line. In the adult, it ascends from an inch to an inch and a half beyond its usual position. In females and younger subjects, this displacement will be less marked. In the normal state, the external condyle is said to be on a level with the upper end of the olecranon, and the internal condyle a little above it. The head of the radius may also be felt (if the tumefaction be not very great) on the external side of, and a little below the point of the olecranon, and its identity as well as the integrity of the bone determined by attempting to rotate the wrist with one hand, whilst a finger of the other is pressed upon the head of the radius. If the radius be unbroken, its head will be found to follow the movements of the wrist.

The diagnosis will be materially facilitated by comparing the two arms placed in the same position, and by measuring in each the distance from the lower end of the ulna to the internal condyle, or to the point of the acromion process. The dislocated limb will be the shorter of the two.

Some of the symptoms just enumerated may be presented in other injuries of the joint or of its adjacent structures. The dislocation of the ulna and radius may be *lateral* and backward, in which case the coronoid process of the ulna will be found resting upon the posterior surface of the external or internal condyle, instead of lodging in the fossa of the humerus, as it

does in dislocations directly backward. If the ulna rest upon the *external* condyle, the head of the radius will present itself upon the outside and somewhat behind the joint, where its rotation may be distinctly felt during pronation or supination. If, on the other hand, the ulna be thrown behind the *internal* condyle, the head of the radius will occupy the posterior fossa of the humerus. These peculiarities will not permit the *lateral* to be confounded with the *direct* dislocation of the elbow-joint backward.

The ulna may be alone dislocated backwards, leaving the radius in its proper place. In this case the fore-arm can neither be extended, nor flexed much beyond a right angle; but its inward inclination, the absence of the head of the radius behind the humerus, where the great projection of the olecranon is to be distinctly perceived, will suffice to establish the differential diagnosis.

In forward dislocations of the radius, its head is thrown in front of the external condyle and against the coronoid process of the ulna. The hand is pronated, the fore-arm slightly bent, flexion to a right angle resisted by the head of the radius striking against the front of the humerus, and complete extension impracticable without reduction. The natural position of the olecranon, the absence of the head of the radius from below the external condyle, and the sudden stop to the flexion of the fore-arm, will sufficiently distinguish this from other injuries. If complicated, however, with fracture of the upper end of either or of both bones, the diagnosis may be more difficult. These accidents are of rare occurrence, and may be detected by careful attention.

Backward dislocation of the radius is much more rare than that forwards, and can scarcely be confounded with the dislocation of both bones, although it does, to a certain degree, impede the movements of flexion and extension.

A fracture of the upper portion of the radius may be attended with considerable tumefaction, and will render flexion and extension of the fore-arm more or less painful. If the fracture be *below* the tubercle, this will be drawn up by the biceps so as to wound the soft parts in these motions of the fore-arm, and if the fracture be *above* the insertion of the biceps the lower frag-

ment will be likewise elevated and occasion the same inconvenience, although in a less degree, during extension than flexion. This action of the biceps, by removing the fragments from each other, will often prevent the crepitation upon which so much reliance is placed in the detection of fractures. This fracture may be distinguished from the dislocation we are studying by the absence of the tumor in the bend of the arm formed by the lower end of the humerus, by the natural position of the olecranon, by the limb not being shortened, by the relaxed state of the biceps and its tendon, &c.

TREATMENT.—“The patient is made to sit down upon a chair, and the surgeon, placing his knee on the inner side of the elbow-joint, in the bend of the arm, takes hold of the patient’s wrist, and bends the arm. At the same time he presses on the radius and ulna with his knee, so as to separate them from the os humeri, and thus the coronoid process is thrown from the posterior fossa of the humerus; and whilst this pressure is supported by the knee, the arm is to be forcibly but slowly bent, and the reduction is soon effected. It may also be accomplished by placing the arm around the post of a bed, and by forcibly bending it while it is thus confined. I have also reduced the limb by making the patient, whilst sitting on an elbow-chair, put his arm through the opening in its back, and then, having bent the arm, the body and limb being thus well fixed, the reduction was easily effected.” Such are the processes recommended by Sir Astley Cooper. More modern writers, however, as Liston, Miller, Druitt, &c., advise the reduction to be effected by forcible *extension* of the arm, and we doubt not that it is in many cases decidedly the best mode. We transcribe the directions given by Liston in his *Elements of Surgery*. “The arm and forearm are extended, and the limb is brought well behind the trunk, so as to relax the triceps; then the surgeon performs extension and counter-extension, pulling the fore-arm with one hand, whilst he pushes with the other placed on the scapula. If the force thus employed prove insufficient, as it seldom will in recent cases, the patient may be placed on his face, on a couch, and on the limb being brought into the favorable position already noticed, counter-extension

may be made by the heel planted against the inferior costa of the scapula, whilst the wrist is pulled with both hands. It is seldom necessary to employ pulleys, except in cases of old standing; if so, the only peculiarity in their application to this joint is the direction of the force, backwards. And this I consider to be a very material part of the manipulations, for, by attention to it, I have succeeded after previous failures,—after great force had been applied, creating excoriation and swelling of almost the whole limb.”

It is evident from this extract that Liston regarded the relaxation of the triceps as the peculiar advantage secured by this mode of reduction. Professor Miller, of Edinburgh, entertains the same opinion. “The patient is placed with his back to the surgeon; and, the chest having been fixed, extension is made with the arm directed completely backwards, in a rectangular relation to the trunk, *so as to relax the triceps muscle.*” (The Prac. of Surg. By James Miller, &c. Philada., 1846. p. 235.) Druitt expresses himself differently: “He (the surgeon) may forcibly straighten the arm, so as to make the tendon of the biceps pull the *trochlea* of the humerus back into its place.” (The Princ. and Prac. of Mod. Surgery. By R. Druitt. Philada., 1848. p. 283.) With due deference to these high authorities, I think that a very important element in the mechanism of this process has been overlooked; which, if borne in mind by the surgeon, will materially increase the chances of success. I allude to the lever power secured by using the olecranon as a fulcrum for dislodging the coronoid process from the posterior fossa of the humerus. This effect will be readily perceived if the reader will place the bones of a skeleton in the position they would occupy in this dislocation, and then gradually extend those of the fore-arm, making at the same time gentle traction. It will be found that the resistance offered to reduction is principally produced by the lodgment of the coronoid process in the fossa—but that, as soon as the extension is carried a little *beyond the straight line*, the olecranon will rest upon the humerus, the coronoid process will rise from the fossa and the bones will promptly slip down into their proper position. The surgeon should therefore carry the fore-arm *a little farther back* than a straight line with the hu-

merus, if he wish to derive all the advantages of this method of reduction.

It is scarcely necessary to say that if the dislocation resist a certain degree of force, whether applied with the arm flexed or extended, prudence would dictate a cessation of efforts, rather than hazard the consequences of such lacerations as might be produced, especially in old cases, and with pulleys. Unless the limb have been injudiciously splintered in a straight direction for some length of time, complete stiffening of the joint will seldom occur. The bones will gradually adapt themselves to their new position, and an useful degree of motion be regained. Too violent efforts, by renewing the inflammatory process, may result in the loss of even this small amount of usefulness, and perhaps more seriously.

The reduction having been effected, the joint should be surrounded with cloths dipped in a saturnine solution, vinegar and water, salt and water, camphorated spirits or any other refrigerating lotion. A roller bandage should be then carried gently above and below the joint in the figure of an 8, and the forearm placed in a sling. The lotions should be frequently applied, without removing the bandages. If inflammation be intense, leeching and other antiphlogistics must be resorted to. After the lapse of a few days, or the subsidence of inflammation, the arm should be daily subjected to slight motions of extension and flexion, pronation and supination. The length of time necessary to retain the limb in a sling must vary according to circumstances. In ordinary cases it may be removed in two or three weeks.

The writer has seen seven cases of dislocation of the Radius and Ulna backwards, and will now very briefly refer to them.

CASE I. Miss A. E. H., having sustained an injury of the elbow-joint by a fall from a moderate height, had been treated three weeks with discutient lotions and a nearly straight splint, when I was requested to see her. The attending physician, whom I met in consultation, not agreeing with me as to the nature of the accident, a third physician was called in, who differed from us both. The opinion of a fourth was now requested, and he fortunately concurred with me in the conviction

that the bones of the fore-arm were dislocated backwards. The patient was immediately seated in a strong chair, with the injured arm projecting through the back of it. A little tobacco was put into her mouth to promote a relaxation of the muscles, and, when nausea supervened, the dislocation was reduced by pulling the fore-arm strongly, and at the same time bringing it into a state of complete flexion around the vertical bar of the chair. The arm, being placed in a short sling, and occasionally moved, was, in the course of a few weeks, completely restored to its natural uses.

CASE II. This case presents nothing peculiar. It was that of a little girl from South Carolina, about 7 years of age, who fell from a chair, and dislocated the elbow-joint. She was sent to me a few days after the accident; the nature of the injury was detected, and reduction immediately effected as in the above case, but without the use of tobacco.

CASE III. Mr. St. J. (about 30 years of age) was thrown from his horse with great force, a short distance from the city. It was probably two hours after the accident that I saw him. The arm was semi-flexed, and the tumefaction of the elbow-joint immense. Another physician was requested to see him with me, but differed with me as to the nature of the injury. The joint was therefore covered with cold lotions and gently splinted until the next day, when I succeeded in convincing my *confrere* that we had to deal with a dislocation of the joint. The reduction was effected by drawing the fore-arm around the bed-post until flexion was complete. The case terminated successfully.

CASE IV. Mr. W., from one of the lower districts of South Carolina, about 20 years of age, in falling from his horse, sustained an injury of the elbow-joint, which was treated with lotions and splints for six weeks before he called upon me. The arm was nearly straight from the continued use of splints, and possessed very limited motion at the elbow. The tumefaction having entirely subsided, the dislocation of both bones backwards was very obvious. After making, in vain, every effort that prudence would permit to reduce the dislocation, I ventured upon the subcutaneous section of the triceps extensor cubiti, just above its attachment to the olecranon, thinking

it possible that by removing what then seemed to be the greatest obstacle to the flexion of the fore-arm, I might succeed in bending it, and that, even if he lost the use of this important muscle, the permanent flexion of the arm would be better than a permanent straightness. I did, then, succeed in bringing the fore-arm nearly to a right angle, but without reducing the dislocation. The patient was directed to move the arm daily, and to shorten the sling in which it was kept, from time to time. About five years have now elapsed since the operation, and I have not heard the result.

CASE V. This was a case somewhat similar to the last. The gentleman (about 25 years of age) resided in one of our upper counties, and had been treated several weeks under a misapprehension of the nature of the injury. He then came to this city and placed himself under the charge of another physician. I was called in consultation and we detected a dislocation of the elbow backward. My professional associate administered chloroform and made every effort to reduce the dislocation, in vain. Under the influence of the powerful aid of Jarvis's adjuster, the attempt to flex the arm resulted in the fracture of the olecranon. This permitted considerable motion of the joint, and the patient left here much improved in that respect—but with the dislocation still existing.

CASE VI. This is the case of a robust Irish laborer, whose dislocation was of some weeks standing, when I was requested to assist a professional brother in its reduction. Chloroform was administered, and the most strenuous exertions made without succeeding in the reduction.

CASE VII. F. W., a boy 7 years of age, fell from a platform about 4 feet high, and dislocated the bones of the fore-arm backwards at the elbow-joint. Seeing him a few hours after, the dislocation was readily reduced by traction with my right hand, whilst the other was applied to the bend of the arm so as to press back the bones.

It will be remarked that of the seven cases above reported, three remained with permanent deformity—and that this result was in these three cases, the consequence of incorrect diagnosis. With these facts before us, the writer trusts that this communication may not appear to be altogether a work of supererogation.

ARTICLE II.

Observations on the use of Veratrum Viride in Fevers, Convulsions, &c. By WESLEY C. NORWOOD, M. D., of Cokesbury, S. C.

In a former communication (see June No. of this Journal, vol. 6,) we stated some of the leading and prominent powers and properties of American Hellebore and its peculiar adaptation to the treatment of Pneumonitis. We then stated, that all the powers and properties it was alledged to possess, were true and free from all exaggeration. We now go farther, and state, from the fact that some are disposed to doubt the validity of the assertions, that the half was not told. Its powers were not fully unfolded nor revealed, lest by giving a too glowing and brilliant statement of its capacity to arrest and subdue disease, it might meet the fate of many articles which flourished no longer than the short time occupied in breathing their praise. We challenge trial, and pledge ourself to demonstrate at the bedside, before any body of physicians, every power and property we have heretofore claimed for it and asserted it to possess. We go farther, and state that all of its leading effects are so striking that we are compelled to attribute them to the article, and to nothing else. We are sorry that we dwelt so long on the ill effects, if it can be said to possess any. It is not a drastic emetic in the sense that tartar emetic is said to be. Its nauseant powers are the most disagreeable of any effect belonging to it. From the description given, we learn that it deterred many from using it, and led others to question its usefulness. We deemed it far more prudent to enter fully into any unpleasant effects, than to keep them concealed, and present nothing but its prominent beneficial effects. We now boldly hazard the assertion, that it is the only article or agent known that will control certainly, and without disappointment, the action of the heart and arteries—that it is the only therapeutic agent known through which we can say to the heart and arteries, so fast shalt thou beat or pulsate, and no faster. We unhesitatingly assert, that it has not failed us in a single case to reduce the action of the heart and arteries, down to any point we wished, as to the number and frequency of pulsa-

tions. We believe that it will be eminently useful in every disease in which increased frequency of the action of the heart and arteries take place to any extent. We also believe that when its powers are fully ascertained, tested and developed, it will produce an era in the treatment of disease.

We have no doubt that *veratrum viride* will prove a valuable agent in the treatment of scarlet fever. We stated in our former article in this Journal, that *veratrum viride* certainly reduced the frequency of the pulse—that it produced the most intense paleness of the surface, and unusual coolness or coldness of the surface, accompanied with more or less moisture. In scarlet fever, we know there is unusual frequency in the pulsations of the heart and arteries; also, that there is great heat and dryness of the skin, and congestion, if not inflammation, of the capillary system. *Veratrum viride* eminently subdues, overcomes and removes every condition we find to exist in scarlet fever. In scarlet fever there is often more or less mucus in the fauces and trachea, and ulceration and inflammation often exists. *Veratrum viride*, by its emetic effect and acrid properties, will remove the mucus and change the action in the fauces. In scarlet fever, the heat, redness and dryness of the skin are extreme. *Veratrum viride* produces coolness, paleness and moisture of the surface; thus certainly relieving all these annoying conditions, and affording to the patient every opportunity of comfort and relief.

CASE I. Miss Q., aged 16 years, the daughter of Dr. Q., was attacked with typhus fever of the nervous type. We were called to see her on the night of the sixth day, at 8 P. M. The following were the symptoms:—Pain in the head slight; tongue covered with a soft, whitish fur in the centre, and inclining to yellow, the edges and tip were red; the mouth sticky and clammy, but not bitterish; the flush of the face not general, but confined to the cheeks, and more or less scarlet; considerable heat and dryness of the skin; the pulse was one hundred and thirty per minute, soft, and not full and strong; nausea slight; bowels rather torpid; considerable watchfulness and restlessness; there was no collection of sordes on the teeth; slight twitching of the tendons; pain in the back: had catarrhal af-

fection, which subsided when attacked with fever. Was, as we should have stated, attacked rather suddenly, without rigor. Was treated, previous to our seeing her, with calomel, Cook's pills, decoction of seneca; and seneca and quinine one day. We were informed that the fever had abated but slightly, a few beats of the pulse every morning, never below 120, except on the day the quinine was taken in conjunction with the decoction of seneca; and that it was materially lessened on that day. We ordered for the night the following, to be continued or omitted as might be necessary. In the first place a large camphorated powder to be given at 9 P. M., and quinine in five grain doses, to be administered every two hours, beginning at 10 o'clock. If the heat, dryness, pain, thirst, restlessness and frequency of the pulse, were not mitigated, or if increased, the quinine was to be omitted after the second portion. It was discontinued after the second dose. The Doctor stated that the symptoms were rendered more violent.

At 8 o'clock the next morning we found her as above, the pulse being 120 beats per minute. We put her on the tinct. of veratrum viride, to be given every three hours. Began with ten drops, the dose to be increased one drop every portion, till nausea or vomiting were excited. In sixteen or eighteen hours nausea and vomiting were excited; the skin became cool and moist, and the pulse was reduced down to 80 beats per minute. The veratrum viride to be continued every three hours in less quantity, to prevent a return of the fever. The young lady disliked it from the nausea it kept up, and, as it was called the *new way of doctoring*, it was omitted. The consequence was, in about eight hours, a return of the fever. We again ordered the article to be given every three hours, beginning with twelve drops, and to be increased two drops at each dose, till nausea or vomiting occurred. In six hours the heat and dryness were again subdued, and the pulse reduced down to 65 beats in the minute.

Notwithstanding the clear manifestations of the powers of the article in controlling the fever, it was again discontinued. In about twelve or fourteen hours the fever returned, and we were again called. The veratrum viride was again resumed, continued three days, and the fever was subdued in six hours, and did not return.

CASE II. A negro boy, about 10 years of age; was taken at the same house. He was treated, by his master, Dr. Q., with calomel, pink-root and quinine, without benefit. His attack and symptoms were very similar to those of the young lady. The symptoms not yielding, we were called on to prescribe for the boy at 8 o'clock, P. M., of the fourth day. Our prescription was tincture of *veratrum viride* every three hours, beginning with eight drops, and increasing the dose one drop each time, till nausea or vomiting ensued. On the administration of the third dose emesis was excited, the skin became cool and moist, and the pulse sank from 120 down to 70 in the course of nine hours from the first dose of the *viride*. By continuing it there was no return of the fever, and the boy rapidly convalesced. There being a great deal of sickness in the family, the boy was obliged to expose himself. The consequence was that he relapsed in ten or twelve days. The severity of the symptoms was more intense than at first. The case being one of relapse, with violent symptoms and threatened stupor or coma, we were called in on the night of the third day. We put him on the use of the *viride*. In less than twelve hours he was clear of all his untoward symptoms and of fever.

This case very strikingly illustrates the peculiar properties of the *viride*. The Doctor having, in each attack, treated the boy with other remedies, and without relief, up to the third and fourth day of his disease, and in each instance the *viride* having removed all the unpleasant symptoms in less than twelve hours. In the case of the young lady, another physician besides Dr. Q. had been in attendance before we were called; showing that at least one of the cases was considered to be severe.

CASE III. On the morning of the third day of the attack, we were called to see Mr. K. The fever had not abated up to the period we visited him. He was taken with a chill, followed by chilly sensations which lasted several hours. Pain in the head severe; flush of the face general, and approaching to crimson; the fur on the tongue more or less yellow, or approaching to a cream colour—the edges and tip rather clean and red; taste slightly bitterish; no collection of sordes on the teeth; slight nausea; little or no torpor of the bowels; pain in back and loins considerable; surface hot and dry; pulse

120 to 130, soft and not very full, yielding readily to pressure; great watchfulness and restlessness; thirst moderately intense. We put him immediately on the use of *veratrum viride* every three hours; the dose to be increased one drop every portion till nausea or vomiting followed. In less than twelve hours the heat and dryness of the skin were removed, and the pulse reduced to sixty-five or less. The *viride* was continued a day or two, and was then omitted without any return of fever. He had taken a portion of calomel the day after his attack. We gave him no more.

CASE IV. A negro girl, belonging to Dr. T., was attacked with a chill, followed by intense pain in the head; intolerance of light; fur on the tongue thin, whitish and slimy, edges and tip red; mouth sticky and clammy; no sordes on the teeth; more or less nausea and vomiting; no diarrhœa; skin hot and dry—being of light complexion, we could see that the flush on the face was not general, but circumscribed; pulse 130, small and soft; more or less spinal tenderness. She had no return of chills. We found her with the above symptoms on the afternoon of the third day. We gave her three powders, composed of five grains of colomel and five of camphorated powder, (with the exception of the first, which had ten grains of camphorated powder,) at 3, 6, and 9 o'clock, P. M. A blister was applied to the forehead. We had no *veratrum viride* with us, or we would have put her on its use. At 12 the next day the calomel had acted kindly; the blister had drawn well; the pain in the head, nausea and vomiting were much relieved; but the heat, the dryness of the skin and the frequent pulse, were not in the least reduced or mitigated. We put her on the use of *veratrum viride* every three hours, commencing with twelve drops, the dose to be increased two drops every portion till nausea or vomiting were excited. In less than ten hours the skin became cool and moist, the pain was removed, and the pulse reduced down to 65 or less. The nurse, contrary to orders, omitted the *viride*: on the next day there was a slight return of the fever. We ordered the *viride* to be given for two days; there was no return of fever, and the patient convalesced rapidly.

We could give many other cases, in which the effects of

veratrum viride were as striking and manifest as in the above ; but we think we have marshalled sufficient evidence to establish the point we had in view when we set out. If we have failed, the fault is with us, and not in the article recommended.

We will give a single case of convulsions, treated, as we believe, successfully with veratrum viride.

CASE V. A child of 5 years of age, the property of Dr. S., had been complaining of pain in the bowels and head for a day or two, but not so as to confine him to bed : had more or less fever. On the afternoon of the day of his attack he had an increase of fever, and complained much of his head and bowels. Between the hours of 5 and 6, P. M., he was seized with convulsions. We saw him at 8 o'clock. The convulsions were general, but one side was more convulsed than the other. The convulsions and spasms or rigidity did not subside until 2 o'clock in the morning. After the convulsions and rigid condition of the muscles had entirely subsided, he lay in a stupid and comatose state, from which he could not be aroused for two hours—the side most convulsed being left more or less paralytic during the intervals of convulsion. He now awoke, as out of a deep sleep, called for water, with an earnestness we never before witnessed, and continued to talk and plead for water, (which was given him, in moderation,) till he became convulsed again. The period of wakefulness lasted half an hour or more. Up to the period we shall shortly mention, there was at no time more than one hour of interval between the paroxysms of convulsions. The skin was hot and dry ; the pulse from 135 to 140 in the minute, without fullness nor hardness ; the face pale and ghastly ; pupils much dilated ; more or less frothing at the mouth. We were compelled to keep a piece of leather between the teeth to protect the tongue from injury. Warm bath was tried ; injections of salt and water were used ; the spine was irritated ; calomel, in ten grain doses, was given every six hours, and oil and turpentine midway between the portions of calomel. Also, China-root tea, several portions of Perry's vermifuge, and five drops of Croton oil. The above course was adopted because of the pain and fullness of his bowels, and his being frightened and grinding his teeth during sleep. We trusted much that as soon as the bowels

were operated on freely the convulsions and fever would subside ; but they continued.

We commenced the above treatment the night we were called, which was continued the next day and night before the bowels were moved. In addition to the purging, we tried tincture of stramonium and laudanum, without benefit. As all things were unfavorable and began to threaten seriously to involve the life of the child, and that nothing had so far controlled the frequency of the paroxysms, nor reduced the heat and dryness of the skin, nor the rapidity of the pulse, we determined to reduce the frequency of the heart's action. With this resolve we put the child on the use of viride. We gave at first three drops, in one hour repeated the dose, giving it every two hours afterwards and increasing one drop every portion. Before the third dose was given, the pulse began to decline in frequency, the paroxysms were less severe, and the intervals much longer. In eight hours the pulse was reduced to eighty beats per minute ; the skin cool and moist ; the pupils contracted, and no convulsions. The *veratrum viride* to be continued every three hours. If any symptoms threatened a return of the convulsions, an intermediate dose was to be administered.

If we had the case to treat again, we would order the viride at once, and would give ten drops every half hour till three doses were taken, and then every hour till emesis was excited or the symptoms were relieved.

Why would it not be the remedy in puerperal convulsions, when accompanied with frequent pulse, great heat and dryness of the skin ; alone or after blood-letting, where venesection was indicated ? We are so fully persuaded in our own mind of its adaptation to the treatment of puerperal convulsions, where the pulse is frequent and the skin hot and dry, that we are determined to try it, either with or without premising depletion, in the first case that comes under our care.

ARTICLE III.

History and Treatment of the Dengue Fever, prevailing in Augusta in the year 1850. By HENRY F. CAMPBELL, M. D., Demonstrator of Anatomy in the Medical College of Ga.

In the present article, we propose submitting a brief notice of the epidemic fever which has solately prevailed in our city, under the various names of "Dengue," "Break-bone" and "Neuralgic fever." As will be seen by reference to all the accounts of the disease, as it occurred during the past season in various localities, it was also here, preceded by a protracted period of unusual heat and drought. For many years past, the almost universal type of our summer and autumnal fevers has been intermittent or remittent; indeed this tendency is so predominant in our locality, that a continued fever is almost unheard of among us during the seasons above mentioned.

As early as the beginning of April, the unusual number of cases wherein the paroxysmal type was very obscure or did not at all obtain, was the subject of remark. In many, the fever would expend itself in one long paroxysm, without the well-marked characteristic stages so constantly observed by our fevers of ordinary seasons. On the 22d of August, my brother and associate, Dr. R. Campbell, saw a case in which he has since recognized most of the characters of the epidemic—though at the time it was viewed by him as a remittent of somewhat obscure type, similar to others that had occurred to us earlier in the season. This case was characterized by great prostration of strength, loss of appetite, furred tongue, depressed pulse, and dry but moderately hot skin. It lasted four or five days, was not at all amenable to treatment, and the convalescence rather tardy, attended with protracted muscular feebleness. Shortly after, other cases appeared in a part of the city quite distant from the above, somewhat better marked than this; but even these did not pass with us as confessedly cases of Dengue. About the 10th of September, the disease became fully manifest in all its unique and indubitable individuality. Then, what was dreaded in the vague uncertainty of rumor became more manifest in the tangible reality of painful, personal endurance.

The popular name of Break-bone Fever has pretty generally here, as elsewhere, been adopted in common parlance, to designate the disease. The term Neuralgic Fever has been suggested, along with various others; but inasmuch as the disease in all its important characteristics, so closely resembles the Dengue of 1828, in Charleston, described by Dr. S. H. Dickson, we have adopted this nomenclature as the one least liable to produce confusion, in a comparison of the reports of the epidemic as it has prevailed here and elsewhere.

Here, the disease was usually preceded by a period of discomfort, varying from twelve to thirty-six hours, in which the patient, without 'any defined sense of ill being,' was disinclined to engage in any exercise whatever, mental or physical. In its accession, he suffered first from a feeling of extreme fatigue—had a disposition to extend the limbs, and to stretch and gape—there was frequently severe pain in the loins, attended with general lassitude and debility. There was loss of appetite; the tongue slightly furred, but red and not very dry; some thirst, and occasionally, even in this stage, nausea would add to the general distress of the patient, though most frequently it characterized a later period of the disease. On examination, we would find the spine almost invariably tender, especially in the lumbar region, where most of the suffering was referred. The pain would also extend down the course of the nerves of the inferior extremities, even to the end of the toes. Frequently, the superior extremities would be affected in the same manner, while the intercostal nerves also would indicate most definitely their distribution in becoming channels of distress to the patient. But, above all, perhaps the most invariable attendant on the earlier stages of the complaint, was the severe pain in the frontal and temporal regions, which, in the majority of the cases, became par excellence, the torture of the disease. The eyes also became affected with a species of rheumatic tenderness, which rendered their movements quite painful. Sometimes, there was total inability to move the eyes; the skin was generally dry and hot; the pulse quick and full, varying from 80 to 120 per minute in the adult: the heat of surface did not always correspond with the frequency of the pulse. After a continuance of from two to eight days, the pain in the head and

loins would subside, the fever decline, and the patient begin a slow and unsatisfactory convalescence.

In the vast majority of cases, though not invariably, during some period of the attack, there appeared on the surface an eruption, which, in adults most frequently, according to our experience, would assume the forms of urticaria or erysipelas, while in children, scarlatina and rubeola were the diseases simulated. This eruption, of whatever form, was usually attended with much itching and burning over the surface, followed often by partial or general desquamation. We have found it most frequently in those cases that have suffered from gastric and intestinal derangement, and on the appearance of the eruption, there was much abatement in the violence of the symptoms.

Above, we have given the progress of perhaps the generality of cases, but by no means, the course *invariably* pursued by this truly protean epidemic. In many cases, the brain gave evidence of being much affected throughout the whole course of the disease. Sometimes, there was stupor so profound, that it was difficult to arouse the patient even for a moment, while in others, the most obstinate watchfulness obtained, amounting even to delirium. In these cases the pulse was full, hard and rapid, the skin hot and dry, and the headache and lumbar pains unusually violent. Another point of variation, is the mode of attack:—in some, instead of the gradual approach above described, the patient would be taken with a chill, more or less violent, attended by great nausea and vomiting; in others, the prostration was sudden, even on the first day rendering him unable to undergo muscular exertion.

We have already adverted to the nausea observed during the earlier stages of the attack. There were cases wherein this symptom was persistent throughout the whole period, accompanied by vomiting or violent retching. The patient would complain of tenderness in the epigastric region, with a sense of great oppression. There was much thirst, and with this urgent craving of cold fluids, the stomach would not retain them more than a few moments. The tongue in these cases was unusually red, with prominent papillæ; sometimes with a whitish fur in the centre, at others perfectly clean. Where nausea obtained, the eruption was tardy or altogether

absent, and on its appearance, the gastric distress usually abated. The pulse was frequent, but depressed, as observed during chill—the skin not generally very hot. The condition of the bowels has varied exceedingly: in the majority of cases, they were in a natural condition, especially where the attack was sudden, then, its access would be preceded by constipation of twenty-four to thirty-six hours, and again a copious diarrhœa would usher in the disease, or be superinduced by the mildest laxative. The tendency to diarrhœa, we found exceedingly embarrassing to our treatment, as it was frequently the case, where purgatives were apparently plainly indicated, a slight laxative would produce the most uncontrollable hypercatharsis. In one case, the bowels being constipated for days, the patient was urgent for a cathartic: being aware of the tendency to catharsis, we indulged him only with a saline enema: a purgation ensued which was extremely inconvenient, and which was checked only by large doses of opium, by the mouth and by enema. The same irritability, though perhaps in a less marked degree, was observable in many of the cases that came under our observation. We will refer here to another, in which this was even more remarkable than the above, where a most troublesome diarrhœa and prostration was brought on by a mild dose of rhubarb and magnesia. The diarrhœa here was so obstinate that all concern for the case was merged into the desideratum of arresting its prostrating draughts upon the patient. The passages were generally copious, and at the end of each evacuation the patient was tortured by violent tenesmus with pain in the rectum.

The condition of the liver varied also—though, from the color of the stools and the absence of bile in the matter vomited, it is probable that it was most frequently torpid, or at least not in the excited condition observable during many of the paroxysms of our autumnal fever. The urinary organs presented nothing remarkable; we can recall no case of hæmaturia complicating the disease.

Of the hæmorrhagic tendency of this epidemic, our observations were numerous. In a few cases, the nose was the seat of hæmorrhage; in two, the gums; in one, the mucous membrane of the lungs and also the gums; in many the bowels; and the

cases of vaginal and uterine hæmorrhage were perhaps the most frequent of all. The blood discharged from the gums was of a dirty, venous colour; it presented the appearance of blood undergoing incipient putrefaction. In the case where the blood was discharged from both the gums and lungs, the difference between the two fluids was remarkable—that from the gums being of the character above described, while the expectorated blood was of a bright red color, and in parts resembled very nearly the lateritious sputa of pneumonia. When the hæmorrhage came from the bowels, the passages were dysenteric and more or less mucus was mixed with blood, the proportion varying according to the predominance of the hæmorrhagic tendency. Frequently, the dysentery would be of the most obstinate character. One case we relate in illustration:—Mrs. L——, having passed through the stages of dengue, regularly, during her convalescence was attacked by a copious diarrhœa, which continued unrestrained under the most energetic astringent treatment. Soon after, dysenteric symptoms became manifest, and she discharged a considerable quantity of blood and mucus at each evacuation. Every astringent means we could suggest was put in requisition, with only temporary effect; passages, fluctuating between a diarrhœal and dysenteric character, continued to recur, with incredible frequency, for nearly three weeks, until the patient was worn to a mere skeleton. Although not on the treatment, we mention here that this case appeared to receive its first impulse towards recovery from the application of a small blister to the sacrum, suggested by our friend, Prof. L. D. Ford, who was in attendance with us.

The relation which this disease sustained to the menstrual function is, according to our observation, truly remarkable: it was invariably a provocative of the discharge. Should the disease exist at any time near the period of the catamenia, we have found it very apt to produce it; should it be before, but within several days, the discharge would anticipate its regular period, and also be unusually copious; should this period have passed, even by the lapse of several days, a recurrence of the discharge would be induced amounting, sometimes, to true hemorrhage. Menstruation occurring at this time was usually

painful, probably owing to the excitable condition of the general nervous system.

In the pregnant condition of the womb, the supervention of dengue was an occurrence attended with much danger to the well-being of gestation. The number of miscarriages was very remarkable, and in the latter months of pregnancy, premature labor was a very common result of the attack; and, indeed, we have been forcibly reminded of that passage, which we quote most reverently, "woe unto them that are with child in those days;" for of all attacked, those females who happened to be in this condition had most to dread from its advent. A species of pains resembling what is known, in obstetric language, as *false pains*, would precede the contractions, and seemed to be excitator of them, if not arrested. Miscarriage, superinduced by dengue, differed from this accident occurring from other causes, in the fact that, if treated early, its prevention was more easy in the former, and also the lumbar pain, first felt, was independent of the condition of the womb, and instead of being the *consequence*, soon became (or rather the irritation upon which it depends) the *cause*, of the miscarriage. The lumbar pain, in ordinary miscarriage, is a true *symptom* of this approaching catastrophe, indicating most frequently its *early* occurrence, whereas, in dengue, the pain in the loins must obtain for a considerable time, when it becomes a *cause* of the accident.

In connection with the hemorrhagic phenomena of the disease, we mention a case of Purpura, kindly reported to us by Prof. Garvin, the former Editor of this Journal. The subject of the disease was a child, and it did not extend further than the lower extremities. He considers its situation influenced by the dependent position of these limbs, and as that diathesis prevailed during the attack, gravity determined it to the extremities. Professors Dugas and J. A. Eve permit us to refer to cases occurring to each, wherein the patient vomited matter analogous to black vomit, closely resembling coffee grounds. In the two cases of Dr. Dugas there were also copious dejections of a similar character.

Convulsions, in children, have been very common in this disease; and when we consider the truly nervous character of the complaint, we cannot be surprised at this complication. In

paroxysmal fever, we know that convulsions are of frequent occurrence, and here, we have a disease which, in the condition of the nervous system, is similar, as we believe, to its condition under a prolonged paroxysm, and in the susceptible innervation of the child, we find ample predisposition to the convulsions. In some cases, the convulsions were of a most obstinate character, continuing for many hours, and returning with remarkable tenacity. One marked feature of our epidemic we have, until now, omitted to notice; yet, from its frequency, it may be considered among its prominent characteristics. We refer to the relapse so generally succeeding the period of apparent convalescence. On the fourth or fifth day from the access of the disease, the patient, having passed through the attack as above described, will experience a respite, varying from one to two or three days, and then, without apparent cause, the headache and pain in the back returns; nausea and vomiting, even if not existant before, will now almost certainly ensue, and the eruption also, at this period, will either be seen for the first time, or reappear—the febrile symptoms, however, do not usually run so high as during the earlier stage of the complaint. The period of relapse has a duration of from two to four days, when, with this last sally, the monster looses his grasp upon the devoted patient, leaving him ill-natured, depressed and wrecked. The appetite is gone, and in its stead, a most villanous and persistent *taste* is left to sophisticate the delicacies which are the natural and rightful concomitants of an ordinary convalescence. We have noticed in the convalescence of several cases, various annoyances, such as falling of the hair, eruption of boils, and an obstinate wakefulness, which, in one iustance, terminated in temporary insanity, rendering imprisonment necessary for the welfare of the patient.

From various calculations we have seen, we think, the number of cases in Augusta during the past season may be estimated at between eight and nine thousand—the majority of which did not receive the attentions of a physician, being subjected to domestic treatment. Out of this vast number, we have not heard of a single death from the epidemic, uncomplicated. Indeed the prognosis of the disease, under ordinary circumstances, was invariably favorable.

A comparison of this disease with the epidemic of 1828 prevailing in Charleston, and so ably described by Professor Dickson, with whose name the very term *Dengue* will be ever inseparably connected, allows the following conclusions, viz: that while the epidemic of '28, was an *arthritic* disease, affecting the joints (especially the smaller joints) in a manner similar to *articular* rheumatism, the present is neuralgic, and bears the same relation to *neuralgic* rheumatism, that the other did to the *articular* form of the disease.

In the former epidemic, the sequelæ were those of articular rheumatism, whereas, in the present, we have heard of but a single case, out of the multitude, wherein anything of the kind obtained. This isolated case was in the practice of Professor Ford.

Above, we have given a brief history of the more prominent peculiarities of this epidemic as they have appeared to us, on a review of our observations of them during the past season. For the last month, the disease has disappeared, with the exception of an occasional case. Scarlatina, which had preceded it, and seemed extinguished, or "disguised in its livery," during its prevalence, is again making its ravages among us. From several cases of this disease, we think we discover a greater tendency to convulsions than usual, whether determined by the previous prevalence of dengue, or not, we are unable to say.*

From the self-limited nature of the disease, the treatment, of course, could not, in accordance with the true principles of therapeutics, have been other than palliative. Any effort to cure or shorten an attack, would, in our opinion, have been as vain as to have attempted to modify the natural and fixed phases of rubeola or scarlatina. Yet, we consider that dengue had its appropriate and necessary treatment, for although "no one could die of the break-bone," still, from the serious amount of suffering to which its victims were subjected, palliation became a very desirable object.

In the uncomplicated form of the disease, we have generally preceded other remedies by the administration of a mild cathartic: oil, or the sulphate of magnesia, has been used by us quite

*Since the above was written, as many as four cases of Scarlatina, attended by convulsions, have come to our knowledge.

often. When there was hepatic derangement, or a tendency to diarrhœa, we have preferred blue mass to any other cathartic, and among children, particularly, we considered it preferable to all others. Emetics we have rarely used, unless the case was complicated by sore throat indicating their administration, for the nausea, which was a frequent attendant on the disease, seemed to forbid their use. After the evacuation of the bowels, we could now attend to the most important indication for treatment, to relieve the severe *pain* with which the patient is tortured; for this purpose, our chief reliance has been upon *Opium*, in the various forms of laudanum, Dover's powder, morphine, &c., &c. From the tendency to nausea, we have avoided, as much as possible, the administration of medicines by the mouth, and we found that by enema, opiates exerted a better influence than by the former mode; indeed, after the evacuation of the bowels, the laudanum injection became our constant and only remedy, and we have found it, above all others, the most efficient.

Now, when we consider the location of the greatest amount of suffering, viz., the lumbar region, and the rapidity of absorption by the rectum, the reason of our selection will be apparent. Indeed, by this mode of administration, it has the double advantage of a general and local remedy. The frequent tendency to diarrhœa and dysentery, so common as a complication, was also another reason for this mode of medication. In those cases where abortion was impending, this, with the application of sinapisms to the lumbar region, would invariably arrest the pains, and by repetition, the accident was prevented.

Rubefacients, also, were of the greatest benefit; mustard poultices to the spine, together with mustard pediluvia, afforded invariable, if but temporary, relief. But in the severer cases, where the intensity of the frontal and temporal neuralgia was too urgent to wait the effect of the opiate treatment, we found the inhalation of *chloroform*, as well as its local application, to afford immediate and entire relief. Of diaphoretics, we can speak in the highest terms: nitre, the general bath, and indeed every other sudorific means, we can recommend with the greatest confidence; and if there is a disease wherein, from an *à priori* view, we would suggest the Hydropathic "Packing"

with wet sheet and blankets, this is the one wherein its use seems most applicable. The neuralgic pain, the hot skin, the entire arrest of cutaneous transpiration, together with the general nervous irritability characterizing its accession, all mark conditions which the swathing and relaxing vaporizations of these therapeutic ultraists are best calculated to ameliorate. We have not made use of this remedial means in any case, but from the general aspect of our epidemic, especially in its earlier stages, we have been strongly tempted to test its efficacy—*“Je prends le bien où je le trouve.”*

Now, with regard to the administration of quinine, as a *general* remedy, we cannot speak with any great degree of confidence. In a few cases, where the fever appeared to assume the remittent type, we have seen marked benefit accruing from quinine and laudanum enemata, applied during the remissions, and in the latter stages of the disease, where the convalescence was unusually protracted, this proved a valuable remedy. The complications of the attack were generally of themselves sufficiently suggestive of their own appropriate treatment, not to require here any amplification. The diarrhœa and the various hemorrhages required nothing peculiar in their treatment; but for the nausea and prostration of the stomach, so distressing during convalescence, we found small and repeated doses of *brandy* the most efficient, indeed, the only remedy. This very disagreeable attendant of the convalescence, seemed to depend entirely upon the enfeebled condition of the stomach, and required stimulants alone for its relief. After the failure of various remedies—opiates, camphor, antacids, with every sedative imaginable, small quantities of brandy relieved this distress with remarkable promptness; and the amount of this stimulant the most delicate females were enabled to bear under these circumstances, without cerebral disturbance, was truly surprising.

Ale and porter were much used in the convalescence, and generally with good effect. Our own experience, however, does not corroborate their advantage, as beverages, to the extent which their popularity would seem to indicate. In a few cases, we have observed diarrhœa of the most violent nature, result from their use, and also much gastric and intestinal distress.

After the long-continued nausea has in a measure subsided, the stomach remains extremely susceptible to irritative impressions—the ingestion of almost any food, causing a return of the distress; therefore it became necessary to select such diet as would be least apt to oppress this greatly enfeebled organ. For this purpose, we have recommended, with advantage, fresh cream and lime-water, one third of the mixture being lime-water. This, being light, nourishing and demulcent, was retained a sufficient length of time for digestion, which healthful exercise repaired the energies of the stomach for more solid ingesta. In many cases, from a second attack of the disease, the convalescence would be unusually protracted. In this condition tonics became appropriate. We have used the bitter vegetable tonics exclusively—of these, gentian, cinchona, and colombo, have been our selection.

In the foregoing very imperfect sketch, we have forborne to indulge in speculations about the *cause* of this singular disease, a question which, judging from the sphynx-like pertinacity with which the riddle has been plied, is infinitely more important to some than their recovery from its inflictions. To the more intelligent of the profession, it will be not more necessary to assign a cause for this, than other epidemics; but to those ardent searchers after the invisible and inappreciable, whom we meet in our daily rounds, to whom an answer must be given, or a lengthy explanation *why we cannot answer*, to such, our invariable reply, delivered with as much profundity of manner as we could assume, has been “It depends upon some modification in the condition of the atmosphere, wherein the elimination of morbid particles has been produced, and the epidemic is the result of this elimination.” The mystery is cleared, the whole becomes lucid as sunshine, it is *atmospheric*, that is, in the air; every one can understand this—the explanation is therefore extremely satisfactory. Until our means and appliances for testing the accidental and occasional constituents of the atmosphere have become more perfect than at present, until years of meteorological and pathological observation have passed, leaving an immense accumulation of simple, isolated, but coincident *facts* on record; until these facts are compared and laboriously studied, with the strict scrutiny of inductive

discipline, and the results accruing clearly interpreted, all of which, we fear, must be in other times and by another race of philosophers than the present,—until then, we say, the answer must still be superficial, to all who make so superficial an interrogation.

PART II.

Eclectic Department.

A Synopsis of the Spinal System. Being the Croonian Lectures, delivered at the Royal College of Physicians, London. By MARSHALL HALL, M. D., F. R. S., &c., &c.

Gentlemen,—I regard it as a great privilege to be deputed to give these lectures. It was within the walls of this college that the great Harvey propounded his incomparable discovery of the circulation of the blood. It is here that we are exhorted, annually, to imitate his bright example in the search after physiological truth; and it is here that each of us may, in his turn, enjoy the opportunity of laying before the profession the results of his labours.

It is to Dr. Croone, one of the original members and founders of the Royal Society, and a fellow of this college, that we are indebted for the lectureship, the duties of which I shall endeavour to fulfil on this and two subsequent days. I can imagine no greater benefactor to our profession, after the labourer himself, than the person who, by such an endowment, encourages the labours of others. Honoured, therefore, be the memory and name of Croone.

In the Royal Society the same munificent individual instituted a lecture, to be given annually, especially appointing its subject, that of *Muscular Motion*. I can suppose, therefore, that he would have felt an especial interest in that of my own labours, which I propose to bring before you on this occasion.

The investigations to which I advert were begun twenty years ago. They have been continued, without interruption, to the present time, and are still alas! how incomplete. Such as they are, however, they have occupied me during 25,000 hours! Nay, if I may add the hours devoted to the same subject *in practice*, as connected with disease, I think I may double this number.

Of this persevering industry I may surely be allowed to boast. It is by such, only, that new and great truths in medi-

cine are detected and elaborated and error avoided. I may too, I believe, in the very spirit of the founder of these lectures, make a boast of another kind,—I have never, during my long career, endeavoured to depreciate the efforts of any one of my professional brethren. I am grieved to say that this cannot be said of all of us, for the race of the Primeroses and of the Parisanuse is by no means extinct.

But I proceed to my subject.

In 1831, I read a paper, on the circulation of the blood, as observed principally in the batracia, before the Royal Society. Besides the different *schemes* of the minute arteries and minute veins seen in the several seats of the *systemic* and *pneumonic* circulation, that paper contained the discovery and the first enunciation of a distinct series of vessels or blood-channels occupying the spaces intermediate between the last branches of the arteries and the first roots of the veins, channels which I then designated the true “capillaries.” This designation has been justly criticized by Dr. Voigt, the pupil of the celebrated Professor Berres, of Vienna, the anatomist to whom we are indebted for the greater part of our knowledge of the different forms of these vessels, which he proposes to call, from their position between the arteries and the veins, the “*systema vasorum intermedium*.”

From the fact, that it is in these vessels or channels that *all* the *changes* in the blood are effected, whether in the systemic or the pneumonic circulation, I have more recently thought that the designation *methæmatous* blood-channels, or more briefly, *methæmata*, would be the most appropriate. The veins, heart, and arteries are mere *machinery* for conveying the blood to and from these methæmata, in which in reality, *all* the *purposes* and *objects* of the circulation are effected. This distinction between the minutest arterial branch and venous root, and the intermediate methæmatous blood-channels, is absolute in every point of view—form, structure, function, &c. In this last respect, the former are mechanical merely; the latter, chemical.

Perhaps the most beautiful fact in the minute circulation is the arrangement of the minute arteries and the minute veins, so obviously adapted to accomplish the precise *object* of the circulation in each individual part or organ. Observe this diagram, and compare with it the following one:—They display the arrangement of artery and vein in the web of the frog—an arrangement obviously intended to admit of the distribution of the intermediate or methæmatous vessels, so as to confer nutriment on the tissues. Compare the representation of the arteries and veins in the mesentery: They are

comparatively large. They obviously proceed farther than they are seen. The seat of their real function is *beyond*. The mesentery is, in fact, *little* more than a framework to convey them to the *intestine*, and there the methæmata are evolved, with their real and appropriate function.

But now, gentlemen, examine the third diagram, displaying the *pneumonic* scheme of arteries and veins, the obvious object of which is to give origin to innumerable methæmata which actually *spread over* the intermediate spaces or area, so as to expose the blood-globules to the action of the atmosphere through the fine (and almost arachnoid) tissue of the lung. It is the lung of the *Triton*. Observe this artery running parallel to this vein. The intermediate space is one field of methæmatous vessels. Here are *branches* of arteries. With each of these arterial branches a venous root runs parallel. In *this* spot there are *no* arterial branches. The same fact is observed in regard to the vein; there are no venous roots. There is, throughout, *perfect parallelism*. How obviously the result of design!

But in this fourth diagram, you see the pneumonic methæmatous channels themselves, as you may, when this lecture is concluded, see, through this microscope, the gold-like flood of blood-globules to which they are destined to give course.

But to return from this digression. It was during the preparation of that paper that I was struck with the occurrence of the phenomenon to which I have adverted, the first of the series which I have since designated "reflex actions."

My friend Mr. Henry Smith and myself had, on a memorable day, observed and traced the pneumonic circulation in the *Triton*. We then removed the head, and divided the senseless body of the animal into three portions, anterior extremities, posterior extremities and tail:

On irritating the separated tail with the point of a probe or the forceps, it was observed to move and become contorted into varied forms!

On irritating the anterior or posterior extremity in the same manner, these also moved, being withdrawn and made to approach the adjacent portion of the trunk of the animal.

Other similar phenomena were speedily observed; and I was gradually led on to the development of the spinal or diastaltic system.

Having thus stated the short *history* of my first observation, I proceed with my subject; yet once more, before I do so, I must take this early opportunity of explaining several new terms, which I have thought it necessary to introduce, in order

that my views may be fully and unequivocally expressed, and all but the most wilful misrepresentation prevented.

On analyzing the facts which have been detailed, I observed that the following anatomical relations are essential :

1. A nerve leading *from* the point or part irritated, *to* and *into* the spinal marrow ;
2. The spinal marrow *itself*; and
3. A nerve, or nerves, passing *out of* or *from* the spinal marrow,—*all in essential relation or connection with each other.*

On these anatomical facts I have ventured to institute a new nomenclature, descriptive of what I have hitherto designated the *Spinal System*, and expressive of these essential points. The term peristaltic (from *περι* and *διδλω*, *to contract*,) is familiar to us all. It may be justly extended to all the movements of the inferior organs, as the heart, the stomach, the large and small intestines, the uterus, &c. These movements, it is well known, are independent of the spinal marrow. But it has been shown that a series of experimental phenomena, and it will be shown hereafter that a series of important functions, are effected by means of the series of nerves in essential connexion with the spinal marrow, to which I have adverted. The action or act is performed *through* the spinal marrow as its essential centre. I propose to designate the phenomena by the term *dia*-staltic.

The spinal system may henceforth be designated—*The Diastaltic Nervous System*, a designation which will have the advantage of including this system in the invertebrate as well as the vertebrate tribes of animals. This system embraces a peculiar anatomy, physiology, pathology, and therapeutics.

Perhaps the only *purely* diastaltic function is *Respiration*; and this is variously modified by volition and influenced by emotion. But there are many other functions which partake of *both* the diastaltic and peristaltic character. Such are the functions of the immediate conduits of ingestion and of egestion;—the œsophagus, the rectum, the uterus. These functions are *dia*- and *peri*-staltic.

How much there is in these terms calculated to excite new and accurate inquiry! How much to refute injurious and calumnious criticism! I have hitherto spoken of the *mode* and *course* of the diastaltic actions and functions. But I shall immediately proceed to show that the *Principle* of action in the diastaltic nervous system is as special and peculiar as its direction. This principle I long ago demonstrated to be the *vis nervosa* of Haller, the “*excitabilité*” of M. Flourens, acting in newly discovered, diastaltic forms. Now, the term “*reflex*” may have been *vaguely* used by Prochaska; but the *full* and

distinct idea of a diastaltic action of the *vis nervosa* had occurred to no one.

We are much in need of other terms still, to aid us in this investigation. The terms incident excitator and reflex motor have been used to designate those nerves whose influence proceeds *to* and *from* the spinal marrow. But they have never appeared to me satisfactory, and I have long wished for others more expressive and explicit. The following compounds of ὁδός, *a way*, have appeared to competent judges very appropriate to our subject; esodic (ἐσώ) will express the action *into*; exodic (ἐξώ,) the action *out of*; anodic (ἀνώ,) will express the *ascending*, cathodic (κατά) the *descending*, course of action; pollodic (πολύς) and panthodic (πας) will express the facts, on which I shall shortly have to dwell at considerable length, of the action of the *vis nervosa* from each *one* point of the diastaltic system, in *many* or even *all* directions, to *every* other.

Many important facts, many important truths, will be conveyed to the mind at once by these simple terms. Their root is already familiar to us in the word periodic; and several derivations from it have already been employed, in the science of his creation, by the illustrious Faraday. It must be distinctly understood that they are all to be restricted to the anatomy, physiology, pathology, and therapeutics, of the diastaltic nervous system, in which they express the modes of action of the *vis nervosa*, distinctly from and exclusive of all other influences whatever, as sensation, volition, emotion, &c.

According to the Roman critic—

“Si forte necesse est
Indicus monstrare recentibus abdita rerum,
Fingere cinctutis non exaudita Cethegis
Continget; dabiturque licentia sumpta pudenter.”

I here present you with the interesting object to which I have alluded. On this plate I have placed a Triton, divided so as to display the head, the anterior and the posterior extremities, and the tail, separately. If you touch the eye with the point of this needle, the eyelids close; if you touch the anterior of the posterior extremity in the same manner, it is retracted; if you touch the extreme part of the tail, it coils up immediately and energetically.

The last phenomenon was, as I have stated, the phenomenon which I first observed. I did not touch nerve or muscle; the phenomenon could not then be one of the *vis nervosa* or *vis muscularis*. What, then, was its nature?—what its motor principle?—what its application in physiology? It was impossible for the inquiring mind not to be led into these and a multitude of similar questions.

If, in the severed head of the frog, the toad, the eel, the snake,—the kitten, the puppy, the young rabbit, &c., we touch the eyelid, the eyelash, or the conjunctiva, the eye is immediately closed. The same event occurs in the horse stunned to insensibility by the blow of the pole-axe.

If, in the decapitated trunk of the animals, we irritate a toe or other part of the foot of the anterior or posterior extremity, this extremity is immediately withdrawn; if we irritate the tail, or the integuments near the sphincter ani, still greater movements are produced.

If the brain merely be removed, in a very young animal, all these phenomena are still observed.

The same effects are produced by irritating the dura mater within the cranium, and other interior membranes and tissues—a fact which throws a beam of light on some pathological events.

By any of these irritations, an act of inspiration, if respiration were previously suspended, is especially apt to be induced.

Each irritation of a cutaneous or mucous surface appears to induce a peculiar, *special*, and definite movement. If in the very young kitten, deprived of cerebrum and cerebellum, the foot be irritated, it is retracted; if a finger be introduced between the lips, an act of suction is excited; if a soft substance, as milk, be inserted into the pharynx, an act of deglutition is attempted; if the border of the rectum be irritated, the sphincter is contracted. The eyelash, the meatus of the ear, and the tufts of hair between the toes, are peculiarly excitor.

Similar phenomena are observed in the anencephalous fœtus in the early stage of asphyxia in young animals, and in the anæsthesia induced by chloroform.

In the case of perfect paraplegia in the human subject, when sensibility is absolutely extinct, and voluntary movements totally abolished, diastaltic actions are excited on the application of appropriate irritants, such as tickling, a puncture, a pinch, or sudden heat or cold; all of which the patient is unconscious.

By continuing my experiments, I soon discovered that an excitant and the presence of the spinal marrow were essential to this phenomenon, and that from the point of the cutaneous surface a continuity of nerve *to* the spinal marrow and *from* this to the muscles of the part moved, were also requisite.

I soon found that the facts themselves were known to physiologists; that Whytt had, in one paragraph, shown the necessity for the presence of the spinal marrow; and that Sir Gilbert Blane had stated that the phenomenon was not one of the sensibility. But in this defective condition our knowledge on the subject remained. It was not known that the phenome-

na in question were *limited* to the spinal marrow *exclusively* of the cerebrum and cerebellum; that they were *always excited* and *never spontaneous*; that they were phenomena of the *vis nervosa* acting according to a *law then unknown*; that though then only observed in experiments, they had an extensive place in physiology and pathology. All these relations, and many others, it has been my object and office to trace.

Perfectly similar experiments had, I have said, been made long ago, by Redi, Whytt, Legallois, Blane, &c. This was fully stated by me in my first publication on the subject, (see *Memoir I.* § 107, 129, &c.) Whytt had even asserted the necessity for the presence of the spinal marrow; and Blane had stated that sensation is excluded. What then? Did those observations lead to any result?—to any detection of the motor principle involved? to any application to Anatomy,—Physiology,—Diagnosis,—Pathology,—Therapeutics,—or Obstetrics? No! Not one tittle of all this was accomplished by any one:

In making this assertion, I include Unzer and Prochaska, of whom so much has been calumniously written in this country:

Whytt speaks of all the phenomena in question as dependent on the *soul*.

Unzer asserts that the spinal marrow is merely “*a thick cord of nerves*,”—(a view which *excludes* the *idea* even of a really reflex or diastaltic action absolutely)—and may be fairly supposed to speak through his pupil, Prochaska. Now the latter author describes diastaltic actions as ‘*impressionum sensoriarum* in motorias reflectio; and, so far from even entertaining the idea of a *dia-staltic function* or *System*, confounds the effects of volition and emotion, and the action of the heart, stomach, and intestines, with actions really and truly diastaltic.

I will only add, in concluding these brief remarks, the expression of my regret, that any of my professional brethren should be found, of minds so ignoble as to persist in the injustice which has been so long, although vainly, attempted in this matter—continually immolating, during the whole course of nearly twenty years, *truth* on the altar of misrepresentation and detraction.*

Allow me now to call your attention to this experiment. In *this* object I present you with the lower extremity of the frog, attached to the lumbar nerve, but otherwise detached from the rest of the tissues. If I irritate the nerve, the toes, the foot, and the whole limb are moved.

In this second object, the nerve is still in connexion with a portion of the spinal marrow, the rest of the tissues being re-

* See Dr. Forbes's Review; Dr. Todd's Cyclopædia, &c.

moved. The same, but more energetic movements are produced, by irritating the spinal marrow.

The nervous influence by which these phenomena are produced was designated, by Haller and his followers, the *Vis Nervosa*. This *Vis Nervosa*, this power in the spinal marrow and muscular nerves, by means of which, if their tissues be irritated, muscular contraction follows, was supposed by Haller, by Bichat, by Professor J. Müller, and I believe, by all physiologists, to act in *one* direction only—from above downwards. Its action was supposed to be *cata-staltic* only.

As long as this view prevailed, this motor power had, and could have, no application to physiology. It was presented to us as a mere experimental fact, or, at the utmost, in its relation to pathology.

It was, like the facts noticed by Redi, Whytt, &c., sterile, and without utility. Now the existence of a distinct and energetic motor power in the animal frame, without utility in the animal economy, would be a perfect incongruity, and contrary to everything observed in creation.

By a series of oft-repeated experiments, I have demonstrated other *Laws* of action of the *vis nervosa*, and especially *one* which may be designated *dia-staltic*.

By this discovery I have been enabled to prove the identity of the motor power in the experiments of Haller, and in the experiments of Redi and of Whytt, and, disentangling the maze, to show that that double series of experiments is *not* without its application in physiology; that the latter have, in fact, their prototypes in all the acts of ingestion and of egestion in the animal economy, and in some instances of pathological and therapeutic actions; and the former, in certain cases of pathology. It is the first real step in the philosophy of involuntary motions.

Observe this preparation of the frog. In addition to the portion of the spinal marrow with the denuded lumbar nerve, presented to you in the last experiment, I have, in this, left the lumbar nerve, of the other separated limb. If I pass a galvanic current of some power along this portion of nerve, the limb to which it is attached *through* the spinal marrow and the other lumbar nerve, but to which it does not itself *belong*, is moved as before, but with less energy. The nervous force called into action—the *vis nervosa*—is the same. The phenomenon is *dia-staltic*, the others being *cata-staltic*.

But if the limb be not removed, and a toe be irritated, the same *dia-staltic* phenomenon is observed.

If the lumbar nerves be denuded, but left in connexion, through the spinal marrow, with the *anterior* extremities, and

subject to an energetic galvanic current, even these limbs are moved. The action is not only *dia*-staltic, but *ana*-staltic.

If the spinal marrow, in the decapitated tortoise or turtle, be denuded and irritated by the point of a needle, or the galvanic current, *both* anterior and posterior extremities are moved. The *same* irritation, applied to one and the *same* point of this nervous centre, produces *all* these movements.

If, instead of irritating a point of the spinal marrow itself, we denude and irritate a lateral spinal nerve,—the same results, the same movements of *both* pairs of extremities are observed.

In the first experiment, it is the *vis nervosa* of Haller which acts on the *posterior* extremities. This is the general view. Are the similar and synchronous movement of the *anterior* extremities, and the similar movements of *both* anterior and posterior extremities in the second experiment, of *different* origin? But if the integument be irritated, the *same* movements still take place; and this is one of the cases of reflex or diastaltic action.

Lastly, if we so irritate the border of the eyelid, the eyelids close; or if we touch the border of the larynx, or of the sphinctor ani, these orifices are closed. But these are *Functions*.

Such I believe to be the *Demonstration* of the diastaltic action, and such the application to physiology, of the *vis nervosa* of Haller.* Previously a sterile experimental fact, this principle of action has now taken its place as the dynamic force presiding over the large *class* of the *functions* of ingestion and of egestion in the animal economy.

It appears to me that the anastaltic and the diastaltic actions, in these experiments, are slower and more combined than the merely catastaltic. There is a similar difference between physiological acts, which are all diastaltic, and those pathological movements which are catastaltic.

Nor is the action of the *vis nervosa*, under these circumstances, merely diastaltic. It frequently occurs that, instead of such an event, there are diastaltic actions in *many* or *all* directions.

Exp.—If a frog be decapitated, and a toe of one anterior extremity be irritated with the forceps, this and all the other limbs are moved. There are, therefore, a reflex action, and actions in the various directions across the spinal marrow, and downwards along this nervous centre, both directly and obliquely, to both inferior extremities.

Exp.—If a toe of an inferior extremity be irritated, the phenomena are still the same: reflex, across, and upwards, both directly and upwards, both directly and obliquely, to both anterior extremities.

* See Memoir II., § 15; New Memoir, § 95.

In the first experiment, the action of the motor power is esodic in the nerves and variously reflex, diastaltic, and *catastaltic* in the spinal marrow; in the second, it is *ana-staltic*. And, so far, the action might be designated as pollodic. But I use the term panthodic in a very emphatic sense: I believe that no spot of the diastaltic system can be excited without telling upon *every* other. Inspiration, partially suspended, is excited by irritation on *any* part of the external surface. The application of cold water on the face not only excites inspiration, but deglutition, micturition, contraction of the uterus, &c. The entire system is in a state of static tension, *each* part being in strict relation with *every* other; this becomes dynamic force on the application of a stimulus.

Sometimes there are *combined* and *concatenated* actions; and wherever the excitation may be applied, there may be—a leap or a jump.

It must be observed that in all these cases the *nerves* are esodic and exodic; but of what takes place *within* the structure of the spinal marrow, on which might well be inscribed the word—*mystery*, we are still utterly ignorant; we only know that the result of irritation in such a case is not only variously diastaltic, but panthodic.

The diagram to which I now call your attention displays, at one view, the progress of the *Demonstration* of the identity of the motor principle in the experiments of Haller, and in those of Redi, Whytt, &c., whilst it illustrates both.

This nerve, or the spinal marrow at this point, being irritated, the limb is moved: this is the experiment of Haller. The toe being irritated at this point, both limbs are moved: this is the experiment of Redi and Whytt. Finally, this point of the nerve being irritated, both limbs are again moved, but the one to the right is moved by downward influence, as in the experiment of Haller, whilst the one to the left is moved by reflex influence, as in the experiments of Redi and Whytt. The *same* excitation has acted downwards simply, *and* diastaltically, both limbs being moved.

I now lay before you another experiment. In the frog here exhibited we have removed the cerebrum and cerebellum, and *all* the viscera. The centre of the cerebral system, and the ganglionic system—and especially that intra-visceral-ganglionic system which is the splendid discovery of Dr. Robert Lee, the most important, in *Anatomy*, of the present century,—are all extirpated. Yet you will observe that an interesting series of muscular movements are still excitable. If you irritate a toe, the limb is retracted and the other limbs are moved; if you irritate the skin near the sphincter ani, various movements of

the posterior extremities especially, and of the animal generally, are still performed. Sometimes, you will observe the combined movements constituting a leap, or a jump even!

This experiment I consider as the *analysis* of the nervous system into the cerebral, the ganglionic, and the spinal. The very centre of the first is removed; the second is removed with the viscera; the third remains, with its remarkable phenomena. I have in this experiment separated and isolated the spinal marrow *for the first time*. It was previously, viewed in its singleness, undetected and unknown.

And what an extraordinary object is this mere nervous and muscular shell of an animal! The skeleton of bones is familiar to us. This is a skeleton of nerve and muscle as it were, all their excito-motor power, and all their motor, remaining in its vigour! I know of nothing so extraordinary in physiology.

The cerebrum is removed, the whole ganglionic system is removed, but the diastaltic nervous system remains, with its diastaltic nervous and motor power, all its diastaltic nervous arcs! The diastaltic system is singled out, and separated absolutely from the other divisions of the general nervous system. Everything which relates to the diastaltic system remains in force; all that is cerebral—sensation, volition, spontaneous movement, emotion, pain—all that relates to the ganglionic system—all *peri-staltic* action, is removed. This experiment alone is sufficient to establish the whole *Theory of Diastaltic Nervous Action* once and for ever!

I beg now, gentlemen, to call your attention to a further experiment. In the one just detailed and placed before you, I made an *analysis* of the nervous system generally; I now propose to make a similar *analysis* of the spinal system specially.

On this plate you will find a frog which was first decapitated and then divided between the anterior and posterior extremities.

It is necessary, in doing this, not to make the division too near the cranium in the first instance, or too near the anterior extremities in the second.

If you touch the eye, the eyelids close, and the eyeball is retracted. If you touch or puncture a toe of either extremity, the limb is moved.

There are *three* modes of annihilating such movement.

The first mode is to remove from the foot or toe the cutaneous tissues. With these the *origins* of the incident or esodic nerves are removed; the first part of the reflex arc is wanting.

The second mode is to divide the brachial or the femoral or lumbar nerve; the course of the arc is interrupted, and the arc itself is again incomplete.

The third is to destroy the portion of the spinal marrow in connection with this nerve itself. The very key-stone of the reflex arc is absent.

Can any thing be more deeply interesting, or indeed wonderful, than this perfect analysis of the spinal system, and of each *arc* of that system, consisting of the fibrillous organ, the progress in an esodic nerve, the spinal centre, an exodic nerve, and its distribution in the muscles, all which is required, in order that the reflex or diastaltic action may be uninterrupted and complete!

Many as have been the years during which I have witnessed phenomena of this kind, I never see them again without a new and peculiar interest, nor do I think there is any fact in anatomy or physiology more extraordinary. It is the complement, as it were, of that which has preceded it, and of the *analysis* of the nervous system.

But I have a fact—an experiment—of a still more remarkable kind, to bring before you.

Examine this prepared frog: the head and the anterior extremities have been removed; the tissues behind the lumbar nerves have been removed in their turn, and the pelvis has been divided, and the posterior extremities separated.

Now observe what takes place when I irritate the foot: the limb is immediately flexed and retracted; the other limb is also moved!

The influence—the nervous motor influence—must have ascended this lumbar nerve, have proceeded to the spinal centre, have been reflected along the same nerve in the contrary direction! The action is diastaltic, and esodic and exodic along the same compound esodic and exodic nerve.

Nor is this all. To and from the same spinal centre, a part or portion of the same motor influence has been sent along this other nerve to this other extremity.

What do you think of these *diastaltic nervous arcs*, of which the idea did not previously exist in the science of anatomy and physiology? Do you imagine that they merely exist in these *limbs*?—or are merely displayed in these experiments? So far is this surmise from being true, that they exist, and are, as it were, diffused, through a whole *Class* of the functions of life. I have shown you—I have laid bare before you—a *diastaltic nervous arc* in the limbs of the frog; but such arcs, hitherto undetected, exist in the animal and in the human frame, and are the anatomical and physiological agents in *all the functions of ingestion, of retention, of egestion, and of exclusion*; each and all of these functions are performed through the channel and agency of a *diastaltic nervous arc*! not as conducting sensa-

tion or volition, but an excito-motor power, so totally distinct from either of them, that whilst they are *physical* in their nature, this is *psychical*: and whilst one of them is frequently spontaneous in its action, this is *always* excited.

The facts which I have to lay before you are absolutely overwhelming from their number, and they are not less momentous than numerous. Examine this diagram. It represents the superior and inferior laryngeal branches of the pneumogastric nerve. In the function of deglutition, during which, as you know, the larynx is closed, the origins of the first of these are *excited* by the contact of the food or drink; the excitement runs along the nerve to the spinal marrow; it is thence reflected, and it is then sent along this inferior and recurrent laryngeal to the muscles which close the larynx.

Thus we have traced a *physiological Diastaltic Nervous Arc*. Many such—a *whole class*, indeed—exist in the animal economy.

It is by the means and agency of such *Arcs* that all the *orifices* and *exits*—all the *sphincters*—of the animal economy are guarded and governed.

The tubal structures, which proceed from, or lead to, these sphincters, are generally influenced by a *twofold* kind of action—the diastaltic, through a diastaltic nervous arc; and the peristaltic, through the ganglionic system. Such is the case with the œsophagus, the rectum, the uterus. Prof. J. Reid observed that the œsophagus is an organ of diastaltic action. I had previously observed its peristaltic action when insulated and separated from all other tissues, and laid on the table for observation: parsley contained in that of the rabbit was gradually extruded.

The same principles doubtless obtain in regard to the uterine organs. Partly diastaltic, partly peristaltic, parturition may take place from the influence and energy of either or both of these powers. This act is doubtless further *assisted* by volition, and it may be promoted by *hope*, or enfeebled by *fear*, as it is by other elevating or depressing passions; whence the questionable use of chloroform in ordinary cases of accouchement.

But the most important, perhaps the most exclusive, of all the diastaltic functions, is that of the *respiratory movements*. They are purely diastaltic, ceasing instantly when their connexion with the spinal centre is, *by any means*, destroyed. These movements may be modified, and are always modulated by volition, and they are rendered variously hurried and irregular by emotion. But otherwise they are solely dependent on several diastaltic nervous arcs, of which the medulla oblongata is the centre. Of these a precise view of each is given in this *Table* and diagram of the *Diastaltic Arcs of Respiration*.

I. The Excitors—

1. The Trifacial,
2. The pneumogastric,
3. The Spinal, Nerves.

II. The Medulla
Oblongata.

III. The Motors—

1. The Intercostal,
2. The Diaphragmatic,
3. The Lower Spinal, &c.

I may here observe that I have been anxious to illustrate every *principle* of the spinal system by *experiments*, and by experiments which I could lay before you; and I have chosen the frog for these experiments, because it is most easily obtained, is most readily subjected to dissection, and possesses in an eminent degree, especially on the return of spring, after the season of hybernation, the properties of excitability and irritability. In every experiment the cerebrum may be severed, and with it, all sensibility and susceptibility to *pain*, as the very first step.

I have thus demonstrated the *Diastaltic Law of Action* of the vis nervosa, and established the *Diastaltic Nervous Arc* in anatomy. In my next lecture, I purpose to apply these principles to Physiology, and to treat of the varied abnormal *conditions* of the excitability, preparatory to treating of Pathology and Therapeutics.—[*London Lancet*.]

The treatment of Sciatica by cauterization at points remote from the seat of pain, has been engaging the attention of our Parisian friends for several months. The application of the actual cautery to the dorsum of the foot was noticed in the last volume of this Journal (p. 179). We now present to the reader some of the researches made in relation to its application to the ear; and also those upon the substitution of Electro-galvanic shocks in lieu of the more objectionable incandescent metal.

Cauterization of the Ear for Sciatica. (Translated from the French for this Journal.)

It is a wise principle to reject nothing in practical medicine, for our art has not unfrequently been benefited by facts collected by the unprofessional. Our readers doubtless recollect the good effects of cauterization of the dorsum of the foot in the treatment of sciatica. We now have a new cauterization, the *modus operandi* of which is still less understood. It is then for

clinical experimentation to determine this, and we therefore reproduce, in extenso, the following case reported by M. Inteau.

Martinet, aged 42 years, of athletic constitution, was admitted on the 22d May into the ward of M. Malgaigne. Two years before, he felt acute pains in the lumbar region, which obliged him to remain in bed. At the end of eight days' treatment, consisting of frictions with an ointment, the composition of which he did not know, he resumed again his occupation. In the month of March last, he had a return of the disease, which caused him to enter the Hôtel Dieu. In three times, forty-eight cups were applied to the right hip and the lumbar region of the same side. Three large blisters were drawn over the trochanteric region and knee. He took, besides this, ten plain baths, fifteen vapor baths, and afterwards forty sulphur baths. He came out of the Hôtel Dieu, 16th May, suffering much less than when he went in, but still feeling pains in the thighs, the knees, and sometimes in the loins.

When he presented himself to us, he had pain in the region of the trochanter, along the posterior part of the thigh and in the anterior part of the knee, and twitchings in the posterior part of the leg and foot. He also felt some dull intermittent pains in the lumbar region. All motion was very painful; walking impossible, without crutches; limping very marked, and great restlessness at night. M. Malgaigne immediately cauterized the anterior part of the helix with a heated iron, and ordered a bath. To our great surprise, the patient felt afterwards but a very insignificant pain. The movements of the knee and thigh were free, and he could walk in the ward without crutches. The limping had almost entirely disappeared as by enchantment. By precaution the patient was ordered to keep his bed. On the 23d he slept the entire night. I feel yet, said he, slight pains in the interior of the thigh, the knee, and the external side of the foot; but they are not to be compared to what they were yesterday morning. He was dismissed twenty-four hours after his admission with the advice to take only moderate exercise, and to return in a few days. He came back on the 27th of May. After going out he fatigued himself a little, notwithstanding the advice of M. Malgaigne, and felt a slight pain behind the trochanter major. When in bed, he suffers no pain. A little feebleness still existed, in consequence of which he limped when walking. Since this attempt, similar cases have presented themselves at the clinic of the hospital Saint Louis, as they were not admitted in the ward. In two of them the sciatica was simple, regular, followed exactly the course of the nerve; in another, it followed very nearly the course of the

gracilis muscle, occupied the knee, and passed down the calf of the leg. The result of the same cauterization has been uniform; that is to say, the pain has immediately disappeared, either entirely or partially, and the patients have returned home without limping, considerably astonished at being so rapidly or so notably relieved, even when they were not completely cured

[*Bul. Gén. de Thérap.*, July, 1850.]

In one of our last numbers we made known the favorable results obtained by M. Malgaigne, in the treatment of Sciatica by cauterizing the ear. However extraordinary a therapeutic agent of this kind may appear, we publish it, because we think that nothing should be disdained in practice, and that it is our duty to keep our readers apprised of the results of cauterization in the hands of various surgeons. M. Malgaigne seems to entertain for it much regard, and, in the last number of the *Revue Médico-Chirurgicale* has announced that of sixteen or eighteen cases, in which he has operated, about one-third have been completely cured on the first day; in another third, the pain has diminished or disappeared to return in two, six, twelve, or twenty-four hours; and, finally, that in many there has been not even a temporary benefit. These results are about the same as those obtained at the Hôtel Dieu, by M. Jobert (de Lamballe). But it does not appear that the results have been so favorable at La Charité. M. Briquet has treated four patients by cauterization. One, alone, was permanently relieved; the other three have derived no benefit, not even at the moment of cauterization. We find in the last number of the *Gazette Méd. de Lyons* an article by M. Berrier, surgeon of the hospital of that city, who, out of six cases, obtained but one complete, immediate and durable success; one not immediate, but temporary; one half success immediate, but final result unknown; one success immediate, and very complete; and, finally, two complete failures. Results so contradictory should make us very reserved upon the future treatment of sciatica. All that is known is that its efficacy is not uniform, but that when it does succeed it is a most rapid and most marvellous remedy. It remains to determine the circumstances in which its employment is particularly indicated.—[*Bul. Gén. de Thérap.*, Aug., 1850.]

In making known to our readers in due time, the new or renewed method of auricular cauterization, applied in the treatment of sciatica, we do not dissimulate the fact that this method is strange, and may even be styled barbarous; nevertheless, favorable facts were reported in great numbers, collected in the large hospitals, and we have reported them as we did

subsequently those which followed. These last were far from being so advantageous a collection as the first. At present, enthusiasm has somewhat subsided, and the truth may be attained. Auricular cauterization produces very dissimilar effects, and in proportions that it is yet impossible to determine. It cures rapidly in a certain number of rebellious cases of sciatica that have been treated without success by other means. This fact is incontestible: it induces immediate, but not durable relief, in a much larger number; and, finally, in a great many it produces no change. But how does it act? It is true, that this inquiry should be made only after positive proof that its effects are favorable. It very frequently happens that we cannot determine the mode of action of a remedy, and when its efficacy is not sufficiently established, we should certainly not pre-occupy ourselves with its *modus operandi*. A priori, it may be concluded that, cauterization acts by producing a brisk and sudden revulsion upon the sciatic pain, in virtue of the Hippocratic principle so often cited—“*duobus doloribus simul abortis non in eodem loco, vehementior obscurat alterum,*” and that what induces us to lean towards this opinion is, that the cauterization is not, as it might be supposed, an insignificant and painless operation. It is, on the contrary, extremely painful, and in patients to whom it is first applied, causes a mingled feeling of surprise and fright, and a great unwillingness to its re-application in those who have once submitted to it. The effect of cauterization seems to indicate that pain produced at the same point by any other proceeding would bring about the same result, and that any great or instantaneous pain produced upon any portion of the body, equally or more sensitive than the root of the helix, would also induce a similar result. This assertion has been fully justified. In a criticism published in the *Union Médicale*, M. Duchenne (de Boulogne) has reported a process of Galvano-cutaneous excitation, which, by the pain produced, rivalled that of cauterization, and the effects of which are identical with it, having the advantage, however, that it produces neither a lesion of the skin nor of the nerves. He applies the extremity of a small copper wire upon the root of the helix, and the extremity of the other one or two inches from it, both wires being insulated with an envelope of silk. The other extremities of the wires are placed in connection with the indirect current of his apparatus. This current produces an electric action upon the cutaneous sensibility. The apparatus being at its maximum, he arranges it so that the intermissions are as nearly approached together as possible. The application is made during one or two minutes. The sensation developed is most acute, and the patients say cannot be

surpassed by the action of fire. This ceases suddenly after the operation, without leaving any appreciable traces, and strange to say, the cases upon which M. Duchenne has experimented have been as rapidly modified as by cauterization. But the results obtained by this physician are not limited to this point. In exciting other surfaces of the body, equally as sensitive as the helix, the lobe of the nose, for example, and in other cases the thoracic region, the face, the dorsum of the hand, he has seen the artificial pain produced cure and calm the sciatica. This demonstrates that the painful excitation of the auricular region does not possess more than that of any other cutaneous surface, a curative privilege in this affection. The future will demonstrate whether, if a galvano-cutaneous excitation, practiced upon a point remote from or near the diseased nerve, will or not cure rebellious neuralgia.—[*Bul. Gén. de Thérap.*, Oct., 1850.]

New Treatment of Constitutional Syphilis By LANGSTON PARKER, Surgeon to the Queen's Hospital, &c., &c.

The treatment itself mainly consists in the fumigation of the body, with mercurial preparations, whilst the patient is in a vapour bath. The salts employed are the bisulphuret, the binoxide, and the black oxide of mercury, and sometimes the iodide. The quantity of the first three mentioned varies from half a drachm to three or four drachms; of the latter, from five grains to half a drachm. It is often useful to combine several of these together, and generally so with the iodide of mercury. Thus, "In affections of the testes (sarcocele) and of the bones (the various forms of ostitis, or periostitis), a combination of a scruple of the iodide, and one or two drachms of the bisulphuret or binoxide, would be a proper form." The binoxide of mercury is less irritating to the lungs than the others, and is to be preferred when it is desired to get the vapour applied to the head or throat. The exact mode of its application is thus stated :

"The patient is placed on a chair, and covered with an oilcloth, lined with flannel, which is supported by a proper framework. Under the chair are placed a copper bath, containing water, and a metal plate, on which is put from one to three drachms of the bisulphuret of mercury, or the same quantity of the gray oxide, or the binoxide.—Under each of these a spirit-lamp. The patient is thus exposed to the influence of three agents, heated air, common steam, and the vapour of mercury, which is thus applied to the whole surface of the body in a moist state. After the patient has remained in the bath from five to

ten minutes, perspiration generally commences, and by the end of twenty or thirty minutes, beyond which I do not prolong the bath, it is generally excessive. The lamps are now removed, and the temperature gradually allowed to sink; when the patient has become moderately cool, the coverings are removed, and the body rubbed dry; the patient is suffered to repose in an arm-chair for a short time, during which he drinks a cup of warm decoction of guaiacum, sweetened with syrup of sarsaparilla." (pp. 5-6.)

When mercury is to be employed internally in combination with the baths, Mr. Parker prefers the binocide or bichloride, in doses not exceeding the twentieth of a grain.

Such are the main features of Mr. Parker's method of treating that protean disease, Syphilis. There are some minor details, such as attention to the bowels, the regulation of the quantity of mercury, and the time of remaining in the bath, which will readily suggest themselves, or can easily be learned from the book itself. Our object is only to direct the attention of the profession to it; and we shall therefore abstain from the expression of any opinion, until we have an opportunity of judging for ourselves. In transcribing some cases from Mr. Parker's volume, the observation occurs, that they are almost all destitute of dates, and of many of those minute particulars which alone can give a scientific value to them: nevertheless, such as they are, we present them to the consideration of our readers. The first is an example of superficial disease of the skin, in which the constitution of the patient was inimical to the employment of mercury in the ordinary way.

"A gentleman consulted me respecting certain symptoms, which he considered, and which doubtless were, due to constitutional syphilis. He had had primary ulcers eight months previously, for which he could not take mercury; the smallest quantity produced diarrhœa, and it even affected him so, when used by friction. The ulcers had healed under a similar treatment, but, soon afterwards, the skin became covered with small scaly blotches; there was deep redness of the throat and nasal fossæ, and the hair and eyebrows came off rapidly. He had taken iodine and sarsaparilla, under various forms, without success; occasionally there was a partial amendment; but he constantly relapsed when medicine was discontinued.

"So confident was I of success in this case, that I predicted a cure by the baths in three weeks. I told my patient he would not relapse. The result justified my prophecy.

"The baths were used twelve times, the gums rendered uneasy and swollen, but nothing more, not a bad symptom accompanied the treatment, and the patient has had no fresh symptom for fifteen months. He took no internal medicines whilst under my care. The third bath checked the falling of the hair and eyebrows, which began rapidly to reappear before the termination of the treatment."—(pp. 21-2.)

We shall next cite a case of sloughing phagedæna quickly arrested.

"A gentleman contracted, from a suspicious connection, a discharge from his urethra, which, in the commencement, was supposed to be gonorrhœa, and for which he was treated. The discharge did not yield to the remedies employed, and about ten days afterwards there appeared round the orifice of the urethra a white ring of ulceration, which spread rapidly. His surgeon became alarmed, and sent him to Birmingham, to be placed under my care. When I first saw this case there was an ulcer the size of a shilling surrounding the meatus, covered with a white slough, and the whole of the glans penis was intensely red, swollen, and shining. On separating the lips of the urethra, the ulcer was seen to extend some distance down the passage.

"I placed this patient immediately in the bath, and kept him there nearly an hour; he was directed afterwards to take a full dose of opium, to apply some decoction of poppies to the part, and to confine himself strictly to bed. On the next day the bath was repeated, and the same practice followed. On the third day the bath was again taken, by which time the sloughs were separating, and a healthy granular surface appeared underneath.

"There had been no extension of ulceration since the first bath. Nine baths completed this patient's cure in less than three weeks, and the medicines employed, as well as the local applications, were of the simplest character. The mutilation was very trivial. The under surface of the urethra and glans penis was destroyed to a small extent, but from this the patient suffered very little inconvenience.

"This was a case of phagedænic ulceration, commencing, as it frequently does, in or at the orifice of the urethra, with the nature of which I was unhappily too familiar, having seen frightful mutilation from sores of this nature under ordinary plans of treatment.

"The first case of secondary phagedæna of the throat, in which I had employed the mercurial vapour-bath, had been so successful, that I felt confident of success here, and the result justified my expectations. I reflected that rules of practice in cases of phagedæna were quite unsettled, mercury being frequently employed as a last resource after the failure of other remedies. It is during this period of bringing remedies to bear upon the disease, and the uncertainty of what to use, that the mutilations so commonly witnessed in such diseases occur. The remedies which I advocate are without risk, and may be employed from the very first appearance of phagedæna, with every hope of success even in the worst cases." (pp. 29-31.)

Lastly, a case which is too recent to be of much value in showing the power of the baths in preventing relapses, but is still sufficiently remarkable to merit quotation.

"A dramatic artist of celebrity, who had been married for some years, and who had not had any primary venereal disease, in any

shape or form, since his marriage, consulted me in the early part of 1849. He came to me as a forlorn hope, despairing of relief, as he had constantly relapsed after discontinuing the best-framed ordinary treatments, conducted by eminent surgeons, amongst whom may be mentioned the late Mr. Aston Key. When I first saw this gentleman, his chief complaint was of the left fore-arm, the bones of which were much enlarged, very painful, and tender to the touch; but his sufferings were much increased during the night, when the pains were at times so excruciating as to deprive him more or less completely of rest; he had not slept one night without pain for seven years. The radius and ulna near the wrist were much enlarged, and were noduleated and uneven. The bones of the nose were a good deal thickened, and he had shooting pains in them. The left testis was five times the size of the right, heavy and lobulated, but neither painful nor tender.

"I considered this case as one of a decided venereal character; although mercury, pushed to salivation, and iodine had previously failed in affording more than a mere transient and temporary relief. Knowing the benefit which I had derived in many former cases, from the use of the baths, I held out to my patient a hope that they might be serviceable to him also.

"I directed him to use half an ounce of the bisulphuret of mercury, and half a drachm of the iodide of mercury for each of the first three baths, which were to be taken every third day, and to take internally some small doses of the hydriodate of potass with colchicum.

"After the third bath, which had slightly affected the mouth, the nocturnal pains had disappeared, and the tenderness was gone from the arm; he could bear the bones pressed and handled, although previously they had been exquisitely tender. The treatment was continued at intervals for three months; sometimes the baths were taken once a week, at other times not so frequently, the medicines also were continued. The pains never returned, and at the end of the period I mention, the nose and testis had been long perfectly well.

"This was a well-marked case of chronic syphilitic periostitis; the treatment of which, by my method, was rapid, safe, and successful. Perhaps no forms of constitutional Syphilis are more formidable than those which are seated in the periosteum and bones; and such affections are very frequently due to exposure, neglect, or want of care during a mercurial course which has been prescribed for the cure of some form of secondary disease, but which not only frequently fails in curing it, but disposes the system to the production of new symptoms of a still more formidable character than those for which the remedies were originally prescribed. It has been said that the iodide of potassium is all but a specific for periosteal inflammation, more particularly that of syphilitic origin; it is certainly, in many cases, an excellent and efficient remedy, but there are many cases also in which it totally and completely fails, not so with the plan of treatment I am advocating; it is all but certain in its influence over such diseases, and the rapidity with which it cures is very remarkable.

"The patient, whose case I have just detailed, was so convinced of the superiority of this method, that I could with difficulty prevent him sending the details of his case, and its treatment, to a Metropolitan daily paper. He had been under the best ordinary treatment for nearly seven years, with little or no benefit; and he was completely and permanently cured in three months by my method. In such cases I believe, after several experiments made on the subject that the best remedies to employ are combinations of the bisulphuret and the iodide of mercury in the proportions, or nearly so, which I have mentioned in the details of the case." (pp. 98-102.)

[*Brit. and For. Med. Chir. Rev.*, July, 1850.]

Iodine Injections in Hydrarthrosis, Hydrocele, Abscesses, Ascites, &c. By M. VELPEAU.

When recently dismissing two patients in whom he had successfully used iodine injection, M. Velpeau took a general review of the present state of the question of the propriety of employing them in dropsical joints, his observations forming a sort of appendix to the celebrated Academic discussion, a few years since.

He observed, that the ill-success recorded by Boyer and other surgeons, as resulting from throwing injections into joints, much alarmed practitioners; the mischief which resulted, however, really depending upon the nature of the cases which were selected and the procedure adopted. M. Velpeau appealed against the abandonment of the practice on these grounds. For fifteen years he had employed iodine as an injection in hydrocele, without any dangerous inflammation being excited, so that he was encouraged to extend its application to various serous cysts; and in his 'Operative Medicine,' published in 1839, he suggested that it might without danger be thrown into the peritoneal cavity, an opinion afterwards confirmed by its injection into congenital hydrocele and hernial sacs. It was only in 1840, however, that he ventured to inject the knee-joint in two instances, and that by accident, the tumour in the ham communicating with the cavity of the joint. The good success of these cases, and of others in which joints were purposely injected by M. Bonnet, of Lyons, led him, however, to adopt the practice in numerous other cases. At least fifty such, treated by different surgeons, are now on record, in none of which were any alarming symptoms developed. The dangerous and fatal consequences resulted, in Boyer's cases, from the joint being widely laid open, and irritating fluids several times thrown in; while in the modern operation the smallest possible trocar is employed, and, in the majority of cases, a single injec-

tion suffices. The pain and febrile action in some cases have been considerable, requiring for their removal, in the opinion of some surgeons, leeching and abstinence; but M. Velpeau has always found them, as in the case of their appearance in hydrocele, if let alone, gradually disappear. Their occurrence at all is, indeed, rare. In general, a little redness, pain, and swelling are observed after the injection, and are soon followed, by slight fever. Increasing during three or four days, these symptoms then become stationary; afterwards to diminish, and to entirely disappear about the fifteenth day, together with a part, or sometimes all the tumefaction.

M. Velpeau now considers there is not more danger in injecting a joint than the tunica vaginalis, the chief fear being, indeed, that the operation may fail. Injection, in fact, succeeds best in proportion as the cavity more nearly approaches a simple serous one. Thus, in cysts of the neck, where the membrane containing the fluid is everywhere surrounded by soft tissues, the operation never fails. In the tunica vaginalis where the testis places the serous membrane in a less favorable condition, failures are sometimes, though very rarely, met with; while in the joints, in which the serous membrane is free only to a limited extent, and chiefly lies on solid or very hard parts, success is more rare. Still, as the employment of injection is not preventive of other means, is not dangerous, and scarcely more painful than a blister, we should always resort to it, when a simple collection of fluid resists ordinary treatment. As, however, such collections are usually capable of being dispersed, it is an operation not often demanded, and it should not be resorted to when the collection of fluid is dependent on caries, necrosis, &c. The fears entertained by some that ankylosis might result, are unfounded, the patient recovering the use of the part as soon as the swelling has disappeared.—*Gaz. des Hop.*, No. 58.

The French practitioners do not seem to have taken much notice of the writings of Dr. Borelli, of Turin, upon the employment of iodine injections in various affections, although he has been one of their most active advocates. In the last paper we have seen, he reiterates his opinion of their great utility, and furnishes additional cases. These consist of examples of *hydrocele* (he states, that hydrocele in boys and hydrocele of the cord in adults are always cureable by simple puncture), *encysted tumours* after the evacuation of their contents, various forms of *acute* and *chronic abscesses* and *adenitis*. He objects to the use of resolvers, as being both tedious and useless. When matter is once formed, the abscess is rapidly cured by the injection. He relates, too, a case of *ranula* yielding to the employment of the injection, although simple puncture had re-

peatedly failed. Bouchacourt had, however, already published a similar case in the 'Bull. de Therap.' for 1843. After quoting a case of *fistula in ano* successfully treated by Van Camp, at Angers, and relating one of his own, he states he has always hitherto failed in treating *fistulæ* in connection with diseased bone ; but quotes cases from the veterinary practice of Professor Beroscino, in which the iodine was successfully used in fistula connected with diseased cartilage.

Dr. Borelli considers his practice in treating abscesses by this means as somewhat peculiar, inasmuch as he seeks to obtain union by the first or the second intention, according to the requirements of the case. The suppurative process being well determined, and the acute inflammation of surrounding parts repressed by cataplasms, after opening the abscess in its most dependent part, and evacuating the contents as far as possible, he introduces a small syringe through the aperture, and throws in pure tincture of iodine with some force, allowing it to remain in, when the pain is not too great, about half a minute. He waits three or four days to see whether the plastic effusion into the pyogenic cavity will effect its occlusion. This, however, is seldom the case, unless the abscess is very small and the engorgement of surrounding tissues slight. The injection, therefore, will require repetition every two or three days, according to the amount of reaction produced ; and when this is in excess, emollient cataplasms are required. The author has never seen any ill-effect, local or general, following the employment of the iodine.—*Omedei Annali*, vol. cxxviii, pp. 79-154.

The question of injecting the abdomen with iodine in *ascites* is no longer one of mere hypothesis. M. Dieulafoy resorted to it three times in the same subject, with the effect of producing a cure, and Dr. Leriche, of Lyon, has recently published two cases, in which a single injection after the complete evacuation of the fluid sufficed. M. Boinet also has recently presented a memoir to the *Société de Chirurgie*, in which he has collected eighteen cases of ascites from various sources, wherein different substances, as gases, water, iodine, &c., have been injected. In fifteen of these success followed, and in only one did the patient die, iodine seeming to be the especially preferable substance, M. Morel, reporting upon this paper, pronounced an almost unqualified opinion in favour of the practice ; but MM. Vidal, Gosselin, Robert, and others protested against drawing any such hasty conclusions from cases the history of which had been imperfectly given, and have to be confronted with others in which a fatal termination has resulted, an example of which has recently occurred in Paris.—*L'Union Médicale*, Nos. 17, 18, 19 and 60.—[*Brit. and For. Med. Chir. Rev.*, July 1850,

Method of making Transparent Preparations of the Spinal Cord.

Mr. Swan details the following process, which deserves to be more known on account of its simplicity, as well as effectiveness:—

The spinal cord is to be cut into pieces of one or two inches long, so that each may include all the roots of one or more nerves in each quarter. The dura mater is to be removed, and the nerves preserved as far as the ganglia. Each portion of the cord is then to be divided through the median line, and each half again between the appearance of the anterior and posterior nerves, so that there will be four quarters separated. As the roots of the nerves enter rather obliquely, it is necessary to cut off close to the nerves a somewhat triangular portion of each side of the quarters, so as to make the preparation a flat piece, containing the nerves and their continuation through the medullary and grey matter. The piece thus cut is to be placed on a glass slide, and dried before the fire on a plate covered with paper. In two or three days it is to be raised from the slide with a thin scalpel, and the soft matter underneath is to be carefully removed; it is then to be placed on a fresh slide, and gently pressed on this with the finger, and there remain until it is dry. When dry it is to be raised again from the slide and turned over, that any matter preventing the transparency may be removed; this is known by holding it from time to time to the light. This process of clearing is facilitated by moistening it with spirits of turpentine now and then dropped on it. If it had become uneven on the surface in drying, another slide may be pressed on it gently so as to flatten it; it may then be examined by the microscope, and any matter still obscuring it be removed. When it has thus been made sufficiently transparent, a little thick Canada balsam is to be smeared on the under surface, and by means of the ball of a finger moistened with spirits or turpentine, pressed on the side so as to remove any vacuity of air bubbles, which otherwise make the preparation uneven under the microscope, and give the idea of a membrane or some different substance intervening amongst the rest. The next day a small drop of Canada balsam is to be smeared over the surface of the preparation with a finger, and immediately after two or three drops of spirits of turpentine; and this process of applying the balsam and spirits of turpentine may require to be repeated two or three times for giving sufficient clearness, and guarding it from mould and atmospheric changes. Although it wants to be transparent, it does not require to shine as if it were varnished,

and therefore the balsam and spirits of turpentine are directed to be wiped off with the finger.

Another preparation may be made by cutting off a portion of the surface of the spinal cord, with very short ends of nerves attached to it, and very little of the medullary matter underneath. It is to be dried, and treated like the other preparations, and when finished it will show the nerves very beautifully, as they are collecting themselves into fasciculi; and some parts of it will be found only a coarser representation of the more internal roots. Another preparation of a similar kind may be made by leaving the medullary surface uppermost, when nearly the whole of the medullary matter has been removed.—*Medical Gazette.*

Rupture of the Spleen.—BY THEOPH. MACK, M. D., *St. Catharines, C. W.*

As the works ordinarily found upon the shelves of a medical practitioner's library afford no instance of the above pathological appearance, except resulting from external injury, I send the following case for publication, in the hope that it may prove of sufficient interest to reward the perusal.

Thomas Flynn, ætatis 48, blacksmith, of medium stature, constitution somewhat impaired. In earlier life he had served as a private soldier in an infantry regiment, and had been admitted to the regimental hospital a few times for some disorder of the chylopoietic viscera, for which local depletion and counter-irritation appear to have been prescribed, as marks of leech-bites and visicants are apparent over the epigastric region. Last summer I was called upon to prescribe for some abdominal affection from which he suffered; its precise nature I cannot now recall to mind,—probably cholera morbus. During the last eight or nine months he has resided chiefly in a shanty on the margin of a stagnant pond, near the debouchement of the Welland Canal into Lake Ontario. For some weeks he had been labouring under Intermittent Fever of a tertian type. In the treatment of this disease he had employed a certain nostrum ycleped "Cholagogue," which as his friend expressed it, "broke the chill," *i. e.*, interrupted the paroxysms, so that he had been enabled to work at his trade for the space of three or four days, still complaining of dizziness occasionally, and the secondary effects of mal-assimilation. Upon the day he was attacked with his last illness, viz., 22d June, ult., he was engaged in the construction of some iron bands; after swallowing a moderate draught of cold water, he was sudden-

ly seized with severe pain, and having been carried to bed a messenger was despatched for me. I found him writhing in great agony ; he referred the seat of pain to the left side of his chest and abdomen. The skin was covered with copious sudor which trickled in streams from his face, and completely saturated the clothing ; features sharpened, and face expressive of great anxiety ; intellectual system not affected ; tongue cool, of a leaden hue, slightly coated ; bowels torpid ; tenderness on pressure, in left hypochondriac region, extending to the umbilicus ; the abdominal pain deep-seated, not of the acute character of peritonitis ; respiration hurried : no abnormal resonance ; no râle ; heart's action extremely rapid, feeble, and accompanied with bruit de souffle ; pulse 160, small and tense ; urine suppressed. The group of symptoms rendered the diagnosis difficult. Calomel and tinct. opii. were exhibited, fomentations, followed by a large sinapism, were directed. No relief having ensued in six hours, Croton oil and enemata, with a long tube, were resorted to without producing any action of the bowels.

23rd. Pulse becoming indistinct ; he appears moribund. As I had decided the previous evening that the symptoms were to be ascribed to some extensive extravasation within the cavity of the abdomen, all curative efforts were desisted from. He died about sixteen hours from the time of seizure.

Morbid Appearances.—Our examination was limited to the abdominal viscera, by request of his friends. The integuments of the abdomen were distended and tympanitic. The first incision through the linea alba was accompanied by the escape of a large quantity of flatus, and followed by bloody serum. Peritoneum slightly injected, peritoneal covering of the intestines of a pink colour. The liver of the usual size and weight, but softened in structure, and upon being incised the parenchyma appeared much more dark than natural. Pancreas small and hard. Stomach and intestines healthy. These being taken away, and having removed with a sponge about *five pints of sero-sanguinolent fluid*, we found upon the left side, extending from the diaphragmatic extremity of the spleen, and behind that organ to the commencement of the lumbar region, *a large clot of fibrin*, from blood which issued through a rent in the investing membrane in the spleen, this was easily peeled off from its contents, which were the substance of the spleen, of a light chocolate colour, and extending from a defined edge, a dark brown mass of effused blood, destitute of any traces of organization. The left kidney was enlarged and pale : the pelvis contained a small quantity of a dark grumous liquid. The remaining contents of the abdomen presented no pathological appearances.

In this case case, it is probable that a rupture of the splenic vessels, occurring during the congestion accompanying the cold stage of ague, first gave rise to an extravasation of blood within the splenic membrane. (This might have been increased at each subsequent congestion.) The afflux of blood following the reception of the cold water into the stomach at the time of the attack, ruptured the disturbed capsule and peritoneum, and a fatal effusion resulted.—*Brit. Amer. Phys. Jour.*

Suppression of Urine, one of the Symptoms of Poisoning from the Chloride of Mercury.

It has been observed that those who have taken large doses of the bi-chloride of mercury, (say poisonous doses), generally have entire suppression of the urine. We believe the cause of such disturbance in the kidneys has never been given, if indeed it has ever been attempted. At a late meeting of the Suffolk District Medical Society, quite a discussion arose on the poisonous effects of this preparation of mercury, the probable quantity necessary to produce death, and the time required to bring about such a result. There was much discrepancy of opinion among the most learned of the members on the points in question. It was agreed, however, that entire suppression of the urine always followed when the poison was taken in sufficient quantity. Another curious circumstance was alluded to, though not explained, viz., that in those who die from the effects of bi-chloride, the *primæ viæ* rarely revealed, on the post-mortem, any sign of inflammation or its results. When the preparations of mercury are exhibited to the patient, they produce effects, varying in manner, according to the form, quantity and manner of administering them. It is known that if ten grains or more of the blue mass, or calomel, are given at one time to a patient, its effects are entirely different from what they would have been, had the medicine been given in divided doses. So with the bi-chloride; if large quantities are taken, it often excites vomiting to such an extent that it proves harmless, when grain doses would destroy life. In coming to the point in question, why is it that the kidneys cease to perform their functions? We think it plausible to assume the following reasons, viz.:—Bi-chloride of mercury is a powerful stimulant as well as sialagogue: it is a specific stimulant to the salivary glands, although many think it acts through the circulation. Now if these glands become aroused to such an extent that they are continually pouring out their secretions, it must be evident that the blood is deprived of so much of its elements, viz., water with a trifle of other matter. This, of course, is the largest

constituent of urine. While the excessive action of the salivary glands is in force, it must necessarily detract from the secretion of the kidneys. It is well known that the amount of urine secreted in the summer is less than it is in the colder or winter seasons, which finds a ready explanation in the fact of one of the great constituents of the urine passing off by the skin in large quantities during the warm season. No matter what it is that takes from the blood its watery part, and causes it to pass out of the body through any other than its natural channel, the effect will be a suppression of urine. Therefore we might safely come to the conclusion, that if the kidneys fail to secrete, in these cases of poisoning, it is because they have not their proper element to stimulate them. As this subject seems to be a mooted one, we have given our views, hoping that sufficient interest may be excited to bring out the opinions of those more learned in such matters.—[*Boston Med. and Sur. Jour.*

Treatment of Rubeola by Inunction.—By JOHN EVANS, M. D., Prof. of Obstetrics, &c., in Rush Medical College, etc. etc.

June 1, 1850, Miss F., aged 15 years, was labouring under the symptoms that characterize a violent attack of Measles. The febrile action was strong—pain in the extremities, loins and head, severe—the eyes were injected, suffused and intolerant of light—distressing nausea was constant, and the characteristic eruption was well marked upon the face, neck and breast.

I gave Dovers powder grs. viij. every six hours with free use of warm teas.

Finding no abatement of the distressing symptoms the next morning, I determined to use inunction, and, as practiced by Dr. Schneeman in Scarlatina, directed the patient to be rubbed with a piece of fat bacon over the entire cutaneous surface.

The relief was marked by the subsidence of all the distressing symptoms in a few hours, and the application was repeated twice the next day. No other treatment was applied except the free use of warm teas. The recovery was more rapid than I had before seen in such cases, and without any disagreeable sequel.

Two other members of the same family were treated by the inunction with the same favorable results.

I have since used the plan of treatment in a number of cases and with uniform and prompt relief.—[*North-Western Medical and Surgical Journal.*

Effects of Ether in Childbirth.—It has been observed, when ether has been given to parturient women, that its odor is discoverable in the breath of the child after birth; showing conclusively, that the blood of the mother must have been very strongly impregnated with the ether. Having observed the same phenomenon in a case that occurred in our practice a short time since, we were fully convinced that the *fœtus* may be etherized in utero. But what appeared to us as very remarkable, was the short time that elapsed between the mother's inhaling the ether, and its sensible presence in the lungs of the child, which was born in just *twenty minutes* after the first inhalation by the mother. The quantity used, in this case, was two and a half ounces only, and at no time was the consciousness of the patient destroyed, but on the contrary she was bright and cheerful, even when the *pains were most severe*. There was no mistake about it whatever, the child being at the time in another room where there had been no ether. The nurse, while washing it, directed our attention to the fact that the "*child's breath was all ether*"; and upon drawing near to it we could distinguish the smell of the vapor. The little fellow was very good-natured indeed, and did not seem to mind the manipulations of his first toilette, which the kind-hearted nurse performed in a manner that would put the opposers of hydropathy in shivers.—[*Boston Med. and Sur. Jour.*]

Theory of the Production of Males and Females. By SILAS HUBBARD, M. D.

To the Editor of the Buffalo Medical Journal:

Sir—Many have been the theories of generation which have been either proved to be, or are now regarded as erroneous, and are merely mentioned as matters of history. Among these by-gones, are all the ancient theories of the causes of the production of males or females; but as this subject still occupies the serious attention of very many respectable physicians, I may be excused for offering the following new and original theory, viz: that males are begotten from one to ten days before, and females from one to ten days after, the courses of the mother. In proof of this observation, I shall now merely say, that it has invariably held true in all the cases I have had the means of knowing, which are half a dozen.—[*Buffalo Journal.*]

The Kite-tail Plug.—This, which has long been employed by M. Bretonneau, of Tours, M. Trousseau regards as excellent in uterine hemorrhages, being both easy of application and withdrawal. It is formed of a thread about forty feet long, to which at intervals of about six or seven inches, pieces of card-

ed cotton (to be oiled before using the plug) are attached. M. Brettonneau prefers it to all other means of plugging in epistaxis.—[*L'Union Médicale*, No. 25. *British and Foreign Med. Chir. Rev.*

Miscellany.

TO THE READER.—In assuming the editorial supervision of the Southern Medical and Surgical Journal, the undersigned is apprehensive that the loss of the judicious and able management of his predecessor may be seriously felt. With abundant professional and literary lore, quickness of perception and a felicitous diction, the late Editor possessed advantages well calculated to intimidate his successor, in view of the unfavorable comparison to which his efforts might be subjected. The undersigned, in entering upon an untried arena, with his attention continually distracted by other arduous professional duties, must therefore crave from the readers of this Journal their fullest indulgence. It is proper, however, to add, that every exertion will be made by the Editor, as well as by his friends who have kindly proffered their assistance, to render the work acceptable, and to sustain its high reputation. With a large and continually increasing list of subscribers, the Publisher will also be incited to renewed efforts to make the forthcoming volume equal in style to any in our country.

Contributions are earnestly solicited from those who have already given interest to the original department of this periodical, and from all who may be moved by the desire to aid in the advancement of science. That there are many, very many, in Georgia and the adjacent States, who might render efficient service to the profession, as well as to their own interests, by devoting their leisure moments to the record of interesting facts and practical observations, cannot be doubted. The monthly form of the Southern Medical and Surgical Journal is peculiarly adapted to the wants of active practitioners, whose time is so much engrossed as generally to forbid the researches necessary in the preparation of lengthy and labored articles, and who might, yet, without much inconvenience, furnish contributions of less pretensions and of more intrinsic worth. Medical statistics, carefully drawn up, showing the peculiar diseases and mortality of whites and blacks in our cities and villages, would be highly interesting, especially if comprehending a sufficient series of years to authorize general conclusions. The bills of mortality of some of the Southern

cities have been published ; but we have never seen any relating to the rural districts. As there are few or no public cemeteries in the country, such bills cannot be obtained complete ; but it would not be very difficult for a practitioner to take the census of the plantations and families under his professional charge, and to note the deaths (with their causes) which occur among them during the year. He would thus in a few years accumulate a fund of facts, of great value in establishing the relative salubrity of different sections of the country, to the white and African races. Annual reports of this kind are respectfully solicited. The proceedings of local medical societies should find a place in this Journal, whenever of general interest. Biographical notices of deceased physicians of distinction, setting forth their virtues and peculiar skill, would not only be a just tribute to merit, but tend to elevate the profession, by holding up examples worthy of imitation. L. A. DUGAS.

The works of Marshall Hall never having been republished in this country, many of his views may be novel to the reader. The practical applications of his researches upon the functions of the nervous system are so numerous and important, that we feel assured that his Lectures, containing, as they do, an epitome of his present and matured views, will not fail to be read with great interest. We shall therefore offer no farther apology for giving them a place in this Journal whenever we can do so.

We invite the reader's attention to Dr. Norwood's article on the properties of *Veratrum Viride*. If this agent can, in the hands of others, be productive of the same striking modification of the heart's action, the discovery will certainly be one of the greatest importance. To be in possession of the means of diminishing at will the most intense arterial excitement, would give the practitioner an advantage not easily over-estimated. We are not surprised, therefore, at the enthusiasm manifested by one who believes that he has discovered this desideratum. The remedy should be fairly tested, and the result reported.

We are authorized, by the publisher, to state that those who may contribute to the pages of this Journal will receive it without charge during the year.

Sir B. Brodie.—Of the moral and scientific improvement of the profession, Sir Benjamin has often descanted, with a clearness and force amounting to eloquence, in his various addresses and introducto-

ry lectures. He never did so more beautifully than at the recent *soirée* of the Western Medical Society, when resigning the presidency into the hands of Dr. Robert Lee. On that occasion, instead of descanting on the "degeneracy" of the profession, like Mr. Skey, he summed up the whole question of the dignity or degradation of medicine in a few words: "Gentlemen," said he, "MEDICINE IS A NOBLE SCIENCE, BUT A LOW TRADE." This is true: pursued as a Science, nothing can be higher; followed merely as a Trade, nothing can be more ignoble; and the man in the poorest practice may reverence his profession as a science, while the richest may grovel before it as mere trading!

It is true praise of Sir Benjamin Brodie to say, that he is more distinguished as a physician-surgeon than as an operating-surgeon. His vocation has been more to heal limbs than to remove them. His imagination has never been dazzled by the brilliancy of the knife, to any great operative display. He has, however, always been a most steady and successful operator: lightness of hand; caution, without timidity; never failing coolness, and fertility of resources, have been his distinguishing characteristics. He has made no secret of his opinion, that the operative part of surgery is not its highest part. Diagnosis has always been his great strength, and his opinion, has therefore, been always deeply valued by the profession and the public. We believe his heart has always been with hospital, rather than private practice, but in almost all cases men are more fond of their early occupations than of those which come afterwards. As a teacher he was always distinguished for the value of the *matter* he had to communicate. Those who heard him in the early part of his career say that he was then energetic rather than polished; that he appeared to struggle with the weight and mass of facts he had stored up in his mind. But in later year, his delivery has been fluent and perfect. No man in his profession can deliver himself more readily or more elegantly than Sir Benjamin.—[*London Lancet*.

Surgical Operation.—Professor Parker, in an obstinate case of Cystitis, at Bellevue Hospital, with the view to give rest to the bladder, has recently cut down as in the lateral operation of Lithotomy, and made a vertical incision into the bladder. This experiment may be unprecedented, but is possibly defensible by the success of Dieffenbach and others in Germany, in an analogous operation for fistula in perineo, in which, by allowing the urine to pass off through the artificial opening made by the incision, the fistulous canal has been closed. Dr. P. is very strenuous in his advocacy of the theory that *rest* to an inflamed organ is the first condition of cure, the hypothesis which is supposed to justify the opium practice in enteritis and pneumonia. This case, however, is probably the first instance in which an inflamed bladder has been opened through the perineum, for the single object of securing rest for the diseased organ, and its result may teach a salutary lesson in therapeutics, as well as surgery.—[*New York Medical Gazette*.

To the Medical Profession.—The undersigned, Chairman of the Standing Committee on *Practical Medicine*, appointed by the American Medical Association, May 1850, respectfully solicits the co-operation of members of the Medical Profession in furnishing materials for the annual Report in May 1851. The duty of this Committee, as defined by the Constitution of the Association, is to “prepare an annual Report on the more important improvements effected in this country in the management of individual diseases; and on the progress of Epidemics; referring, as occasion requires, to medical topography, and to the character of prevailing diseases in special localities, or in the United States generally, during the term of their service.” In order to fulfil the objects thus expressed, the requisite data must be supplied by medical practitioners in different sections of the Union. This is more particularly true with reference to the “*progress of Epidemics*” and “*the character of prevailing diseases in special localities.*” Communications, therefore, are particularly desired from persons residing in places in which Epidemics have prevailed, or in which prevailing diseases have been marked by special characters during the present year. Epidemic Cholera and Dysentery are known to have prevailed more or less extensively in different parts of the country during the past summer. Facts bearing upon the features peculiar to the present season, the production, diffusion, mortality, treatment, etc., of these diseases, will be acceptable. It is requested that Communications upon these or any of the subjects coming under the cognizance of the Committee, be transmitted to the undersigned by the 1st of March, 1851.

All contributions with which the Committee may be favored, will receive due attention and acknowledgement. AUSTIN FLINT.

BUFFALO, N. Y., Nov. 1850.

[*Buffalo Medical Journal.*

Medicine in Turkey.—The government of the Sublime Porte have just decreed the formation of a body of salaried medical men, who shall attend both the rich and the poor, with the obligation of not receiving any remuneration from the latter, and to pay especial attention to all questions relating to the public hygiene of the country.—[*London Lancet.*

NOTICES OF NEW BOOKS.

We have received from the publishers, through Messrs. Joseph A. Carrie & Co. and Thos. Richards & Son, of this city, the following works, to which we cheerfully call the attention of the Profession. They are all standard works, which we regret not having room to notice critically.

The Diagnosis, Pathology, and Treatment of the Diseases of the Chest.
By W. W. GERHARD, M. D., &c., &c. Third edition, revised and enlarged. Philadelphia: Barrington & Haswell. 1850. pp. 351.

It fell to our lot to review for this Journal the first edition of Dr.

Gerhard's very valuable work. The present edition merits still more the high encomiums bestowed upon the first, for it is brought up to the present state of the science. It is certainly one of the very best works of the kind, and inculcates the principles of physical exploration so concisely and plainly that no one can read it attentively without feeling that the difficulty of acquiring a knowledge of auscultation has been very much exaggerated.

Renal Affections: their Diagnosis and Pathology. By CHARLES FRICK, M. D. Philadelphia: Lea & Blanchard. 1850. pp. 189.

This is a valuable compend of the present knowledge of Renal affections, and especially of their diagnosis. It ought to be in the hands of every practitioner who has not leisure to study more voluminous productions on the subject.

The Races of Men: a fragment. By ROBERT KNOX, M. D., &c., &c. Philadelphia: Lea & Blanchard. 1850. pp. 323.

A work characterized by great independence of thought and novel views. Although conflicting with many received opinions, it will be read with interest by the unbiassed student of Natural History.

A Practical Hand-book of Medical Chemistry. By JOHN E. BOWMAN, Fellow of the Chemical Society, &c., &c. Philadelphia: Lea & Blanchard. 1850. pp. 288.

This publication is quite opportune. The subject of organic chemistry is daily increasing in interest to the practitioner of medicine. We have here in a concise form "instructions for the examination and analysis of urine, blood, and the more important animal products, both healthy and morbid, and also directions for the detection of poisons in organic mixtures and in the tissues." This mere statement of the contents of the work will sufficiently indicate its importance.

Elementary Chemistry, theoretical and practical. By GEO. FOWNES, F. R. S., &c., &c. Edited, with additions, by Robert Bridges, M. D., Prof. of Chemistry in the Philadelphia College of Pharmacy, &c., &c. Third American edition, from a late London edition, with numerous engravings. Philadelphia: Lea & Blanchard. 1850. pp. 516.

The call for a third edition of Fownes's Chemistry, and its adoption as a text-book by many of our medical colleges, is as good an eulogy of the work as any we might indite.

Observations on certain of the Diseases of Children. By CHARLES D. MEIGS, M. D., Prof. of Midwifery, &c., in the Jefferson Med. Col. of Philadelphia, &c., &c. Philadelphia: Lea & Blanchard. 1850. pp. 215.

This work contains chapters upon Diagnosis, Caput Succedaneum, Inflamed Eyes, Coryza, Bowel complaints, Jaundice, Dress, Cyanosis Neonati, Respiratory disorders, Whooping cough, Laryngismus, and Scarlatina—all of which topics are treated with Dr. Meigs's acknowledged ability and original diction. The work is neither a systematic nor a complete treatise upon the Diseases of Children, but a fragment, which may be consulted with much advantage.

The Microscopic Anatomy of the Human Body, in health and disease. Illustrated with numerous drawings in colour. By ARTHUR HILL HASSAL, M. B., &c., &c. In two vols. London: S. Highly. 1849-50.

We are indebted for this splendid work to S. Hart, Sr., of Charleston, Importer of Foreign Books. It is decidedly the best treatise of the kind in the English language, and should be re-published in our country.

Baron Humboldt is to spend the winter in Paris, and hopes are entertained that he may visit this country in the spring.

It is stated that there were fourteen hundred deaths in Cincinnati, by cholera, during the months of June, July and August last.

Prof. Parker, of New York, is said to have stated in one of his late Clinical Lectures that Calculus of the Bladder has diminished in that city since the introduction of the Croton water.

Our friends of the far-West are determined to educate their physicians at home. The new building for the "College of Physicians and Surgeons" of the Iowa University, in the city of Keokuk, is completed, and will be used for the Lectures during the present term.

PRACTICE OF SURGERY.—The undersigned has made ample provisions for accommodating such Surgical Patients as may be sent to him from the country. White persons in indigent circumstances, who may require Surgical operations during the term of Lectures at the Medical College, will be provided for and operated upon gratuitously.

L. A. DUGAS, M. D.,

Professor of Surgery in the Medical College of Georgia.

SOUTHERN MEDICAL AND SURGICAL JOURNAL.

Vol. 7.]

NEW SERIES.—FEBRUARY, 1851.

[No. 2.]

PART FIRST.

Original Communications.

ARTICLE IV.

*Contributions from the Case-Book of W. H. ROBERT, M. D.,
of Orion, Pike Co., Alabama.*

CASE I. *A Keloid or Fibrous Tumor of the Skin, with Neuralgic disorder of the Stomach.*—Dilse, a negro woman aged about 24 years, was brought to me in February last. She had been taken with a very severe vomiting, about six months previous, while scouring; menstruating at that time, the discharge was suppressed. The vomiting which followed was so severe, that it became necessary to call in a physician, by whom she was temporarily relieved. The vomiting returned at intervals of two or three weeks, until I saw her; for which she had been cupped, blistered, &c., without any more than a temporary alleviation of the disease. I was told by the owner, that the physician, under whose care she had been, regarded it as a case of cancer of the stomach.

The patient presented no external appearance of disease; her countenance was not distorted in the least—the pulse was not excited, yet presented the characteristic pulse of a nervous female—small and weak. She was subject to head-aches—appetite very defective—no unusual thirst—bowels regular—complained of pain in the region of the stomach, which was aggravated by pressure. Over the region of the stomach was a large fibrous tumor (caused by the blister) about five inches long by three wide and one thick, which was very painful and increasing in size. Vomiting occurred very frequently, without any regularity. The matter vomited, consisted of food or

gastric juice, mixed with water, and was never at all offensive to the smell. There had never been the least trace of blood vomited. The catamenial discharge was irregular. A careful view of the symptoms led me to diagnosticate it, a case of Neuralgic Disorder of the Stomach, and I based my treatment upon that supposition.

I advised the removal of the fibrous tumor, which was readily consented to, and therefore, put the patient upon a tonic treatment to prepare the system for the operation.

April 19th. Assisted by Dr. J. Johns, I carefully dissected away the whole of the tumor; but before I could put in operation the plan I had devised to cover this large denuded surface, the patient was taken with a spasm, apparently affecting the diaphragm, the muscles of the chest, and the arms. The spasm continued about a minute, and subsided as suddenly as it occurred. During the spasm, the pulse was not more affected than the operation would have warranted. I made a counter incision through the skin on each side of the wound, sufficiently long to enable me to approximate the edges to within an inch of each other. Then by sutures and adhesive strips, they were retained in position, and the wound carefully dressed. For forty hours after the operation the spasms continued with increasing severity, regardless of all treatment, until the tincture of assafetida was given in very large doses.

As soon as it was convenient after the operation, she was put upon the use of the Tinct. of Nux Vomica, in doses of ten drops three times a day, for the relief of the neuralgic affection of the stomach. Under this treatment she has regained her health. The cicatrix is now (Nov. 19th,) scarcely one inch wide and three and a half long.

This case is interesting, as illustrating the liability which *some* of the negro race show to the formation of such tumors, after blisters or other wounds of the skin. I say *some* because all are not liable to it. I believe that whites are entirely exempt from it, at least, so far as a practice of twelve years will warrant such a conclusion. Moreover, I believe the nearer they approach the pure African race, the greater their liability to them.

The occurrence of the spasms during the operation, and their steady increase for 40 hours after the operation, is also an inter-

esting fact, when taken in connection with the gastric affection, and the beneficial effects of the treatment instituted for both.

CASE II. *Amaurosis successfully treated.*—In April last, I was requested to prescribe for Mrs. P., aged 62 yrs., of very sanguine temperament. I received the following history of the case:

The lady had always enjoyed very good health, had a good appetite, and was fond of indulging it. Two weeks before consulting me, while taking a walk in the garden, she discovered that she could not see with her right eye; which appeared to be covered with a white cloud. When she applied to me, she could not distinguish the slightest object; scarcely day from night. I directed her to apply a blister above the orbit, and after it had drawn to remove the cuticle, and apply strychnine ointment to the denuded surface. The ointment was made with 25 grs. strychnine to the ounce of lard. She was directed to take a cathartic twice a week, and to live sparingly on milk and vegetables; as soon as the blister cured up, another was to be drawn and the ointment again applied, until some success followed. In two months the cure was complete, and at this date (Nov. 19th,) her vision is as good as ever.

CASE III. *Paralysis of the right side of the Face.*—The patient, Mrs. S. aged 42 years, daughter of the lady referred to in the second case, of sanguine temperament and flushed face, although of very abstemious habits, has once before had paralysis of the face (some five years ago.)

She was attacked one night in the latter part of June 1849, and was not aware of her misfortune until morning. Was treated for some months by a physician without any benefit. In November, 1849, I was consulted, and advised her to be blistered over the angle of the jaws, to use strychnine ointment and to be purged as in case two. The amendment was more rapid than in the preceding case. The treatment was faithfully followed, and in less than two months she was perfectly well and remained so.

CASE IV. *Extensive Eruptive disease in a new-born Infant.* The subject of the present history was the first child of white parents, a female of seven days old when attacked. At birth she appeared healthy in every respect, and nothing unusual was

observed about her during the first week, except a tumor about the size of a half walnut, situated under the scalp and over the posterior superior spinous process of the left parietal bone.

On Saturday the 7th of Sept. 1850, a small pustular eruption commenced appearing around the mouth, on the chin and neck. Being regarded by the parents as the hives, no anxiety was manifested about it. The eruptions progressed very rapidly, and from the size of a pin's head, they would attain the diameter of a half dime or less, in twenty-four hours. Then breaking and discharging a thin seropurulent fluid, and forming a scab and remaining so. Sept. 12th. Dr. J. Johns was called to see the little patient, and found it in the condition described above, with the exception that the eruption had made its appearance on other parts of the body, viz: on the fore arm, the back, the genitals, and the thighs.

Dr. Johns ordered the child to have a dose of sweet oil, the parts to be anointed with mercurial ointment, and afterwards to be powdered with pulverized starch. Sept. 13th. Being called in consultation, I arrived at the house of the patient at four o'clock, P. M., and found the child presenting a most frightful appearance. The face, all around the mouth and on the cheeks, was thickly studded with the eruptions, having on them scabs; some few points of the eruption were as high up as the temple, but none existed on the head. The neck and throat were almost raw; the forearms and wrists were also affected; the back presented some points of the eruption as large as a dime, the thighs and space between the vulva and the groins, were a perfect mass of pustulated points. I should have mentioned, that the whole inside of the mouth, now for the first time, presented a highly inflamed appearance. Tongue, gums and roof of the mouth almost purple.

The older pustules appeared to be stationary, and the great point of interest was now in the left hand. About 10 o'clock to-day, a small vesicle made its appearance at the root of the nail on the index finger. It progressed so rapidly, that at my visit (4 o'clock, P. M.) it occupied both the palm and back of the hand. The cuticle was raised so as to resemble a bladder; the fluid contained was of a dark color, as is the case in blisters preceding mortification. The vesicle was punctured and the fluid allowed to escape.

The pulse was so exceedingly rapid that it was impossible to count it, and the child cried almost incessantly. As an external application, we used the common wheat flour, freely sifted over all the ulcers, and internally gave one sixth of a grain of calomel and one drop of the tinct. opii. camph. every four hours.

As soon as the raw surface was thoroughly covered with the flour, the cries of the child ceased and it soon passed into a quiet sleep, which lasted two hours, after which, she partook freely of a sucking bottle, which she would not do before. 14th, 8 o'clock, A. M. Our little patient rested pretty well until 12 o'clock, after that time she became very restless and has continued so until now.

The large vesicle on the left hand has progressed very rapidly towards the elbow; each of the pustulated points on the left arm and wrist is surrounded by a red margin, which progresses very rapidly under the cuticle, and presents the appearance of a vesicle caused by fire, the serum being evacuated. These local inflamed spots very soon meet and form one extensive surface extending in every direction with undiminished speed. At daylight this morning, a small vesicle was first observed at the root of the thumb nail on the right hand, and by the time I arrived there it had extended so rapidly as to occupy the whole hand. The cuticle slipped about but contained no fluid; all the pustules on the arm, and in fact, on the whole body presented the same inflamed appearance, the same disposition to extend and to commingle. The child is scarcely able to swallow the blandest fluids; the mucous membrane of the mouth, is much darker than yesterday. The same treatment to be continued. 4 o'clock P. M. Little patient continues to grow worse. The pustules on the back have taken on the same inflammatory action, and will now measure an inch and a half in diameter each. There is one pustule on the breast below the left nipple, and this is about all there is on the anterior part of the body, unconnected with the pustules on the extremities. The pustules on the face and neck have disappeared and in their stead is an erysipelas, occupying all the neck and lower portion of the face, which is gradually progressing up towards the eyes. The eruption on the lower extremities occupies a much larger space than at the last report, so also that on the

upper extremities. Two new spots have made their appearance, one on each heel. Patient can swallow some better, and has nursed from the bottle. Continue the same treatment, and anoint freely with sweet oil.

15th, 8 o'clock, A. M. Patient worse, the spots which appeared on the heels spread over the whole of the feet by midnight last. The eruption at all the parts affected continues to spread over the sound skin. That on the face has spread over the eyes, and is worse, if possible; can scarcely swallow, slept very little last night; during the first part of the night its bowels became very open, the discharges were at first very dark and of sufficient consistence, but soon becoming watery, they were arrested by an anodyne enema. The respiration has become very hurried; for a few moments it is so, then there is an entire cessation of respiration lasting generally about 15 seconds; then, as if by a convulsive action, the respiration is resumed again for a few moments, to be interrupted as before.—Wherever the eruption makes its appearance, the cuticle becomes separated from the true skin, and very great care is necessary in handling the child to prevent detaching it. The eruption on its legs has become very dark. Four o'clock P. M. Little patient is fast sinking; the eyes are much affected by the eruption, one is closed and the other so much affected as to present a very bad appearance. Patient died calmly at six o'clock; no examination was allowed.

REMARKS.—The object in publishing the foregoing case, is to call the attention of the profession to an anomalous disease; at least so far as my reading and observations extend. If not new to all, it may present points of interest to them.

First, we have a pustular eruption making its appearance on different parts of the body of a child seven days old; this state of things existing for six days, is followed by an erysipelatous eruption, if erysipelas it could be called, attacking the left hand and then the right, where there had been no former eruption, and finally invading parts previously affected with a pustular eruption.

So far as the treatment is concerned, I have but little to say. I did not believe it possible to produce effects upon the system of so young a child, sufficient to counteract such a disease.

ARTICLE V.

Remarkable Case of Amaurosis, illustrating the Anatomy of the Optic Nerves. By HENRY F. CAMPBELL, M. D., Demonstrator of Anatomy in the Medical College of Georgia.

The following very unusual case, we observed at Aiken last summer, in the person of a gentleman of about sixty years of age, and its history is as follows:—For many years he had been the subject of Myopia, which he thought was greater in the left than the right eye. His occupation being that of barrister, he had used his eyes perhaps imprudently, and for some time previous to our seeing him, he said that he had been led to think he was losing the sight of his right, or as he termed it, his best eye. At the time of our observation, he remarked frequently that he was very often unable to see at all with his right eye, and that when he caught a glimpse of objects they were such as were passing before him; but, as a general thing, vision was extinct in that eye. With the other eye, exactly the reverse obtained: here, the faculty, though much impaired in its distinctness, was still generally present, but occasionally he lost sight of objects for a moment, when they would reappear as they changed their position on the field of vision.

In order to test the correctness of his views in regard to his case, we passed the hand slowly before each of his eyes successively, the other being closed; on the left side, he could see the hand until it reached a certain point to the right, when it would suddenly disappear, but by continuing the movement it would become again visible. On the right side, the hand, on being passed as above, was *not* perceived till it had attained a point on the *left* exactly corresponding to the point on the *right*, at which he could not distinguish it. This experiment we repeated frequently and invariably with the same results.

To explain the very singular feature in this case, viz., that in the right eye vision was confined to a small portion of the retina, while the generality of this membrane was entirely amaurotic; and that at the same time the reverse obtained in the left eye, which had most of its retina sensible to luminous impressions, with only a small amaurotic spot, corresponding to the healthy spot in the amaurotic eye, we will review some of

the peculiarities in the anatomy of this important pair of nerves. Firstly, we know that the nervous filaments, which are to compose the optic nerves, arising on either side from the geniculate and quadrigeminal bodies, proceed through the optic tract to the chiasm. Here all of them, with the exception of a few fibres, cross over to constitute the optic nerve of the eye on the opposite side, into whose retina they are finally expanded, forming by far its greater portion ; but the few fibres which do *not cross* and only *approach* the chiasma, pass on with those from the opposite side to expand into the retina on the side from which they originate, yet from their paucity, they can supply only a very small portion of this membrane. And, secondly, the retina of each eye is produced out of fibres from both sides of the brain—consequently the destruction or injury of either nerve behind the chiasm would affect vision in both eyes, though much more extensively in the eye opposite to the tract injured. This is the fact illustrated in the present case.

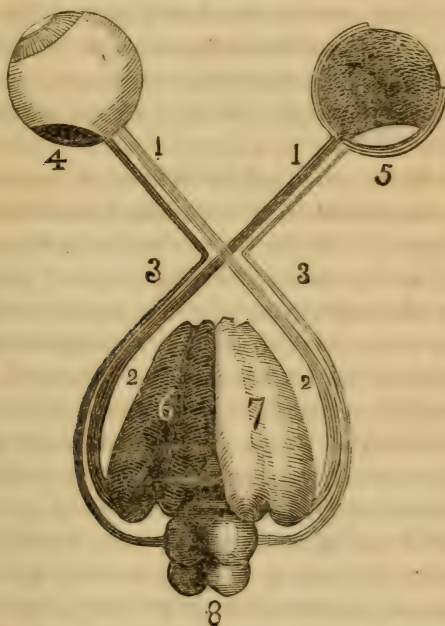
Cases of partial amaurosis, we find reported in treatises on diseases of this organ, but they were of an evanescent character, and did not, in all probability, depend upon any affection of the brain, but of the nerve—or perhaps only the retina itself. Dr. Wallaston's case, which occurred in his own person, is more analogous to the present than any within the range of our information. There, in looking at objects, but half of them could be descried ; he would see but half of a man's face and could read but half his name on a sign : thus, "JOHNSON" appeared "son", and in like manner every object he looked at. The affection subsided in a short time, and there was no recurrence of it till twenty years subsequently, when he was similarly affected.

Dr. Todd* refers to cases of partial amaurosis, which he says are very unusual, but they were all of a temporary character and in none of them was the fact of chiasm so plainly illustrated as in the one under consideration. Here the disease was enduring, the death of the nerve, functionally, at least, complete for a very considerable time, and full opportunity was allowed for a correct observation. Certain pathological observations find an explanation in our case : for instance, it is known that when the optic

* Cyclopædia of Anatomy and Physiology. Art. Optic Nerves.

nerves of an individual who had been blind in one eye for a considerable time previous to his death, are examined, the nerve of the healthy eye will be of fully its natural dimensions, while the optic tracts of both sides will be wasted, because they both contribute to the formation of the perished nerve. Here both retinæ are in a measure amaurotic, because both originate from a defective tractus opticus.

A reference to the following diagram will serve to elucidate our theory with regard to the extent and distribution of the amaurotic regions in each eye, as well as the probable locality of the disease from which it originated.



A figure illustrating the origin of the fibres from the two sides of the brain, and also their termination in the retinae. The dark parts of the cut indicate portions of the brain, nerves and retinae that are diseased.

- 1.1. The two optic nerves, a majority of whose fibres cross to the opposite eye.
- 2.2. The crossing fibres which ascend and expand, to form the greater portion of the retinae.
- 3.3. Convergent fibres which are few and ascend to form a small portion of the retinae of their own sides respectively.
4. Amaurotic spot on healthy retina.
5. Healthy spot on amaurotic eye.
6. Left thalamus dark, to represent the disease of which it is the probable seat; from it arises the paralytic nerve.
7. Healthy thalamus from the geniculated bodies of which arise the sound nerve.
8. Quadrigeminal bodies: the left represented dark, the right healthy.

Now, when we apply these pretty well established facts to the observations made in the above case, we find the anatomical account and the condition of the retinæ affording mutual corroboration. Thus the extensive amaurosis of the right eye corresponds with the extensive distribution of the paralytic nerve on that side; but the whole eye is not amaurotic, because the whole retina is not constituted by a diseased nerve, and therefore a sensible spot is found upon it. Nor yet, again, is the left retina wholly cognizant of rays, for a small portion of its extent owes its development to the few fibres that do not cross, but only approach the chiasm. Thus we find the distribution of the nerves accurately defined by the proportionate extent of the amaurosis in each eye. The cause of this condition we must necessarily infer to exist at some point in the optic tract or brain itself, posterior to the chiasma of these fibres.

Apart from the pathological interest investing the above case, it has importance in the relations it bears to the anatomy and physiology of this portion of the nervous system, which, even at this advanced state of science is still, in many points, the subject of some degree of doubt and uncertainty. Inasmuch as our means of studying the functions of these nerves are very limited, on account of the mutilation and disturbance of important superjacent parts necessary to arrive at their very obscure position, we are in a great measure deprived of the benefit of vivisections, and restricted in our investigations to post-mortem observations and pathological phenomena.

On a careful consideration of our case, we think the following facts in the anatomy of these nerves may be considered, in a great measure, corroborated by it: firstly, that the theory of chiasm in the fibres of the optic nerves, is correct, and also that each nerve is engaged in the production of the retina of both eyes; secondly, that the fibres are very unequally divided, one eye receiving by far the greater number; and, thirdly, that in their distribution to the retinæ the two sets of fibres, viz., the crossing and continuous, are not intermixed together, forming *all* parts of the retina, but are engaged in the production of separate and distinct regions of this membrane.

ARTICLE VI.

Case of Glossitis. By C. T. QUINTARD, M.D., of Roswell, Ga.

The following case, presents some peculiarities which call for its publication. On Tuesday, September 23d, was called in consultation, with Dr. P., on the case of J. F., who, I was informed, had been sick since the Friday previous. The following is the history of the case to date. About six weeks ago, a dentist, in attempting to extract one of the larger molars on the right side, broke off the crown, and left the root. For ten days a continuous pain was felt about the part; it then became intermittent until the 12th inst, when it ceased altogether. On the evening of the 10th, the pain returned,—the patient applied a few drops of ol. caryoph, and went to sleep. In the morning the pain became severe, and the tongue was slightly swollen. During the day (20th) he complained of pain in the back, and general malaise. In the evening Dr. P. visited the patient, prescribed a cathartic, and applied a blister about the anterior part of the neck.

Sept. 20th. Tongue, sub-lingual and sub-maxillary glands much swollen; pain severe; the masseter muscles rigid, particularly that of the right side.

22d. Made my first visit at 10 o'clock, A.M. Patient, a wagoner, aged 28; fine ruddy complexion, light hair, weighs 175 lbs., and is five feet one inch high. This morning there is an aggravation of all the symptoms. Patient unable to articulate; tongue protruded between the lips—is tense, red and painful to the touch; his breathing laborious; his brow bathed in perspiration; surface in other places hot; pulse 100, and full; saliva flowing profusely. He had passed a restless night, tossing to and fro on his bed, without any cessation of pain. No dejection since the operation of the cathartic administered on the 19th. Blood was at once abstracted to the amount of 25 oz.; a large dose of sal. epsom administered, and a poultice of hops and meal applied to the neck. Patient experienced considerable relief from the bleeding, and was able, after some effort, to swallow the salts. Not being able to remain with the patient, I advised the application of C. cups *ad nuchæ*, but no blood was drawn, as I learnt on my return at 6 P.M. Breath-

ing easier, tongue not so painful; salts have operated well. The pulse being about the same as at my former visit, again advised venesection, and twenty-five ounces more blood was drawn, together with six or eight ounces by cups under the clavicles. At 9 o'clock, there was a free discharge of fetid pus from an abscess at the base of the tongue.

23d. This morning found the patient sitting up, quite cheerful. No pain; tongue not so much swollen, nor so much lessened as was to have been expected from the discharge of pus which had continued through the night. It was still impossible to pass the finger back to the base of the tongue. Ordered only chicken broth. In the evening the discharge had ceased, but was renewed by using a probe.

24th. Patient slept well till towards morning, when there was a recurrence of pain. The right side of the tongue was now more swollen than the left, and as there was evidently another abscess forming, and the pulse again full and frequent, the patient was put on the use of half a grain of tart. emetic every two hours. 5 P. M. Has taken but two doses of the tartar, which acted freely on the bowels, as well as having produced the desired effect. At 2 o'clock the second abscess discharged an enormous quantity of offensive pus. Ordered a cup of strong green tea, and morph. gr. ss. to be taken at 8 o'clock.

25th. Patient better in every respect. Has some appetite, and considered convalescing. It is proper to state that scarification of the tongue had been attempted, but would not be submitted to by the patient.

Among the causes which predispose to glossitis are reckoned compression of the jugular veins, (Stahl); ptialism, (Slegel, Frank, Hosack); rubbing the head with mercurial ointment, small-pox, &c., (Trincavalleus). Among the occasional causes, are wounds, laceration, and contusions of the tongue. The application of emetic to the organ, burns; while the most frequent is the action of acrid or acro-narcotic substances on the tongue. "Such effects have been produced by the juice of the daphne mezereon, by tobacco leaves, and by the sting of wasps, bees and other insects." Dupont relates a case of a young man who, for a wager, "took two bites at a toad, and was speedily attacked with severe glossitis." (Vide Brit. and For. Med. Chir. Rev., July, 1850, p. 54.)

The treatment of this disease is generally simple, consisting of venesection, scarification of the tongue, or incisions made parallel to the raphe, the introduction of ice into the mouth, and if the patient can swallow, the administration of cathartics or laxatives. Emetics are highly commended by Dupont, Raggi, Wettengins and others, in the earlier stages.

ARTICLE VII.

Case of Puerperal Convulsions. By P. W. HARPER, M. D.,
of the Shoals of Ogeechee, Georgia.

On the 12th of last November, I was called at 9, P. M., to visit Mrs. L., aged 18 years, complexion very fair, of a healthy and strong constitution, with her first child. Parturition had commenced some twelve or fifteen hours previously to my seeing her. I learned she had had two violent convulsions, and while I was conversing with her, one came on that lasted several minutes. I bled her while convulsed, until the pulse yielded, say from twelve to sixteen ounces. This composed her for an hour, then another convulsion came on, though not so violent nor so long as the other. I opened the vein and took about eight ounces of blood. This reduced the pulse considerably, and I had hoped would put a stop to the fits. At twelve o'clock, she had another convulsion as violent as any of the others. I opened the vein again, and took, say twelve ounces more of blood, pulse weak and 120 per minute. During all this time, labor steadily progressed, and at one o'clock in the morning, she was delivered of a very large child, though dead. This was what I expected. From the first to the last, she was the most ungovernable patient I have ever attended, and during her convulsions, it took several persons to keep her confined to her bed. After being composed for two hours, she had another convulsion, though slight. I gave her a tea spoonful of paregoric which made her rest well for a while. At four o'clock, she had another convulsion, though mild. I gave her a tea spoonful of laudanum, which composed her until eleven o'clock that morning, when she awoke perfectly in her senses, but not recollecting what had happened not even my visit and attendance on her. She had no other

convulsions afterwards. After giving the usual directions in such cases, I left her in the afternoon much better than I could have expected.

14th. Found that the *uterus* had not contracted, with great soreness extending from the *pubis* to the *umbilicus*. I directed a dose of castor oil, tepid injections into the *uterus* and emollient applications over the abdomen. 15th. No better, complains of great soreness in the abdominal region, much swollen and very hard. Applied a large blister which drew well.

16th. Swelling about the same, with great soreness, pulse weak and 120—opened the bowels with castor oil, and opium given at night to compose her. 17th. Rested well; swelling as great, soreness and hardness not so much as the day before; the same directions continued. 18th. Blister looks well; no change in the swelling, &c.; complains of numbness extending from the left hip down the thigh, leg and foot. Directed her bowels to be opened with the saline purgative mixture, composed of epsom salts, 2 oz; cream tartar, 1 oz; tartar emetic, 2 gr; water, 12 ounces. One wine glassfull to be taken every three hours until the bowels are well opened. Tepid injections still continued, and one grain of opium at bed time.

19th. But little alteration in the general appearance of the abdomen, left leg and foot very much swollen and very painful, particularly to the touch, no pain in the limbs. Directed laudanum and spirits of camphor, in equal parts to be rubbed over, and sulphate of morphine at night. 20th. Swelling of the abdomen nearly the same, though not as much soreness and hardness; leg and foot pretty much the same. Directed the saline purge, the same external applications together with the tepid injections and morphine. 21st. Much better except her leg and foot, which are very much swollen and very painful. The same directions, with the omission of the purgative. 22d. The blister looks well and the case assumes a more favorable aspect. Directed a dose of oil, the same applications and morphine. 24th., Still improving; blister nearly well, with very little soreness and pain. Bowels opened with oil and the same remedies continued. 27th. Recovering rapidly; the same course continued. December 2d. so much improved as to discontinue my visits.

PART II.

Eclectic Department.

A Synopsis of the Spinal System. Being the Croonian Lectures, delivered at the Royal college of Physicians, London.
By MARSHALL HALL, M. D., F. R. S., &c., &c.

LECTURE II.

GENTLEMEN,—In my former lecture, I described, or rather demonstrated the diastaltic law of action of the *vis nervosa* of Haller, and the diastaltic nervous arc in anatomy.

I showed you, by means of experiments and diagrams, that though the action of the *vis nervosa* is from above downwards in all preceding experiments, yet, that in newly devised experiments, that action is first from below upwards, or from without inwards, and that it is then reflected by the spinal centre from above downwards or from within outwards, either along the *same* or *other* nerves, to the muscles of the same and other limbs, establishing another, or panthodic law of action of this singular vital power.

This diastaltic law of action of the *vis nervosa* is portrayed in these diagrams, to which I beg again to direct your attention:—

The action is in the direction of the spinal marrow and nerves, *towards* and to the muscles. It illustrates the *facts* and the *Law* of Haller. The action is first *to*, and then *from* the spinal marrow. It is the *fact* of Redi, Whytt, &c. The action is *both*. It is the demonstration of the *identity* of the principle of action in both, and of a new *Law* of Action of that principle.

From one and the same point of irritation or excitation in one limb or part of the frog, the stream of power may be sent in all directions, upwards or downwards, into *all* the other limbs, just as we observe in traumatic tetanus, wherever the wound may be, in the hand or in the foot, the muscles of the maxillae, of the neck, of the back and abdomen, and of all the limbs, are thrown into tetanic spasm in the human subject, as in animals. The action is panthodic.

This, gentlemen, is the nearest approach to a *circulation* in the nervous system. The course of the streams of nervous power is not a perfect circle, but it is an arc or arcs, very nearly approaching to the circle, and broken only by the minute space between the integument and the subjacent muscular fibre, when the same limb is affected by excitation and movement.

It is by the law of diastaltic action of the *vis nervosa* that the experimental facts of Haller are identified with those of

Redi, Whytt, &c. But, what is far more important, it is by the discovery of this law of nervous dynamics that the *vis nervosa* of Haller becomes capable of application, for the first time, to physiology, to the functions of the animal economy.

Previously, the facts of Haller and of Redi, were mere objects of experimental lore or curiosity, sterile, and without application or utility. I was persuaded, at a glance, that this could not be. A dynamic must have its use. Nature does not expend itself in the mere production of useless power. Wherever a power exists its application exists, and that application must be *sought for*. The *vis nervosa*, or the power or dynamic in the spinal nervous system, is such a power. It was without application. It is now of most extensive application. It is the active or controlling agent in all the acts of exclusion, of ingestion, of retention, of expulsion, in the animal economy!

What is the *nature* of this surprising power? Of this, at present, we know *nothing*! But we know what it is *not*. We know that it is *not* sensation or volition, and that it is *not* electricity in any of its known forms or modifications. We know that its seat is the spinal or diastaltic system *exclusively* of the cerebrum and cerebellum, and in a certain sense, of the ganglionic system.

The actions of this power are always in diastaltic nervous arcs, consisting of an esodic nerve, the spinal centre, and an exodic nerve in *essential* connexion and relation with each other—a new fact and principle in anatomy, and represented in its simplest forms, in this diagram of the Triton.

Each of these four portions of the animal presents the phenomenon of a diastaltic nervous arc. In the first you have the trifacial, in the frog, in essential connexion, through the spinal centre with the facial; if you excite the *border* of the eyelid, the eyelid closes; the other eyelid closes simultaneously. You have therefore a *double* diastaltic arc—from the border of the eyelid to the orbicularis, of the same and of the opposite side. Similar facts and phenomena are traceable in this, the second portion of the diagram, in reference to the nerves and muscles of the anterior extremities, in this third, in reference to the lower extremities, and in this fourth, in the tail.

But I hasten to call your attention to the same, and other similar phenomena in the human subject. In this beautiful diagram you have the diastaltic nervous arc of the human eyelid. From the border of the eyelid, I trace a branch of the trifacial to the medulla oblongata; from this last, I trace the facial to the orbicularis. If, in a patient affected with coma and a gaping eye, you excite the border of the eyelid, it instantly closes. The action is produced through this diastaltic

nervous arc. This fact I have frequently observed in apoplexy, in hydrocephalus. The *degree* of diminution of the diastaltic action affords a measure of the degree of danger!

In this second diagram, I have represented the diastaltic nervous arc of the larynx. Along this superior laryngeal nerve the energy of the vis nervosa proceeds to the medulla oblongata; and thence along this inferior and recurrent laryngea, the same energy proceeds to the muscles which close the larynx. Every excited closure of the larynx is of this kind. If a crumb of bread or a drop of water falls on the border of the glottis, this organ is forcibly, violently closed, by the diastaltic action of the vis nervosa, through this anatomical diastaltic arc.

Still more earnestly I beg to call your attention to the next diagram.

It represents, for the first time, the natures, cause, and mode of action in the vital function of Respiration.

The first inspiration, as the acts of inspiration in peculiar circumstances of asphyxia or syncope in after life, is excited by the contact of the cool atmospheric air with the origins of these the trifacial, or these the spinal nerves, in the cutis of the face and general surface. Rhythmic respiration is excited by an internal stimulus acting on the origins of an internal excitor of respiration—the pneumogastric. As the pneumonic circulation proceeds, the blood exhales carbonic acid in the air-cells of the lungs; this irritating gas excites the origins of the pneumogastric nerves in those cells, and inspiration and a concatenated expiration (as in sneezing) are effected. The same series of phenomena is repeated, and this with a rapidity in a direct *ratio* with that of the circulation.

This ratio is thus explained: during sleep the circulation is as slow as possible. The evolution of carbonic acid, and the number of respirations in a given time, are proportionately small. Let the circulation be rendered rapid by activity, by the acceleration of the motion of the blood in the veins by muscular action, and the evolution of carbonic acid and the excitation of respiration are proportionately augmented.

In this manner the *ratio* between the circulation and the respiration is strictly maintained. It is physiological. Whenever it ceases, the phenomenon is one of pathology—an event particularly apt to occur in diseases of the encephalon, in which with comatose affection, the respiration is apt to become morbidly slow, irregular, suspicious, stertorous, &c.

Such is the nature and importance of the *Diastaltic Arc or Arcs of Respiration*.

The actions of this power are distinct from all actions of *volition* or of *emotion* or *pain*, though they are frequently

mingled with, and modified by them, the first of these occupying the upper part of the cerebral system, the second the lower, and the third the lowest, with the ganglionic.

The *disinction* between the spinal system or the diastaltic nervous system from all these is *absolute*, though their *union* in the general nervous system is most intimate. It is only in the latter restricted sense that we can any longer speak of the *cerebro-spinal axis*. we *may* speak of the *cerebro-spinal axes*, for this structure *embraces* the cerebral *and* the spinal axes or centres.

Each diastaltic nervous arc is actuated by the *vis nervosa*; we must no longer speak in regard to the diastaltic action or closure of the eyelids, or of the larynx, for example, as even the late able and otherwise accurate Professor J. Reid has done, of the "*sentient*" and of the motor parts of this arc; or as Prochaska (of whom so much has been, rather malignantly, I fear, than ignorantly, and, at any rate, untruthfully, written) does, when he speaks of "*impressionum sensoriarum motorias reflexio*," stumbling at the very threshold.

You will still, gentlemen, hear much of Unzer and the author whom I have just quoted. The whole idea of "*anticipation*" by this author is a fiction and a falsehood, totally unworthy of further notice, and totally unworthy of our profession. Unzer did not proceed beyond considering the spinal marrow or centre as a "*chord of nerves*," *excluding* in one word, all that I have said to you; and his pupil, Prochaska, had not even the initiative idea of a diastaltic arc in anatomy, in vital dynamics, in physiology, or in any sense whatever. They and all who have followed them, remote or recent, have—the former ignorantly, the latter more culpably—erred at the very threshold of this *new* department of anatomy, physiology, and pathology.

But, to quit this discreditable theme I must proceed to state to you two important *principles* or *facts*:

The *first*, that the *vis nervosa* usually exists in the form of mere *static equilibrium*. It requires in every instance, a distinct *excitant* to rouse into *dynamic force*, *action*, or *act*. Its agency is therefore, unlike that of volition, *never spontaneous*.

The *second*, that, in the spinal centre, but *not* in the exodic nerves, and, I suppose, *not* in the esodic nerves, the *vis nervosa*, or the "*excitabilité*" of M. Flourens, admits of distinct *augmentation* and other abnormal conditions.

These facts are portrayed in a diagram, which will be given in the second part of this lecture.

The *in-excitor* property of the cerebrum and cerebellum; the *excitor* property of the medulla oblongata and medulla spin-

alis, with its susceptibility of augmented excitability, and the excitor power of the exodic nerves, with *in*-susceptibility of augmented excitability, are all displayed, and placed, as it were, before the eye in this diagram in a manner not easily to be forgotten.

I proceed to discuss this important topic particularly.

Condition of the Vis Nervosa ; Static and Dynamic.

The cerebrum and the cerebellum are insensible and *in*-excitor or *a*-static, on being punctured or lacerated, whilst their principle of action, the $\psi\chi\eta$, is *spontaneous* in its motor influences.

The spinal marrow, on the contrary, is essentially excitor, requiring the application and repetition of a stimulus for the development of each and every movement.

The natural condition of the spinal marrow is one of inaction, or of static equilibrium. It is by appropriate and successive stimuli that its dynamic force is made effective and manifest.

This statement is true in every condition of the spinal marrow. Even when its excitability is extreme, under the influence of strychnine, freedom from stimulus is freedom from all motor action.

Still more is this the case in the state of diminished excitability from *shock*, from chloroform, &c.

After the application of a stimulus and the phenomena of dynamic force, the spinal marrow again resumes its condition of static equilibrium, but with reduced excitability. The action of each stimulus is followed by this effect, and each second stimulus is accordingly less effective than the former one. The excitability is, on the other hand, restored by repose. And thus the static equilibrium and the dynamic force bear a certain relation to each other.

A frog, affected by shock, or placed under the influence of chloroform, may be deprived of voluntary movement, respiratory movements, and reflex actions, the circulation being also almost extinct. If it be now left at rest, respiratory movements return. If it be excited, they again cease. And thus repeatedly. The same observation applies to all other movements. Quiet is the restorer, excitement the exhaustor, of the motor energies.

The Spinal Marrow susceptible of augmented Excitability.

The degree of Excitability of the spinal marrow is, in general terms (like irritability of the muscular fibre,) inversely as the degree of activity or of stimulus.

Augmented or restored during sleep, it is diminished during each day, by every act of volition, every act of the respiration, and by each meal.

But the excitability of the spinal marrow admits of intense augmentation and extreme diminution by therapeutic agents.—That of the nerve admits of no such augmentation.

Exp.—The tenth part of a grain of the acetate of strychnine dissolved in distilled water, and applied over the cutaneous surface of the frog, induces the most extreme excitability, or hyperethism. The slightest stimulus induces violent tetanoid spasm. Meantime, the circulation, in the intervals of such spasms, remains unimpaired.

Exp.—On the other hand, if ten drops of chloroform be dropped on a bit of spong and attached to the upper part of a tumbler, and this be inverted on a plate of glass, so as to enclose a frog, this animal first ceases from voluntary movements, then loses its excitability, and lastly, its circulation.

Undue excitability is generally the effect of teething, of irritated esodic nerves in general, and especially in the case of a wounded nerve, as in tetanus.

The usual immediate effect of a convulsive seizure is augmented excitability; and therefore one seizure frequently succeeds to another. The remoter effect is diminished excitability, and the patient is frequently secure from other attacks until the excitability is slowly restored.

Indolence allows the excitability to become morbidly great; activity diminishes its degree or intensity. Hence the importance, in such cases, of restraining the excitability by daily exercise, limited only by approaching fatigue.

Relation of Irritability of the Cerebrum and Spinal Marrow.

We are naturally led by the consideration given in the last paragraph to the subject of the present one. Every act of an organ is followed by diminished energy or power. This is not only true of the nervous tissue, but of the muscular fibre.—Each contraction of a muscle is followed by a diminution of the irritability of the muscular fibre. If, on the contrary, all stimulus be removed, the irritability exists in its maximum degree.

But, for the perfect state of the muscular irritability, it is essential that the muscle should have remained in connexion through the nerves, with the spinal marrow. *The spinal marrow is, so far, the source of muscular irritability.*

If, in experiment or disease, the influence of the brain, that is, of volition, be withdrawn from a muscle, its irritability becomes greater, comparatively, than that of the similar muscles. In cerebral paralysis, or that paralysis in which the influence of

the cerebrum is removed from a limb, the muscles of that limb are more irritable, *tested* by the *mildest* galvanic influence which will produce an obvious effect, than those of the other limb.

But if the connexion between the spinal marrow and the muscle be severed, either in experiment or by disease, the irritability of the muscles of the paralyzed limb (and the excitability of the severed portion of nerve) is less than that of the healthy limb.

These conclusions are founded upon a vast number of experiments, most carefully made and observed.

The fact affords a *Diagnosis* between cerebral and spinal paralysis, or between the cases of paralysis in which the influence of the *cerebrum* or of the *spinal marrow* is severed, respectively—a diagnosis frequently of great importance.*

Relation of Excitability and Irritability to Stimuli.

The chief stimulants of the animal frame are the acts of volition, and what are in exact proportion to these, heat, food, and air. The excitability of the nervous system, and the irritability of the muscular, are inversely proportionate to these stimuli.

This *Law* of the *Inverse Ratio* prevails throughout animated nature, and is, perhaps, the most general of all. It was announced by me nearly twenty years ago, in the *Philosophical Transactions*.

During activity, the stimuli are all augmented; the excitability and irritability are proportionately diminished. During sleep the reverse obtains; the stimuli are at their minimum, the excitability and irritability are at their maximum.

Exp.—Having removed the head of a frog, we separated every part of the animal, leaving only a portion of the spinal marrow in connexion with the denuded and separated lumbar nerves, and the lower extremities deprived of integument.—We passed a galvanic current through the nerve and limb, until the movements had nearly ceased. We then passed a very mild current equally along both lumbar nerves, excluding the muscles; and then a stronger current equally through the muscles of both limbs, excluding the nerves; we found that the *excitability* of the nerve and the *irritability* of the muscles had been *alike* reduced by the repeated action of the stimulus.

A frog, prepared so as to expose the nerve in connexion with the muscles, has been designated "*galvanoscopic*." Galvanism is, in its turn, the *Test* of the excitability of the nervous, and

* Vide the Medico-Chirurgical Transactions, vol. xxii. xxxi., and The Lancet and the London Journal of Medicine, for 1849.

of the irritability of the muscular, fibre. So tested, these properties are found to be greater as we descend in the zoological scale, whilst the quantity of stimulus—food, respiration, temperature—is known to be less, in the same ratio, but inversely.

These facts, these principles, are the foundation of the *pathology* and the *therapeutics* of the diastalic system. In various maladies, as epilepsy and tetanus, we have augmented excitability of the spinal centre; in *all diastaltic* actions of remedies, it is the principle of renewed excitant or of *alternation* which is our guide in practice. It is this principle, the principle of alternation in the application of temperature, of relative cold and heat, &c., which should be our guide in the treatment of asphyxia.

There is a *third* principle of action in regard to the vis nervosa, which I will merely mention in this place, for it still requires investigation. A patient, once the subject of epilepsy, is peculiarly liable to a return of the malady; augmented susceptibility seems to have been superinduced. But the patient who is liable to epilepsy seems to lose this susceptibility *for a time* immediately after each attack, as if the susceptibility had, for that period, been diminished or exhausted, the interval of comparative security being unlike in any two instances.

All attacks depend upon these principles of repetition of *excitants* and of alteration of *excitability*.

I now, gentlemen, proceed to illustrate these principles by experiment:—

I have here a frog from which the cerebrum and cerebellum have been extracted, and on the skin of which I have dropped five drops of a solution of the acetate of strychnine, or the eighth part of a grain of that terrific poison.

In five minutes the animal is brought under its influence.—Now, let us carefully examine the extraordinary effect. Whilst it is left alone, untouched, unshaken, absolutely unexcited, it lies tranquil, as if nothing had occurred to it. But observe the extraordinary effect of a jar given to the plate or the table. It is thrown into a state of rigid tetanoid spasm; all its limbs are violently extended and agitated. Now the paroxysm is over; it has sunk into a state of relaxation. It would remain in this state until, if previously unmutilated, it recovered, or, being mutilated by the removal of the brain, it ceased to live! No excitement, no tetanus!

A thousand ideas rush upon the mind on viewing and contemplating this extraordinary scene!

The first appearance which strikes the observer is the difference of position assumed by the anterior extremities of the male

and female frog; those of the former being arched over the thorax, those of the latter placed in straight lines along the sides of this region. These positions result from the difference of development of the nerves and muscles of these limbs in the two sexes, especially in the early part of spring. The fact displays the special action of the spinal system, so similar to design in various cases.

The second fact is of still greater interest. The animal remains perfectly motionless unless it be *excited*. The *vis nervosa* is in a state of static equilibrium, unless that equilibrium be disturbed and changed into dynamic force by some cause of excitation. But if I jar the plate, or the table, or the floor even; observe the effect—sudden rigid tetanus!

The animal has now resumed its relaxed condition. This it will retain until a fresh cause of excitation is applied.

All this is an effect of the spinal centre, the centre of the spinal or diastaltic system. The cerebrum has been removed. The viscera *may* be removed without interference with these phenomena.

But observe this singular fact: if the integuments be stripped from the foot, no irritation of the toe has any influence. The *origins* of the esodic excitator nerves have been removed, with the other cutaneous tissues.

The same effect results from dividing the nerve which proceeds from the foot towards the spinal centre in any part of its course.

Lastly, a similar effect is instantly induced by destroying the corresponding portion of the spinal centre itself.

This law is uniform, in pathology as in physiology. Destroy any part of the diastaltic arc, and its phenomena cease.

I have now a remark to make of great importance. You have seen that this tetanoid condition exists independently of the cerebrum and cerebellum, which were removed in the first instance in the experiment which I have laid before you. I have also shown you that, *after* the induction of the effect of the strychnine, all the viscera may be removed, without removing the effect. This condition exists, therefore, independently of the cerebrum and of the ganglionic system.

It consists in exaltation of the excitability.

Now this exalted state of the excitability, or *vis nervosa*, is limited to the *centre* of the diastaltic system, and is not extended to its nerves. As long as the femoral nerve remains attached to the spinal centre, in the tetanoid state induced by strychnine, the muscles partake of the rigid spasm excited by any irritation. But let the nerve be divided, and let its lower portion be irritated, and it is found to possess the normal degree of excitability.

The excess of this excitability, then, is restricted to the centre of the system, exclusively of its nerves.

The centre and the nerves—the exodic nerves at least—are both endowed with excitability, but the former alone is endowed with the power of taking an augmented or exalted excitability.

This principle doubtless prevails in disease. In teething, in epileptoid disease, in tetanus, in hydrophobia, the spinal marrow is in this condition. Hence the value of sinapisms and liniments well applied along the whole course, and especially the upper part of the spine, in such cases.

The effects of strychnine present the *type* of hydrophobia. They consist of augmented excitability, originating, like hydrophobia, in a poison applied by the blood to the spinal centre, or the centre of the diastaltic system. Now, gentlemen, a frog so affected by strychnine—so made the subject of a tetanoid condition—*recovers*, if it be placed in a little cool water, and left, absolutely undisturbed, in a cool place! It *dies speedily* if continually stimulated even by the touch of a feather! Quiescence cures, whilst each excited tetanoid spasm exhausts the vital power!

Do not these facts present invaluable suggestions for the treatment of the class of diseases involving exalted excitability? Might not the hydrophobic patient even, who infallibly dies if exposed to sources of excitement, survive if it were possible to preserve him from all excitement *absolutely*? One thing is certain, the physiological facts and principles which I have unfolded suggest the *principles* on which all our *treatment* is to be conducted.

An interesting question presents itself. How are poisons eliminated from the system? or, on what principle do their effects subside? Some facts, which it would be out of place to detail on this occasion, lead me to think that some poisons, whilst they are removed in all the secretions, are especially separated by respiration. The subject is full of interest, and calls for investigation.

I may here ask another interesting question. What is the difference between the phenomena of hydrophobia, which is a poisoned condition of the blood, and tetanus, which results from injury of an esodic nerve? The *origin* of the two diseases is essentially different. Are the phenomena so too; and in what degree, and in what respect?

It has been shown that the tetanoid state induced by strychnine is one of poisoned blood, acting on the spinal centre, inducing there exalted excitability, but not necessarily, or without an excitant, a state of tetanus or spasm. The case is *tetanode*, a state *full* of tetanus, without being tetanic.

I have now to state that tetanus—traumatic tetanus—is more than a mere augmented or exalted excitability. There is, in addition, a constant *excitant* in the wounded irritated nerve. There is therefore constant spasm. But there is, also, exalted excitability, and this spasm is exasperated in paroxysms on the application of any other excitation.

The effects of strychnine, hydrophobia, and other congeneric affections resulting from poisoned blood, are *inter-mittent*; tetanus is *re-mittent* only!

Teething, and all those cases of epileptoid disease in which the chief exciting cause, though it be an excitant of the nerve, is *intermittent*, also leads to an *intermittent* form of disease.

Have I convinced you, gentlemen, by these observations, of the *value* of these investigations in *practice*? Have I not put into your hands the clue of Ariadne, to lead and guide you through this labyrinth?

In my next lecture, I propose to bring before you two most important pathological laws, to which I, at least, can detect no exception:

The first—that no disease of the cerebrum or cerebellum *can* induce *spasm*, except through excitation, by contact or counter-pressure, of the spinal system.

The second—that no disease of this system, wherever situated if limited to this system, *can* affect the cerebrum, except through the nerves, and muscles, and veins, of “The Neck”—that *medical region* to which I have recently, and earnestly, called the attention of the profession.

Before I close the present lecture, I beg to notice a remark which has been made to me, upon a statement made in my former one. It was said that I had done injustice to the late Professor J. Reid, when I stated that he had spoken of the action of the superior laryngeal nerve, in the excited closure of the larynx, as being “sentient.”

Nothing would grieve me so much as to misrepresent and depreciate the opinions of any of my professional brethren.—*This I have never done.* I cannot say that I have never been the subject of misrepresentation and depreciation; for, as I have already said, the race of the Primeroses and of the Parisanuses is not yet extinct amongst us. But most of all I should regret any such act on my part towards Professor J. Reid, whose memory I respect, and whose labours I place in the very first rank in physiology; for if they do not rise so high as to be entitled to the designation of *discovery*, they certainly present a series of new and invaluable results, and especially the papers on the pneumogastric nerve. But I will read to you the paragraph, which I quoted from memory in my last lecture. You will perceive

that the very term sentient, or at least "sensitive," is used as I quoted it.

"When any irritation is applied to the mucous membrane of the larynx, in the healthy state, this does not excite those contractions of the muscles that approximate the arytenoid cartilages, by acting directly upon them, through the mucous membrane, but this contraction takes place indirectly, and by a reflex action, in the performance of which the superior laryngeals act as the *sensitive*, or afferent nerves, and the inferior laryngeals as the motor or efferent nerves."—Dr. J. Reid's "Researches," 1848, p. 251-2.)

The last proof that the phenomena in question do *not* depend on sensation, if such proof were required, which it is not, is afforded by the testimony of patients afflicted with paraplegia. When this malady is complete—when, as in a case which I recently attended with Mr. Edwards, of Queen street, Cheapside, the spinal marrow is absolutely *divided* by the disease,—when *all* sensation and all voluntary motion are extinct—these diastaltic actions exist in their full force, the patient *seeing* the movements induced, but not having the slightest power to *feel* or to *control* them.

I have witnessed many cases of the same kind. The proof is absolute, and the lengthy controversy on this point may be considered as terminated.

I have been favored by another criticism from a fellow of this college, whom I beg leave to thank, both for the kind terms in which he has spoken of my former lecture, and for his able and learned suggestion. The latter relates to the term which I employed at that lecture. Of opinion that the term "diastaltic" is a happy substitute for the former term "reflex," this friendly writer suggests the use of other compounds of $\sigma\tau\epsilon\lambda\lambda\epsilon\iota\nu$ in the place of the terms esodic, exodic, &c. It would certainly be desirable to preserve uniformity in our nomenclature; and the kind suggestion, for which I beg to offer my sincere thanks shall receive my most mature consideration. But I think both terms must be preserved; for example I do not see how we could express by any compound of $\sigma\tau\epsilon\lambda\lambda\epsilon\iota\nu$ the idea conveyed by the term panthodic.

On Stricture of the Urethra, and Fistula in Perineo. By JAMES SYME, F.R.S.E., &c., &c. Edinburgh, Sutherland and Knox. 1849. 8vo., pp. 72.

Stricture of the Urethra, its Pathology and Treatment; comprising Observations on the Curative Powers of Potassa Fusa in that Disease; with Cases. By ROBERT WADE, F.R.C.S., &c., &c. Second Edition. London, Churchill. 1849. 8vo., pp. 247.

When a metropolitan surgeon, of such high reputation as Mr. Syme has earned for himself, comes before his professional brethren with a new plan of treatment for a frequent and important disease, his proposal calls for serious consideration; and it is with this feeling that we have taken up Mr. Syme's essay on strictures of the urethra and fistulæ in perineo. In the preface he remarks,

"That the method of treating obstinate strictures of the urethra recommended in the following pages was communicated to the profession, five years ago, through the periodical press, and again, two years ago, in a collection of surgical essays; but, so far as I know, it has not as yet been adopted by others, even in a single instance. Being deeply impressed with the importance of the subject, I feel it my duty to make another attempt, with the view of awakening attention to it, by publishing, in a separate form, full details of the procedure, together with its advantages, positive and comparative; and also further evidence of its efficacy, from cases in public as well as private practice. Having done this, I leave the matter to the profession, trusting that, whatever may be their decision, they will at least give me credit for an earnest desire to render the opportunities committed to me conducive to the improvement of practical surgery."

Mr. Syme commences his observations by remarking, that the occurrence in surgical practice of cases in which strictures of the urethra have existed for the greater part of a lifetime, notwithstanding the efforts to remove them by practitioners of the greatest skill and experience, evidently shows that the means of treatment hitherto employed must either be uncertain in their operation, or only temporary in their beneficial effect; and adds, that his object now is to explain and recommend a method of treatment which has been found an effectual remedy for the disease, even in its most inveterate forms.

In referring to the obstinate cases of permeable stricture, in which his new mode of operation is recommended,—for his observations do not at all apply to what are called, and, as he says, improperly, impermeable strictures,—Mr. Syme says:

“I do not here allude so much to the mere tightness of contraction, and the difficulty consequently experienced in passing a small instrument through the stricture, as to the unyielding disposition manifested by the constricted canal, and its tendency to contract, perhaps even more closely than before, after being partially or completely dilated. One other feature of such obstinate cases, of great importance to notice, is the strong and general sympathy of the system with every change taking place in the local disease; when rigors and febrile attacks, leading to various derangements in different parts of the body, more or less connected with the part locally affected, are apt to result from attempts, even of the most gentle kind, to pass instruments into the bladder.”

The following is Mr. Syme's description of his operation:

“If the patient has a great deal of pain, and wishes to escape from the slight degree of it which attends the requisite incision, he should be placed under the influence of chloroform; not partially, so as merely to suspend his consciousness, or impede his recollection of suffering, but completely, so as to prevent any restlessness or unruly struggle, which would tend very seriously to increase the difficulty of the procedure. He should then be brought to the edge of his bed, and have his limbs supported by two assistants, one of them standing on each side. A grooved director, slightly curved, and small enough to pass readily through the stricture, is next introduced, and confided to one of the assistants. The surgeon, sitting, or kneeling on one knee, now makes an incision in the middle line of the perinæum or penis, wherever the stricture is seated. It should be about an inch, or an inch and a half, in length, and extend through the integuments, together with the subjacent textures, exterior to the urethra. The operator then, taking the handle of the director in his left, and the knife (which should be a small, straight bistoury) in his right hand, feels with his fore-finger, guarding the blade, for the director, and pushes the point into the groove behind, or on the bladder side of the stricture, runs the knife forward so as to divide the whole of the thickened texture at the contracted part of the canal, and withdraws the director. Finally a No. 7 or 8 silver catheter is introduced into the bladder, and retained by a suitable arrangement of tapes, with a plug to prevent trouble from the discharge of urine.

“The patient has merely to remain quietly in bed for forty-eight hours, when the catheter should be withdrawn. The urine sometimes maintains its proper course from the first, but more frequently passes in part through the wound for some hours; no attention or interference is required on this account, but at the end of eight or ten days a moderate-sized bougie should be passed, and repeated once a week or fortnight, for two months.”

Mr. Syme recommends the same perineal incision in cases of obstinate stricture, accompanied by fistulæ in perineo, and gives two cases treated in that way.

He details eleven cases of permeable stricture, which he treated by external incision, and says :

“That of all the cases in which I have divided the stricture, only one has been followed by any unpleasant result ; on that occasion the patient suffered from a formidable attack of erysipelas, which, commencing in the perinæum, gradually extended over the whole surface of the body, accompanied by constitutional disturbance, so violent as to prove all but fatal, and productive of emaciation, with prostration of strength, to an extreme degree. During this illness, the wound, instead of healing as usual, remained open for several weeks, just as when first inflicted, and it retained its conical form after the process of granulating contraction began, so that when the cicatrization was at length completed, the urethra had a very thin covering at the seat of the aperture, which, therefore, was apt to open from time to time, and discharge a little urine. It may be added, that the combination of circumstances which gave rise to this untoward occurrence was so complicated and unusual that it can hardly by any possibility happen again.”

Having reviewed the different methods hitherto proposed for the treatment of stricture of the urethra, Mr. Syme concludes the subject in the following manner :

“From what has been said in the foregoing pages, I trust it will appear established,

“First. That division of a stricture by external incision is sufficient for the complete remedy of the disease in its most inveterate and obstinate form.

“Second. That in cases of less obstinacy, but still requiring the frequent use of bougies, division is preferable to dilatation as affording relief more speedily, permanently and safely.”

We confess that we were rather startled when we read the foregoing sweeping conclusions, at which Mr. Syme has arrived, in favour of the treatment of passable strictures of the urethra by external incision. That a free division of a cartilaginous stricture will enable the surgeon to pass a large-sized catheter on into the bladder, and thereby afford him an opportunity of finishing the treatment by dilatation, we at once admit ; but we are at a loss to understand on what principle the mere longitudinal division of an adventitious texture, which very frequently surrounds the urethral canal, and the nature of which we know from experience to be that of having an extraordinary tendency to contract, can lead to a permanent cure. Mr. Syme says that no attention or interference is required, on account of the urine passing out through the wound for a few hours or days ; but at the same time he states that at the end of eight or ten days a moderate-sized instrument should be passed, and repeated at intervals for two months ; and, as if not

quite satisfied himself as to the permanency of the cure by external incision, he says at p. 43:

"In most cases the cure may then be deemed complete and lasting; but if the tendency to contraction should have been extreme, or if the patient's way of life should be such as to favour the reproduction of the stricture, it will be a prudent precaution to have the bougie passed four or five times in the course of a year, in order to avoid all risk of future trouble."

We must say, after a very careful perusal of Mr. Syme's essay, that we are far from being convinced of the advantages of the treatment by external incision over dilatation in cases of permeable stricture of the urethra; and as the *permanency* of the cure by the perineal incision is the great point on which Mr. Syme lays stress, we should have wished him to have followed out each case, so as to have proved the *remote* results of his operation; for, with the exception of two, all of his cases have been operated on within the year, many but a few months since; and his reports of the great bulk of his cases cease with their dismissal from hospital, or, in private practice, within a few days after the operation was performed. The following is a report of the last of Mr. Syme's cases, which will give an idea of the summary manner in which the cases in general are dealt with, and which we conceive has left the boasted advantage of Mr. Syme's treatment, viz., its permanency, unproved.

"CASE XI.—A. S., aged 28, a book-binder, was admitted into the Royal Infirmary, on the 29th of July last, for stricture of the urethra at the bulb, complicated by a false passage, as stated in the recommendation of a medical man which he brought with him. The complaint was attributed to a gonorrhœa contracted ten years ago, and had been very troublesome for the last five years, impeding micturition so much, that the urine frequently could not be voided except by drops, and occasionally causing complete retention. It was through ineffectual attempts to afford relief during these attacks, by introducing instruments, that the false passage had been established.

"Having allowed the patient to remain quiet for a few days, I succeeded, on the 7th of August, in passing a bougie through the stricture, and thinking it likely that the treatment by dilatation would prove unsatisfactory, performed division on the 11th. The urine escaped partially by the wound for a few days, and the patient, who had been quite well for a fortnight, was dismissed on the 2nd September without any trace of the disease or its remedy."

But we are not informed as to the present state of the patient. Every hospital surgeon has sent out cases of stricture treated by dilatation, with as favourable a note as Mr. Syme's

of A. S., but that does not prove the *permanency* of the cure; and although in Mr. Syme's hands the operation by external incision has, he says, but in one case turned out unsuccessful, and which case Mr. Syme has, as we would have expected from his high character, brought forward, still that one case is, in our opinion, quite sufficient to place the profession on its guard, the infliction of a fistula in perineo being a serious addition to a stricture of the urethra. In absence, therefore, of the proof of the permanency of the cure of a permeable stricture of the urethra by external incision, we are not at all surprised, as Mr. Syme expresses himself to be, that the profession has not adopted his treatment of stricture by the perineal incision, in preference to that by dilatation.

The great practical feature in Mr. Wade's book consists in his warmly advocating the treatment of stricture of the urethra by potassa fusa. After furnishing his readers with a history of the treatment of strictures of the urethra, Mr. Wade says:

"The severe effects occasionally produced by the nitrate of silver in the hands of Sir Everard Home, who used it very freely and boldly, naturally excited much prejudice against the method he employed; consequently, so formidable a weapon as the armed bougie of Sir Everard is seldom wielded by modern surgeons. The nitrate of silver is occasionally used in small quantities to irritable strictures with very good effects."

And after informing us that Mr. Whately recommended the employment of potassa fusa in strictures, in preference to nitrate of silver, Mr. Wade says that the practice of Mr. Whately has generally been regarded as ineffectual, from the extremely small quantity of potash which he employed; and tells us that it was the inefficient action of the nitrate of silver that first induced him to try the effects of potassa fusa in impervious stricture, more as a forlorn hope than with any confidence in its success; and he soon found it was necessary to use the potash in more efficient quantities, and more frequently, than recommended by Mr. Whately; and, encouraged by success in two or three cases, he has been induced to persevere in the use of that agent. The cases in which Mr. Wade recommends the use of the potassa fusa are the following:—Firstly, fibro-cartilaginous strictures, impervious to instruments, without the employment of injurious pressure; secondly, hard strictures of long standing, which, although admitting the passage of a small bougie, bleed freely on its introduction; thirdly, irritable strictures; fourthly, spasmodic strictures, not arising from acute inflammation of the urethra; fifthly, strictures which have a

marked disposition to contraction. Mr. Wade applies the potassa fusa by inserting a small portion of it into a hole made in the point of a soft bougie, and says that the eighth of a grain is the smallest, and a grain the largest quantity of the potash that he ever uses. Mr. Wade refers to the Westminster General Dispensary, to which institution he has been attached for the greater portion of his professional life, for ample opportunities of witnessing the effects of different modes of treating strictures of the urethra; and the result of his experience, he says, is, that more can be done in bad cases of stricture by the judicious employment of potassa fusa, than by any other means. Any person taking up Home's work would be led to suppose that the treatment of stricture by nitrate of silver was the only safe and permanent one, and as innocent as Mr. Wade says his treatment by potassa fusa is; however, a careful perusal of Sir Everard Home's own cases at once proves that the treatment by nitrate of silver has been often followed by serious results. Notwithstanding, such was the influence of Home's name and writings, that for a time the treatment by nitrate of silver became the fashion; so much so, that the late eminent Professor Colles says in a clinical lecture, the notes of which we have by us, "I recollect when Dublin men, physicians and all, ran mad about strictures; it was just after Home's work came out, and every man thought he had a stricture, and nothing was spoken of at club-houses, &c., but strictures, and 'how is your stricture?' became a complete watch-word. But some of the most valuable lives were lost by caustic before sufficient experience was obtained, and many died, others got violent rigors, which they were pleased to call intermittents, but which, in fact, were urinary fevers; and after a short time we found to our mortification it was a most dangerous practice. Home's plan did not get into discredit till many of his patients returned to Dublin, and then we found out that they were not *permanently* cured.

We are of opinion that the use of potassa fusa, in sufficient quantity to act on the urethra, is open to all the powerful objections which experience has raised against the treatment by nitrate of silver. If there is danger in applying the nitrate of silver to the sound, instead of the diseased portion of urethra, how much must the danger be increased in applying so diffusible an escharotic as potassa fusa? And although Mr. Wade says that in only one case was this use of the potash followed by an untoward result, viz., by perineal abscess, even if the patient should escape from urinary fever, false passage, profuse hemorrhage, retention of urine, still we hold, that if the deliquescent potash be used in quantity sufficient to destroy an existing stricture, it will produce one of a more formidable character,

from the unavoidable loss of substance and the consequent contraction, one bearing, in fact, a considerable analogy to that form met with occasionally by every hospital surgeon at the orifice of the urethra, and which takes place at the former seat of a sloughing chancre, and is the result of the subsequent cicatrization. If on the other hand, an insufficient quantity be used, as recommended by Whately, and to a certain extent followed up by Mr. Wade, no more advantage is gained by the application than would be obtained by the simple introduction of a bougie, the potash, under such circumstances, exerting no escharotic effect on the adventitious structure.—[*Dublin Quarterly Journal*.

Hemorrhage Arrested with Spirits of Turpentine.
(Translated for this Journal.)

Among the numerous therapeutic purposes for which the spirits of turpentine has been used, there is one which we do not find mentioned by any French writer. It is the use of this article in arresting hemorrhage. English physicians, such as Adair, Brooke, Chyne, Clutterbuck, Copland, Elliotson, Hunter, Thompson, Vincent, and others, consider it as an agent possessing the most certain hemostatic qualities.

The known rapidity with which it suppresses mucous fluxes, and its efficacy in the treatment of purpura hemorrhagica, might have led to the belief that it would be good in hemorrhages, and particularly those in which there is no reaction or inflammatory phenomena, that is, those of a passive or atonic character, and also those produced by an alteration in the blood or a peculiar diathesis. It is in the latter forms of hemorrhage, that this medicine has proved most efficacious.

In the London Medical Journal, Mr. Smith has reported facts, showing the success with which he used spirits turpentine in hemorrhages, and its superiority over other styptics and astringents.

It will probably seem surprising, to see this used in hematemesis and enterorrhagia; its action, nevertheless, is sometimes surprising.

In hematuria it is also used very successfully. It may suppress the hemorrhage by its astringent properties, in the same manner that it does fluxes and morbid secretions from the urinary passages. It remains to be seen, however, whether certain conditions of the kidneys do not contra-indicate its use; and on this account physicians should, perhaps, be more careful in its administration in this disease than in any other.

Mr. Smith says that in all the cases of hemoptysis in which

he used spirits turpentine, there were pulmonary tubercles in various stages of development. In the hemorrhagic diathesis, he used it with much success, and would check for a time the progress of phthisis.

In atonic epistaxis, such as is observed in the aged and cachectic, the internal administration of turpentine will arrest the hemorrhage in a very short time.

According to English writers, the use of spirits turpentine with care, will never be accompanied by any unpleasant effect. It will frequently produce purging, very seldom vomiting, and nothing particular about the genito-urinary organs. We believe, however, that this medicine should be used with great care, and its effects very closely watched.

The dose is 20 drops every three or four hours, but 4 grammes may be given every four hours when the hemorrhage threatens the life of the patient. The best vehicle for its administration, is water with the addition of some aromatic syrup. It is well to combine the turpentine with some other therapeutic agent according to the case. In epistaxis, and generally in passive hemorrhages when the loss is very great, it is well to add the muriate of iron. In hematemesis and intestinal hemorrhage, the addition of sulph. magnesia, iced water, tannic or gallic acid is advised, and so on with the other forms of hemorrhage.

The following are some of the formulæ of Mr. Smith :

R	Comp. infusion of roses,	225 grammes.
	Sulph. Magnesia,	250 "
	Manna,	16 "
	Spts. Turpentine,	6 "

Add according to circumstances,

Tinct. Digitalis,	6 "
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In hematemesis, enterorrhagia, hemoptysis, two table-spoonfuls of the mixture every four hours.

R	Spts. Turpentine,	6 grammes.
	Comp. Powder Adraganth	8 "
	Ext. Hemlock	60 centigrammes.
	Tinct. Digitalis,	4 grammes.
	Camphorated Mixture	250 "

In gastro-intestinal hemorrhage, dose same as above.

R	Sulph. Magnesia,	30 grammes.
	Spts. Turpentine,	40 "
	Puly. Uva ursi,	4 "
	Camphorated Mixture	250 "

R	Pulv. Uva ursi,	4 grammes.
	Ess. Peppermint,	2 drops.
	Spts. Turpentine,	8 grammes.
	Pulv. Adraganth,	4 “
	Water,	250 “

The two last used principally in hematuria. Two table-spoonfuls every four hours.

R Spts. Turpentine, 4 grammes.
 Muriated Tinct. Iron, 10 drops in a little water. Used in atonic epistaxis. Repeat the dose every two hours, but give only half the quantity after the first dose.

NOTE.—M. Nimmo, of Glasgow, has discovered a method by which the taste and smell of spirits turpentine may be destroyed entirely. He takes eight parts of the medicine and one part of rectified alcohol; this is well shaken and put aside for a while, it is then decanted and another portion of alcohol added and decanted again, and this is repeated until the smell and taste of the turpentine is entirely destroyed. It must then be kept in halfounce vials well stopped, and away from the contact of air and light, or it would very soon regain its former characters. It is questionable whether this manipulation does not destroy some of its therapeutic properties.

[*Bul. Gén. de Thérap.*]

Pharmaceutic Notice of Coffee and Caffeine.
 (Translated for this Journal.)

M. Vanden Corput has just published an article upon the chemical and medical properties of coffee, and its active principle, caffeine. Their febrifuge and anti-neuralgic properties are now well understood by the Belgian physicians. Numerous applications of coffee in the treatment of disease have been made, although the fact is but little known. Lanzoni says that he has obtained cures of obstinate diarrheas with the infusion of coffee. Nebulius employed it in headache. Baglivi used it with advantage upon himself in this disease. Alpin employed it as emmenagogue, anti-arthritic and anti-asthmatic. The females of Ethiopia have used it from time immemorial as an emmenagogue. Dufour, in the seventeenth century, gave it in phthisis, in fever, and in sick headache. Willis, in the fifteenth century, recommended it as an antidote for narcotics. This knowledge he obtained from the Turks, who use it to counteract the bad effects of opium. Grindel and Dorpat employed it as febrifuge. Musgrave, Pringle, Monin, Percival, Lawrence,

and many others, derived good effects from it in Asthma. In that portion of Batavia belonging to Holland, the inhabitants use the infusion of coffee, with a little lemon-juice, in their pernicious fevers. In Holland, this is preferred to quinine for intermittent fevers. M. Amati has used with advantage the vapors that are disengaged from it during its torrefaction in chronic diseases of the eyes. Martin-Solon has administered coffee in the adynamic form of typhoid fever. It has also been proposed as a disinfecting agent; and M. Guyot has recently recommended it in the treatment of whooping-cough.

Besides its medical properties, properly speaking, coffee possesses another that is very precious. This property M. Vanden Corput has forgotten to mention in his very interesting treatise. We will notice it, however, in order that it may be generally known among practitioners. It possesses the property of concealing the disagreeable taste of the sulphate of quinine, sulph. magnesia, senna, &c. We have already called the attention of the readers of this Journal (T. xxxiii., p. 131) to this singular property. The question whether or not the sulph. quinine preserved all of its medicinal properties, when it was mixed with coffee, or whether it lost some of them, was discussed, but not satisfactorily answered. Are not the febrifuge properties that it possesses, in favor of its administration in connection with sulph. quinine?

Another property of coffee that has not yet been noticed, is, that it favors and develops the action of certain remedies. Thus the effects of *haschisch* are rendered much more certain when administered with coffee.

According to M. Paven, coffee is composed of cellulose, fatty matters, glucose, an intermediate vegetable acid, legumine, caseine, chlorinate of potash and caffeine, free caffeine, an essential concrete oil, a fluid essential oil and mineral substances. Torrefaction produces a pyrogenous oil, that gives to coffee its peculiar taste and odour, and forms a certain quantity of tannin, which makes it tonic.

To what principle does coffee owe its medicinal properties? It certainly derives them from an association of different principles, but particularly from caffeine. We will therefore mention the mode of obtaining it, borrowing the process from the *Traité de Chimie*, by Liebig.

The best process for extracting the caffeine, is to infuse the coffee in boiling water, and to add to it acetate of lead whilst warm, and then a little finely powdered litharge. The liquid should be reboiled as long as any of the yellow precipitate remains that was caused by the action of the lead. After all the precipitate has been taken up, the mixture should be filtered,

and diluted sulph. acid added. The sulph. of lead should then be separated from it, and the liquid be evaporated, when the crystals of caffeine will be formed.

The preparation proposed by M. Vanden Corput is the citrate of caffeine. This salt is obtained by saturating a solution of citric acid with pure caffeine, and elevating the temperature to 32° R. The salt then crystalizes in long brilliant white needles, grouped concentrically around a central point.

It can also be obtained by placing pulverized coffee in a very weak solution of citric acid, agitating the liquid with an equal volume of sulph. ether, decanting and leaving the aqueous solution to crystalize. This salt is very soluble in water. The quantity of the tribasic citric acid that saturates the caffeine is but small, and hence the citrate solution produces but little precipitate by the addition of acetate of lead.

The citrate of iron and of caffeine is prepared by a combination of one part of citrate of caffeine and four parts of citrate of iron. The crystals formed are in radiated scales, that are very soluble in water. The lactate of caffeine is obtained by direct combination in dissolving caffeine in diluted lactic acid, and evaporating by gentle heat. It crystalizes with difficulty, and forms frequently an amorphous or half crystalized mass. *[Ibid.]*

Efficacy of Citrate of Caffeine in Sick Headache.
(Translated for this Journal.)

Our co-laborer, M. Dorvault, in one of the last numbers of the Bulletin, published the opinions of M. Vanden Corput upon the chemical properties, the pharmaceutical preparations and therapeutical effects of the active principle of coffee, and mentioned, among others, its previous property as anti-neuralgic. We will now mention the result of some attempts of M. Hannon, which tend to demonstrate that caffeine, and especially the citrate of caffeine, enjoy an incontestible efficacy in nervous or sick headache. It is in the idiopathic and not in the symptomatic variety, that it is so serviceable. In two cases of idiopathic sick headache that returned periodically, M. Hannon administered citrate of caffeine as follows:—The evening before the first paroxysm he administered 10 grs.; the evening before the second 20 grs.; thus increasing the dose at each attack. These diminished in intensity, occurred at longer intervals, and finally disappeared. A third case, in which the disease occurred twice a month with great intensity, the citrate of caffeine was given in larger doses (36 grs.), and at the end of six months a complete cure was obtained. In many other

cases that the author did not think necessary to report it has been invariably successful. At each prescription, the symptoms either diminished or disappeared. The citrate is the best of all the preparations, and has the advantage over pure caffeine, that it is not so irritating. These facts are not at all surprising, for there is no physician who has not seen in others, or felt upon himself, the power that coffee possessed in curing or alleviating an attack of sick headache. We have, ourselves, obtained good results in administering coffee combined with lemon-juice.

The following is the mode of administration that Hannon recommends. It has already been shown in what doses this remedy is given in some cases; they should vary, however, according to circumstances of intensity, obstinacy, duration, &c. If the disease produces intense pain and suffering, the dose should be from 30 to 72 grs. before, or from the beginning of the attack. In cases, where the interval between the paroxysms is very long, the dose should be proportionably large. When the disease is ancient, the treatment should be continued a long time; but if, on the contrary, it is recent, and occurs at short intervals, the dose should be small. M. Hannon is convinced, by experience, that this medication should be made use of, the evening before, or from the beginning of the attack, when it cannot be foreseen the evening preceding. He subdivides the dose into several parcels, and gives each parcel at equal intervals, except when the attack has commenced, and then he gives the whole at a single dose. A trial of this agent is very easily made, and but a short time is necessary to determine its true value.—[*Presse Med. Belge.*

Sick Headache cured by full Inspirations.

(Translated for this Journal.)

When a medication is based upon the experiments made upon himself by an honorable professional brother, it is far better, in reporting it, to give his own words. We will then simply publish the communication of M. Tavignot upon the new therapeutic agent in the cure of this painful, if not dangerous disease.

“It was in the following manner that I discovered the efficacy of this new and apparently strange method for the cure of this affection. In October last I was attacked with pain and weight in the head, anorexia, a physical and moral prostration, &c. Experience taught me that I had to remain in this state for twenty-four hours. I concluded that this peculiar state of the nervous centres might depend upon a stagnation of blood in

the venous sinuses of the dura-mater, as M. Auzias Turenne supposes, or upon an imperfect aeration of this fluid. I immediately commenced respiring freely and fully during several minutes. I perceived a sensible relief, which induced me to continue, and in a short time I was cured. I got up and undertook my usual occupations, as I felt but a slight pain in my temples, which vanished in a quarter of an hour. This result was doubly agreeable to me, as it furnished me with a new and practical remedy. In ten persons, upon whom it has been tried, one half have found instantaneous relief, and in the others there has been an amelioration, or a complete failure. However, upon interrogating with care those who were not relieved, I am convinced that they did not have genuine sick headache: they had a neuralgic pain of the head, but it was not accompanied with that profound prostration and melancholy that I have mentioned as characteristic of the disease. It seems to me to be useless to search for the *modus operandi* of full and profound inspirations in the cure of sick headache. It is evident that by this means the venous circulation is accelerated, and the chemico-physiological act of hematosis is hastened. Then the explanation of the success of this new method is in one or the other of these conditions, or perhaps in both.

[*L'Observation.*

Spontaneous Tetanus cured by Inhalations of Chloroform.
(Translated for this Journal.)

When a new medication is proposed, the best means to test its value is to publish the results obtained by its administration. This is the fourth fact that has been published in your excellent journal upon the good effects of chloroform in tetanus.

The 1st August last, I was called to a young girl, 18 years old, of good constitution, who, two days before, had been taken, without known cause, with pains and contractions of the muscles of the neck, which gradually extended to those of the chest, abdomen and back. When I arrived, the masseter muscles, the muscles of the back, and particularly those of the abdomen, were considerably contracted. The head was drawn backwards, the lower jaw fixed and immovable, the skin covered with an abundant perspiration, the face red, pulse frequent and respiration accelerated. The girl groaned from the great pain, and could not move in her bed. Spasmodic movements occurred at intervals, and increased the pain and danger. I first administered calomel and jalap, which caused several alvine evacuations, and the expulsion of two *ascaris lumbricoides*. This, however, produced no amendment in the state

of the patient. I then prescribed 15 grammes (270 grs.) of chloroform. I recommended a few drops of this to be placed upon a piece of cotton and held to the nose every two hours, and also during the spasms, when they occurred. The inhalation soon procured repose, and arrested the paroxysms. The patient was not incommoded nor stupified by the anæsthetic agent. During three days the progress of the disease was arrested, though the muscles continued tense and she frequently bit her tongue. She could drink, but with great difficulty, and the urine flowed involuntarily. Stools, however, were obtained with great difficulty, even when injections were employed.

From the third to the sixth day the paroxysms diminished in frequency and intensity, except at night, when they continued strong. In the morning of the sixth day I administered the following prescription:

Distilled orange flower water,	120 grammes,
Chloroform,	2 “
Tinct. of Belladonna,	10 m.
Syrup of Gum,	30 grammes.

This was given by table-spoonfuls every two hours. A warm bath was also prescribed.

On the seventh day the state of the patient was more satisfactory—the pulse had improved, the skin was covered with a slight moisture, and the patient suffered much less; she slept well the preceding night, the jaw was more relaxed, the head had returned to its natural position, and the spasms had ceased. The chloroform was stopped except when the spasms threatened to return.

Between the eighth and ninth days the spasms showed a disposition to return, but were warded off by chloroform. The patient refused to take the internal medicine, and frictions upon the abdomen and along the vertebral column with chloroform and tinct. of belladonna in equal parts were recommended. The state of the case was much ameliorated on the tenth day: she spoke and desired to eat. Inhalations morning and evening, and at the moment of the paroxysm, were prescribed.

On the twelfth day the attacks were rare and very short. The patient consented to take a few doses of the portion above mentioned.

On the fourteenth day she sat up in her bed for the first time, took, without assistance, a few spoonfuls of milk, and was able to project the tongue from the mouth; the tongue was soft, moist, and wounded in several places; the muscles were less contracted; the head free, though the face was still red. She passed tranquil nights, but obstinate constipation existed during the last few days.

Convalescence was apparently commencing on the sixteenth day. She spoke easily and laughed with her companions. Liquid and fœtid stools were obtained by the use of injections, and her appetite increased.

On the nineteenth day she was convalescent. The spasms had not occurred for four days. There was a little stiffness in the muscles of the neck, back and abdomen, but in a much less degree than the preceding days. All danger seemed to be over, as there was no appearance of a return.

This case, and those that have been reported in your journal, have evidently been cured by chloroform. It is well known how few cases were cured before the discovery of this precious remedy. I attribute little or none of the good effects obtained to the belladonna that was prescribed at the same time. The inhalations were not pushed so far as to produce syncope, and yet the spasmodic movements have been readily dissipated by their use. The portion taken internally seemed to have materially aided that inspired in producing a cure. I am satisfied that I owe my success in this case to the chloroform alone.

[BARTH, M. D., *Bul. Gén. de Therap.*

Should not the above case be regarded rather as Hysterical than Tetanic?—EDT.

Cauterization of the Nasal Fossæ in Chronic Ophthalmia.
(Translated for this Journal.)

About twelve years ago, M. Morand of Tours, made known the good effects he obtained by cauterizing the nasal fossæ in certain chronic ophthalmias, and particularly in the scrofulous variety. The idea of the practice, was suggested to him by the intimate relations that exist between the scrofulous affections of the eye, and chronic inflammations of the nasal fossæ.—This practice has been most generally abandoned, yet according to Tavignot, it should not be neglected, but on the contrary it should be more generally resorted to. Since 1844, he and M. Aug. Berard have employed revulsives to the nasal mucous membrane in scrofulous and chronic affections of the eye, of another character. The results obtained by them have been very satisfactory. Sometimes they cauterized the membrane with a stick of nitrate of silver, in other cases they employed the ointment of nit. silver, made by rubbing up 180 grs. of lard with 18 grs. nitrate of silver. The first eight days the mucous membrane should be cauterized each day upon the side corresponding with the diseased eye. If both are affected, the side most diseased should be cauterized first. At the end of this

time the ointment should be employed. The best mode of applying this remedy, is to pass it into the nasal fossæ by means of a quill opened at both extremities, and after the introduction of the quill, the ointment should be pushed out by a stick.

M. Tavignot has changed this method, though he employs it in the young to whom he cannot apply his modification.—He has substituted for the cauterization, a powder composed of an inert substance to which he adds an astringent or caustic in proportions varying according to circumstances. The patient can snuff this with great facility. The best formula seems to be: sulph. zinc 36 grs. and pulv. camphor 18 grs. rubbed up together. This powder should be snuffed 5 or 6 times per day. A kind of erythematous inflammation is thus obtained, that will suffice in slight cases. If a greater effect is desired, the zinc should be increased to 72 or 144 grs. to the same proportions of ingredients. The following proportion is more active:—nit. argent 36 grs., pulv. camphor 18 grs., rub together. The different active substances in these powders may be increased or diminished according to the effects produced, or the results desired to be obtained.—[*Ibid.* *Union Medicale.*

Good Effects of Camphor in Nervous Coughs.

(Translated for this Journal.)

Camphor is evidently an antispasmodic. Experience has proved this, and our predecessors have employed it with advantage in many cases. Because very great abuse has been made of this agent, is it any reason why it should always be discredited by physicians? This is not our opinion, and we will continue to collect facts that will tend to make known its true therapeutic effects, and to regulate its use. After having a long time struggled against prejudices of this kind, M. Alquié of Montpellier, determined to employ camphor in obstinate nervous coughs, which had resisted the agents usually administered in such cases. The results he obtained are too remarkable to be unnoticed. The first opportunity he had to prove the prompt and decided influence of camphor in such cases, was in the case of a young lady, very nervous, who had been affected about a week with an obstinate and dry cough that produced great weakness and pain in the chest. He advised her to take 12 grs. of camphor. The next day the cough had almost entirely disappeared, and 10 grs. more completed the cure. A short time after M. Alquié was called to a lady affected with violent cerebral congestion and a strong cough in consequence of exposure. A large bleeding, sinapisms to the

feet, and a blister to the arm, promptly dissipated the cerebral symptoms but did not benefit the difficulty of respiration, or amend the nervous cough. The latter continued dry, painful, and was accompanied with a little fever. M. Alquié ordered camphor, as in the preceeding case, and the following day the cough had disappeared. From these facts it would appear, that camphor rapidly dissipates not only simple nervous coughs, but also those that are dry, painful, produced by catarrhal irritation of the bronchi without any appreciable lesion of the lungs. No benefit, however, is derived from camphor when the cough has become humid and accompanied with expectoration of thick and yellow mucus, nor in cases where there is a material lesion of the lungs. The mode of administration is very simple. The camphor should be slightly pulverized or crushed, and a small portion swallowed at intervals of several hours.

[*Rev. Therap. du Midi, and Abeille Medicale.*]

Pruritus of the Vulva of Infants, treated with Saltpetre baths.

Pruritus is caused by the presence of small worms in the genital organs and anus of little girls. These worms are called, by Rudolphi, oxyurus. We give two cases by Dr. Vallez. The first was a little girl, ten years of age, who was brought to the doctor for an affection of the eyes. During the examination of the eyes, he found, by the frequency with which she carried her hand towards the region of the vulva, that she had been suffering from an intolerable itching in these parts for a long time. The itching was so great that it was almost impossible for her to remain quiet for the shortest time. M. Vallez advised lotions of sublimate, but at the end of a few days, no improvement having taken place, M. Vallez proceeded to an attentive examination of the genital organs. To his astonishment he discovered a quantity of small worms in the fossa navicularis and fourchette, which by their movements produced the itching. He prescribed tepid hip-baths, containing a quarter of a pound of saltpetre in each bath. Whilst the patient was in the bath, the lips of the vulva were kept separated so as to aid imbibition. After taking three baths the patient was radically cured.

The second case, was that of a young girl who had been suffering from continual itching about the vulva for two years. M. Vallez treated her with the saltpetre baths, and after taking two she was entirely cured.

Whilst recommending the treatment of M. Vallez, we must add, that in several cases which came under our observation, either in young girls or pregnant women, two or three frictions

made with mercurial ointment upon the seat of the disease caused the itching to subside.—[*Gaz. des Hopitaux*.]

Treatment of Varicocele.

To the Editor of the Boston Medical and Surgical Journal:

SIR,—The treatment of varicocele by the pressure of a truss over the spermatic veins, at the external inguinal ring, as first recommended, in my notice, by Mr. Curling, in the London Lancet for June 15, 1845, and since then approved by several others, does not seem, as yet, to have obtained so general a reception in practice as its merits deserve. The reason of the caution or neglect with which the suggestion has been received, undoubtedly is, the theoretical presumption that such a remedy would inevitably be injurious, by preventing the return of the blood from the spermatic veins. A single trial of the truss in a case of varicocele will remove that presumption. No danger, or inconvenience, or discomfort, will result. The proximate cause of the varicosity and of the suffering that attends it, is the pressure of the superincumbent column of blood, unrelieved and unsupported by healthy vein-valves. The truss, by its pressure, closes the vein, supports this column, and relieves the distended veins below: these then contract of themselves; the blood, sent into them by the spermatic artery, returns through the superficial veins, and the irritation, which results solely from the distending pressure, speedily subsides, and restores the patient to comfort, and after a few weeks or months, to health, or all the signs of health.

I first used this remedy four years ago. Then, and in several cases in which I have recommended it since, it proved harmless and effectual. If the remote cause of the disease be, as I suppose, a deficiency of the valves, it is, of course, beyond radical remedy. And, accordingly, I find that the complaint, sooner or later, is apt to return, if the truss be dispensed with, and to necessitate a second resort to that remedy. But if it may be said, on this account, that the cure is not radical, it is at least true that the treatment leaves the disease no more than an inconvenience.

The case, the worst, and at the same time the most satisfactory, of all I have treated, came into my hands in January, 1850. The patient, C. R., had himself brought to me, from his residence, twelve miles distant, on his back—a position that, with intermissions of not more than ten minutes, he had maintained for three months, if I remember rightly, and which he continued to maintain, as I will explain, as much longer. He was about 50 years of age, and had suffered from varicocele

since early puberty. For several years, so irritable had the parts become, he had been frequently obliged to confine himself, for weeks and months at a time, mainly to the horizontal posture. At such times, as he informed me, the parts effected were inflamed, swollen, tender and unusually painful. As we often see in such cases, the pain had come to be, in a measure, of a neuralgic character. The patient's physical and mental powers were suffering under the constant irritation, and the superinduced hypochondriasis. Bad off as he really was, he thought himself worse, and had as little peace of mind as of body. He had long used a suspensory bag, but of late had found its relief very limited. Other remedies had been tried. He had consulted a good many physicians, and among them two professors of surgery, who, having (very justly) little inclination to recommend the common methods of seeking a radical cure, told him, "if he could not get along otherwise, he had better have the testicle removed." It was, in particular, for my opinion on this point, that he came to see me. I recommended a truss. This was altogether contrary to his theory of the disease. He had read a good deal on the subject, and perhaps was not the easier to manage on that account. I explained my notions to him, and he went away half convinced; consulted his books, and his favorite doctor in the neighborhood where he lived, and came back to me afraid to try it "for fear the veins would swell up and inflame." This course of proceeding was repeated several times, till at length I gave him my views in black and white, fully reasoned out, to all possible contingencies and results. With this memorandum in his pocket, for easy reference, he was able to keep his judgment steady. He got a truss and put it on; and the next time he came to see me, much to my gratification and relief, he came on his legs. In short, he now calls himself well, and insists that I shall "publish his case," or he will do it himself.

A single practical direction in regard to the amount of pressure: it should be quite slight, just enough to close the calibre of the vein. Any easy hernia truss will answer the purpose.

Middlebury, Vt., Dec. 24th, 1850.

CHAS. C. P. CLARK.

Chloasma. By WM. GRAY, M. D., of Manchester.

This disease of the skin is also known by the names, Ephelis, Maculæ hepaticæ, Pityriasis versicolor, Leberflectete, and Liver spots; and generally makes its appearance on some part of the chest or arms, and extends in very irregular patches to other parts of the body, some times covering nearly its entire

surface. As far as the disease spreads, the skin assumes a dull yellow or brown color, sometimes varying in tints. There is a very slight elevation of the cuticle in most cases, with a very fine eruption. Occasionally the itching is very annoying, though not at all constant. The patches are often covered with minute scales.

This disease is supposed to exist as a sequel to disease of the stomach or liver; but several cases have certainly come under my observation, where there was no perceptible functional derangement, either of the stomach or liver; and I am of the opinion, therefore, that it has no more connection with derangement of the stomach and liver than has impetigo, lepra or psoriasis.

My principle object in introducing this subject is to speak of the treatment which I think has been heretofore unsatisfactory, both to the physician and the patient. During the early years of my practice, the cure of this superficial disease annoyed me exceedingly. In 1844, I began to use the Sulphur Fume Bath as a remedy, and from that time have had entire success; and am now prepared to recommend this remedy as a specific for this disease, if there be any specific in medicine. In recent cases a few applications are sufficient; and in no case has it been necessary to apply it more than eight or ten times. If any member of the profession has a remedy as certain as this, and more easily applied, it would be highly gratifying to have it made more public.—[*New Hampshire Jour. of Med.*

Microscopic Examination of the Discharges from the Bowels in Cholera. By R. S. HOLMES, M. D., of St. Louis, in a letter to the Editor of the American Jour. of Med. Sciences.

I have examined (microscopically) the discharges from the bowels in six cases of cholera, and have found the cells of cryptogami in a greater or less degree in four of these cases, and *vibriones* very abundantly in one. The theory I think amounts to nothing. I have found in flour *every one* of the forms of cryptogami that I have been able to discover in cholera cells; one has a peculiar shape, which I have not seen described. I have had a bottle of flour and water on my table for some months, and I am confident I could show in the course of a few days every one of the forms of vegetable growth in it that are seen in cholera discharges, by a Ross one-eighth lens: I say in a few days, for these cells vary in the flour, and are sometimes not to be seen; the cell of the mould of flour precisely resembles that of the smallest of the cholera cells, which is not more than the one twelve thousandth of an inch in diameter,

although the peculiar *cholera cell*, so called, seems to have been limited by the English investigators to a much larger cell, with buds upon it.

I may mention that I discovered distinct crystals, having the exact forms of those of lithic acid, in one case where there was suppression of urine.—[*Amer. Jour.*

Subcutaneous Punctures in articular Rheumatism. By M. GUREN.—Frequently joints which have become invaded by an attack of rheumatism long remain the seats of most obstinate pain. On a close examination we may assure ourselves that this pain is neither uniform nor general, but partial and localized at certain points. On handling the part we can even feel, opposite the immediate seat of pain, little knotty points which are exquisite to the touch. Such points exist even during the acute stage of rheumatism, but are much more easily recognised and isolated in the subacute stage. It is towards these points that the subcutaneous punctures should be directed, taking care, as in the ordinary application of the method, to raise a fold of the skin. The point of the instrument divides and liberates this tumefied and, so to say, indurated part; and the instant this is effected the pain ceases, and pressure can detect no trace of the nodosity thus destroyed. Whether a few drops of blood flow or not, the same result follows, so that the practice does not operate as an antiphlogistic. It is in fact only a liberation (*debridement*).—[*Gaz. Méd.* 1850, No. 22. *British and Foreign Med. Chir. Rev.*

On very Minute Doses of Tartar Emetic, in Phthisis and Asthma. By M. BERNARDEAU.—In vol. xxxi of the *Bull. de Thérap.*, M. Bernardeau gave an account of the great benefit he has seen derived from the administration of minute doses of tartar-emicetic in the hectic of phthisis. Since that period he has used it in other stages of tuberculization, and in several cases of asthma, with excellent effects. He gives from three to six pills in the twenty-four hours, each containing 1-25th of a grain. By their use, the cough, dyspnœa, and inordinate action of the heart become calmed, and in fact all the good effects of morphia, without its inconveniences, seem to be produced.—[*Bulletin de Therapeutique*, vol. xxxiv, ii, p. 311. *Brit. and For. Med. Chir. Rev.*

Stethoscopic Sound attending the detachment of the Placenta. M. Caillault relates in "l'Union Médicale" the discovery of a peculiar sound produced during the detachment of the placenta

after the delivery of the child. M. C., in making observations at the "Hopital Beaujou," found that upon applying the stethoscope over the hypogastric region immediately after the expulsion of the fœtus, he heard at first nothing but the sounds produced by the intestinal movements—but that, as soon as the uterus began to contract, a new sound was heard gradually increasing and diminishing in intensity with the increase and subsidence of the uterine contraction. This sound consisted of a series of rapid crackings similar to what might be produced by passing the finger nails over the straw bottom of a chair; and it was regularly reproduced at each contraction, until the placenta was expelled. M. C. has had ample opportunities to determine the uniformity with which this sound is produced in every instance—and has had his discovery confirmed by the other physicians of the institution. He is therefore disposed to deny the correctness of Velpeau's opinion, that the placenta is usually already detached *before* the delivery of the child.

May not M. C. err in attributing this sound to the act of detachment, instead of regarding it as the mere effect of the compression of the placenta and to the consequent extrusion of a portion of its blood. It seems probable that this may account for the sound.

Caseine in the Blood of Nurses.—The blood of two women, whilst nursing, was examined by M. M. N. Guillot and F. Leblanc, and the serum, after being separated from the albumen, furnished an abundant white precipitate when it was boiled with a few drops of acetic acid. They discovered all the characters of casein in the solution. The quantity of this product seemed to be in proportion to the diminution of the quantity of albumen.

In experimenting with the blood of new born infants, no sensible traces of casein could be found.

The blood of men and women treated in the same manner, gave a light precipitate which was redissolved in a few drops of carbonate of soda. The precipitate was much less, and of a different appearance from that obtained from the blood of nurses.

On the Treatment of Sprains of the Ankle. By M. BAUDENS. —M. Baudens observes, that judging by the frequency of the occurrence of this accident, its treatment ought to be well understood and successfully practised: but that this is in fact far from being the case, and he is therefore desirous of making his own plan of treating it, by the cold-bath and gum bandage, more extensively known.

The indications are, first, to prevent or remove inflammation, and then to secure immovability to the distended or lacerated parts, until they have recovered their power, the patient being at the same time allowed the use of the limb. For the purpose of subduing inflammation, numbers of leeches are usually applied, and then an emollient cataplasm; and M. Baudens feels convinced that it is in consequence of such treatment that degenerated sprains so often augment the number of amputations in hospitals. By free leeching of a joint, the seat of sprain, two mischievous effects are produced. In the first place, the pain, which is the first of the series of symptoms of inflammation after sprain, is increased by the leech-bites, in place of being mitigated; and, in the next, the increased afflux of blood towards the part is encouraged instead of being repelled. M. Baudens, on these grounds, strictly forbids the application of leeches in all surgical maladies attended with acute inflammation, while he often derives most excellent aid from their employment in chronic inflammations; thus, by the induction of a temporary congestion, giving a fillip to the too languid action of the part. When blood need be taken in sprain, he abstracts it by venesection, although probably both the profession and the public, from the force of habit, would tax with ignorance any one who neglected the use of leeches. As to emollient cataplasms, they favor in place of opposing the afflux of fluids to the part, while the long maceration the joint has been thus submitted to, deprives it of its elasticity, gives rise to a pasty engorgement, and predisposes to the formation of white swelling.

M. Baudens has pursued his own plan of treatment now for twenty years, and under it his patients have been enabled to resume their trying military duties in a very short time. He is not the first who has employed cold water in the treatment of sprain; but his originality consists in trusting to it alone, and continuing its application for so long a period. His plan of employing it, contrasted with that of his predecessors, may be thus summed up:—1. *Period of the Application.* Cold has usually been thought desirable only when it could be resorted to very shortly after the accident; but he applies it not only immediately, but also several hours or days after the occur-

rence, or even in chronic sprain—whenever, in fact, there is a *morbid degree of heat to abstract*. 2. The local bath has never been ordered by others for longer than five or six hours, although some practitioners, since his first publication on the subject, have ventured to extend it to twenty-four. In certain of his cases, however, immersion has been continued for eight or ten days, and, in one example, for fourteen days; while in no case has it been less than for two. 3. *Mode of application*. The vessel containing the water is brought to the bedside of the patient so that he can conveniently place his leg in it, having the heel resting on a sponge at the bottom, the leg and thigh being supported by cushions, so that the position may be maintained as many days as required. In the vessels used at the Val-de-Grâce the water reaches as high as the middle of the leg, and is changed about every three hours in order to keep it sufficiently cool. Spring water is usually employed, and if the inflammation is intense, ice is added. A purgative is given, and, if indicated, one or two bleedings are resorted to. 4. *Effects*.—One of the first of these is the cessation of pain, which sometimes occurs at once, and at others in an hour or two.—From the moment the foot is placed in the bath, the swelling becomes stationary, and soon after, with the heat and redness, decreases. About the fourth or fifth day the part becomes wrinkled like the hands of a washerwoman, and usually about the third or fourth day, the patient finds the water too cold, and then the limb is removed from it—the period for doing this being regulated by the patient, he being told to keep it in only as long as he derives comfort from so doing. Few of the patients suffer from any general reaction. *Gangrene* has been said to have resulted from this application, but the author has never met with such a case. The patient sometimes persists in keeping the limb in water after the dispersion of the heat and pain, and the consequence is the production of engorgement of the joint, a tense state and dark color of the skin, together sometimes with darkish lines—precursory signs of congelation in fact—on seeing which the joint should be enveloped in a fomentation of elder-flowers and poppy-heads at the temperature of the atmosphere. The objections which have been urged from the fear of producing *repurcussion*, are quite theoretical and unfounded. It is in fact only the *excess of morbid caloric* that is abstracted.

Gum-bandage.—When the inflammation has been subdued, all the depressions in the vicinity of the joint are filled with wadding, and a bandage carefully and equably applied. This is well moistened, by means of a brush with very thick gum, which in a short time imparts to it almost the hardness of wood. After this has been worn for twenty-five or thirty days, it is

removed and the joint slowly and gradually exercised; for want of which precaution many patients (especially those treated by leeches and poultices) suffer all the symptoms of a sub-inflammation of the white tissues of the joints, even for years.—[*Gaz. des Hôp. Brit. and For. Med. Chir. Rev.*

Refracture of a Leg, to improve defective Surgery. By R. D. MUSSEY, M.D., Professor of Surgery. Medical College, Ohio.

On the 29th January, 1848, Miss J. E. Kingsley, a school teacher, in Jefferson county, East Tennessee, in descending a hill, was thrown from a buggy, and had both bones of the left leg broken in two places; one three and a half inches below the knee, the other two and a half inches above the ankle.

It was six weeks before Miss K. began to sit up in bed, and four months before she was able to ride out. She came to Cincinnati in July of the same year. Ever since the injury, the leg had been considerably swollen, and there had not been a day without more or less pain, sometimes severe, extending from the upper fracture to the heel, back of the foot and toes, indicating lesion or compression of the fibular nerves.

Both fractures were firmly consolidated. The lower fracture was well enough, exhibiting no deformity—at the upper one, the leg was sadly bent, exhibiting a prominent external convexity, or angle, so great as to shorten the distance from the knee to the inside of the foot about an inch and a half; the plantar surface of the foot looking inward, and its outer edge looking directly downward. Of course, the limb was altogether useless in walking; any attempt to apply the foot to the ground aggravating the pain. It was impossible to place the sole of the foot down flat, or bring the heel within an inch of the ground. The limb was therefore left to swing, while Miss K. moved about upon the other leg, and a pair of crutches.

In September, 1848, aided by my son, Dr. Wm. H. Mussey, I operated in the following manner. A firm pad an inch and a half thick, was laid upon the inside of the knee, another upon the inside of the ankle, extending five inches up the leg. A splint of hard wood, one inch thick, and three inches wide, was laid, and secured by a bandage, upon these pads. A broad padded belt was placed over the angular projection of the fracture, and gradually tightened by a mechanical power, derived from Jarvis' adjuster, till the fracture was crushed, and the leg straightened.

Miss K. having been placed under the influence of Chloroform, was wholly unconscious of pain during the operation, and occupied herself all the while, in singing sacred songs, and

holding celestial conversation; and while a bandage and splint were being applied to maintain the new position of the limb, finding herself coming to earth again, she entreated most earnestly for more Chloroform, to prolong the ecstatic illusion. After the operation, the pain in the leg and foot were diminished, and in two months the fracture was consolidated.

Dec. 12. There is now no pain at the heel, and comparatively little in the leg and foot. The limb has its natural direction, is as long, and apparently as strong as the other. She can now walk with a cane, and limpingly without one.

Feb. 1849. Miss K. now walks very well without crutch or cane, and only now and then feels slight pain in the leg, the nervous injury having been almost repaired. Some months after the above date we saw Miss K. walking well in the street as if nothing had happened.—[*Western Lancet*.

The Difficulty of Breathing from an over dose of Opium, relieved by inhaling the Vapor of Water. By CHARLES W. WRIGHT, M. D., of Cincinnati.

The difficulty of breathing which is commonly met with in cases of poisoning with opium, is generally ascribed to its producing paralysis of the respiratory muscles, and that asphyxia is thus induced, which is the immediate cause of death.

Having observed this symptom in several instances, and knowing the power which this drug has of arresting all the secretions, except that of the skin, I was led to suppose that the difficulty of breathing was not, in all cases, to be referred to paralysis of the muscles of respiration, but was to be accounted for, in part, at least, on different principles.

Now it is absolutely necessary, that the mucous membrane of the lungs should be kept constantly moist, otherwise it is impossible for oxygen gas to be absorbed, and carbonic acid eliminated. It is observed in some cases of poisoning with opium, that the mouth and fauces become so dry, that it is almost impossible for the patient to swallow or speak, and that if the dose is sufficiently large, this dryness may extend into the respiratory organs, and thus give rise to great difficulty of breathing. In these cases the patient is not so much disposed to sleep, as when this symptom is not observed.

Having seen this effect of opium in several cases, I had determined to try the effect of the inhalation of the vapor of water, in mitigating the unpleasant symptoms thus induced. This I was enabled to do in my own case, a short time since, from having taken by mistake an over dose of opium, which could not have been less than ten grains. In this instance, the first warning of the mistake I had committed was embarrassed res-

piration, which soon amounted to an agony, without the least symptom of narcotism. In this case much the same sensation was produced by each inspiration as is experienced by the inhalation of pure nitrogen gas, the air seeming to leave the lungs without having performed its functions, there being at the same time a sense of dryness in the fauces and larynx. In this condition I commenced breathing the vapor of hot water, which produced immediate relief. After this, having ejected the poison from the stomach, by an emetic, no unpleasant effect followed.

When it is remembered that the power which a membrane possesses of absorbing a gas, is in proportion to its moisture, and that a dry one is as impenetrable to gases as horn, it is not surprising that the above symptoms should be induced by opium, which above all other substances, has the property of diminishing the secretion of the mucous membranes. It should also be borne in mind, that by arresting the pulmonary secretion, the blood loses its attraction, for the mucous membrane lining the lungs.*

Probably the best treatment which could be adopted, where this symptom is observed, would be to allow the patient to inhale the nitrous oxide gas, saturated with vapor, which would have the effect of restoring the moisture of the lungs, and presenting oxygen in a much more soluble form, than that which enters into the composition of the atmosphere.—[*Ibid.*

On Chorea. By DR. LEE.—An analysis of various published cases of this disease, and of forty-two observed by himself at the *Hôpital des Enfants*, leads Dr. Lee to the conclusion that there are four principal varieties of it.

1. One which has been called *sympathetic*, coincides with the local lesions of the various viscera of organic life, and especially with disease of the gastro-intestinal system and of the heart.

2. A second, which is very common, depends upon a *general* disease, and especially *rheumatism*. So frequent is this variety, that it constituted seventeen out of the author's forty-two cases, and thirty of seventy-four he has collected. Rheumatism indeed may not only give rise to chorea, but to a variety of other nervous disturbances, as simple convulsions, contractions, tetanic convulsions, pseudo-meningitis, pseudo-myelitis, &c.; and, in fact, there is no symptom usually referred to lesions of nervous substance, which may not be dependent upon a rheumatic affection of the joints or heart, such affection

* See Liebig's late work on the motion of the juices in the animal body.

being almost always marked by the nervous derangement, and giving rise to only very slight local suffering and febrile action, especially in a chronic neurosis like chorea. When, however, the febrile reaction is intense, the neurosis is usually only developed when the inflammatory fever has undergone some remission; and a reproduction of the febrile action always induces an improvement in the nervous symptoms; except in some cases in which the disease proves quickly fatal.

3. Another form of chorea quite independent of cerebral alteration, is the so-called *essential* chorea, in which no appreciable change of structure is recognisable either in the organic viscera or the nervous system; this, like rheumatic chorea, is a very common form.

4. The last form depends upon *cerebral* or *spinal* lesion, and is but the symptom of various cerebral and spinal affections.—[*Bull. de l' Acad. British and Foreign Medico-Chir. Review.*

Influence of a Salt Diet on the Composition of Blood.—Poggale has, moreover examined the blood of man, both at the time that the usual diet was taken, and whilst 154 grs. of salt were consumed daily. The following are the results:

				During usual diet.		During salt diet.
Water	"	"	"	779·9	"	767·6
Blood corpuscles	-	-	-	130·1	-	143·0
Albumen	"	"	"	77·4	"	74·0
Fibrin	"	"	"	2·1	"	2·3
Fatty matters	-	-	"	1·1	-	1·3
Extractive and salts	"	-	-	9·3	-	11·8

From which it is evident that the proportion of solid constituents are increased; this occurs chiefly in the blood corpuscles and extractive, the amount of albumen being slightly diminished.—[*Compt. Rendus*, xxv.

Boussingault has also extended his observations concerning the influence of salt on the fattening of cattle. His earlier experiments had shown, that salt does not exert that beneficial influence on the growth of cattle, and the production of flesh, which is usually ascribed to it. His present experiments have been extended over a period of thirteen months, and have been made on a number of steers, some of which had their rations salted, while the others had not; in other respects they were treated in a precisely similar manner. The results have shown that the increase in the proportion of flesh does not pay

for the salt employed. Boussingault, however, remarks, that a saline diet exerts a beneficial effect on the appearance and condition of the animals; for the steers which were deprived of salt for eleven months, appeared sluggish, and of a languid temperament; their coats were rough, devoid of gloss, and partially bare; while those which had been fed with salt were lively, had a fine glossy coat, and were sure to obtain a considerable higher price at market.—[*Ann. Ch. Phys.*, and *Liebig's Report. Ibid.*

Condition of Carbonic Acid in the Blood. Liebig remarks, that while water only takes up its own bulk of carbonic acid gas, serum has the power of absorbing twice its bulk of it.—Now as this cannot be dependent on the presence of neutral carbonates in the blood, Liebig endeavors to show that it is owing to the existence of basic phosphate of soda. This chemist finds that a solution of one part of dry phosphate of soda (2NaO , HO , PO^5) in 100 parts of water, absorbs, likewise, a double volume of carbonic acid. By shaking up with air, or by diminishing the atmospheric pressure, two thirds of the carbonic acid taken up are evolved at the ordinary temperature; the entire amount of carbonic acid gas is given off during simple evaporation in the atmosphere. When the blood absorbs carbonic acid, the soda of the former is appropriated partly by the carbonic acid, and partly by the phosphoric acid; but the phosphoric acid which has been expelled, remains and tries to reunite itself with all the soda; consequently the phenomena are different from what they would be if the blood really contained carbonate of soda as such.—[*Liebig's Report.*

Liebig states, that serum strongly concentrated by evaporation, does not evolve a trace of carbonic acid, on the addition of acids. Lehmann, on the contrary, asserts that blood contains a large quantity of alkaline carbonate. He has communicated the results of experiments, in which the free carbonic acid was expelled by hydrogen introduced into the blood, and the combined acid by means of acetic acid in a rarified space. According to his determinations, 1000 grains of fresh ox-blood yields on an average 0.132 grains, or 0.28 cubic inches of free carbonic acid, and 0.676 grains or 1.42 of combined. He mixed blood with an equal quantity of water, coagulated the albuminous constituents by heat, and evaporated the filtered fluid to dryness. The residue was incinerated at the lowest possible temperature; in 100 parts of ash there were found from 4.1 to 4.5 of sulphate of soda; 3.7 of phosphate of soda (3NaO PO^5); from 15.8 to 18.1 carbonate of soda; and from 74.0 to 75.0 of alkaline chlorides.—[*Liebig's Report. Ibid.*

Observations on the Growth of the Hair and Nails. By Dr. BERTHOLD.—To determine the time required by the nail to grow to a certain length, the writer first made some experiments on himself, and found that the nail of the middle finger grew 11 millimètres in four months. Continuing his experiments, he found a great difference in the growth of the nail, according to the age of the person, and the season of the year. For instance, he found that the same nail, which would take 152 days in winter to attain a certain length, would grow to the same length in 116 days in summer. The growth also differs on different fingers, as also on the right and left hand. On the right hand the growth is quicker than on the left.

The hair of individuals, from 16 to 24 years old, grew in two years 12 to 16 inches, or 7 lines a month.

The growth of the hair is accelerated by frequently cutting it.

During the day, reproduction of hair goes on more rapidly than at night. In warm weather, the reproduction is greater than in cold weather.

The quantitative formation of nail and hair coincides with the peripheric secretions, perspiration, &c., in this—that it increases in summer, and decreases in winter; whereas, the development and nutrition of the body is decreased in summer, and increased in winter; so that the weight of a man is greater in winter than in summer.

The growth of hair decreases in the night, which coincides with the decrease of the secretions, perspiration, formation of carbonic acid gas, urine, milk, bile, &c.

[*Müller's Archiv. Ibid.*

On Cod-Liver Oil in Phthisis. By M. DUCLOS.—M. Duclos thus sums up the results of his experience with this substance: 1. The presence of fever is what we must chiefly attend to, relying more on this remedy when it is absent, and less when it is present. 2. The remedy frequently arrests the progress of the disease when only in the first stage. 3. It rarely arrests it when in the second stage, although it may retard it. 4. The third stage is not favorably influenced by the oil. 5. The oil should be administered for a considerable time; and, if a good effect results, it should be suspended awhile, to be again resumed. Thus, it may be given for two months, and then suspended for a fortnight, resumed for a month, and re-suspended for a fortnight again, so as gradually to reduce the length of the intervals during which it is given. 6. The clear, slightly smelling, nearly tasteless oil, is less efficacious than the brown, thick, strong oil.—[*Bull. de Thérap. Ibid.*

Common Salt in Intermittents.—Prof. Piorry, in reporting to the “Académie de Médecine” upon the proposed use of table salt (chloride of sodium) in intermittent fevers, states that if administered in doses of two table-spoonfuls, it will not only arrest the disease, but also exert upon the spleen as marked an effect as quinine does. In 12 cases of intermittent fever, the salt uniformly arrested the paroxysms and lessened very materially the size of the spleen. The spleen was also found to diminish when the remedy was given in cases of typhoid fever.

If similar results can be obtained in this country, the discovery will be one of great value. It is very desirable to find a cheap substitute for quinine,—we would like to hear from those who may try the salt.

Periodic Hemorrhage from the Face.—Dr. Chrestien, of Montpellier, relates the case of a young lady who had been sent to the baths at Rennes for the regulation of her menses which had never appeared through the genital organs, but through the pores of the skin of the cheeks. Drops of blood appear upon these parts, return very soon after being wiped off, and continue in this way until the loss amounts to about 100 or 120 grammes of blood per day. This hemorrhage has already appeared several times at intervals resembling those of natural menstruation, and seems to supply its place. He does not give the result.

Nephritic Amaurosis.—M. Landouzy announces the following facts:

1st. That an impairment of vision is an almost constant symptom of Bright’s disease.

2d. This impairment constitutes a new form of amaurosis which may be called nephritic or albuminous.

3d. Amaurosis cannot be attributed to the deterioration of the strength.

4th. It frequently announces the disease before the appearance of the other pathognomonic symptoms.

5th. It disappears and reappears without following exactly the changes of the albuminous deposition in the urine or œdema.

6th. Albuminous nephritis should be considered as the result of an alteration in the nervo-ganglionic system.

New method of uniting Wounds of the Scalp.—We notice in the French journals the excision of encysted tumors from the scalp of a female, after which the edges of the wound were drawn together by platting the hair across it, and adhesion by the first intention obtained. The advantage of this method is obvious when it is desirable not to cut away the hair.

Ethereal Solution of Cantharidine.

Pulv. Cantharides,	-	-	-	1 part,
Sulphuric Ether,	-	-	-	2 parts.

Digest 3 days—and then separate by expression. By applying this solution to the skin with a camel-hair pencil, vesication takes place in one or two hours in children, and in three or four hours in adults.

Cantharidine Ointment.—This may be prepared by rubbing together equal parts of the Ethereal Solution and lard or mutton suet. Frictions made with this ointment induce vesication in a few hours. It is much used in Prussia.

Cochineal for Hooping Cough.—An anonymous writer, in the N. Y. Medical Gazette, recommends very highly the following prescription for hooping cough—to be given in teaspoonful doses, three times a day. He regards the cochineal as the active principle of the prescription, and hence gives it in larger doses than usual.

Cochineal, in very fine powder,	-	-	3 ij.
Carbonate of Potash,	-	-	3 j.
Sugar,	-	-	3 j.
Tincture of Spear-mint,	-	-	3 ij.
Water,	-	-	3 xiv.—Mix.

Miscellany.

“Surgical Report for the American Medical Association.—The committee is invited to meet in the Charleston Hotel, South Carolina, the evening of the first Tuesday in May next. All professional brethren, who have *surgical* facts connected with the improvement of this branch of the profession during the year, will please address them to the chairman of the committee by the first of April, at *Augusta, Ga.* As all cannot be reached by a circular, it is hoped no one will wait for a more direct application than this general invitation.

"By extending this notice, the medical periodicals of our country will advance the interests of the American Medical Association, and the editors will confer a favor upon their recent confrère.

PAUL F. EVE, M. D.,

"Prof. of Surgery in the Louisville University, and
Chairman of the Committee on Surgery.

"LOUISVILLE, KY., Dec. 1850."

Negroes in the Medical College at Boston.—We publish the following intelligence without comment, not doubting that Southern readers will duly appreciate the philanthropy of those who wish to furnish Liberia with "colored Doctors," and our own country with "women Doctors."

"Trouble among the Medical Students at Harvard University.—The following facts have been collected respecting some unhappy proceedings last week at the Massachusetts Medical College in this city. Among the students attending the medical lectures, are *three colored young men*. One of them is from Pittsburg, Pa., one belongs in this city, and we believe is a native, a son of the late Rev. Mr. Snowden, a colored preacher of much eminence for many years; the locale of the other is unknown to us. They are all, as we have understood, under the immediate auspices of the American Colonization Society, and by them are to be educated as physicians for the colony at Liberia. It was understood by the students last week that a *lady* was also to be added to the class. These departures from established rule gave offence to a portion of the members. On Tuesday morning the class held a meeting, and appointed a committee to draft a set of resolutions. The meeting was adjourned to the afternoon, when the students again assembled. The resolutions, respectfully remonstrating against the admission of *colored men* and *white women* were then taken up *seriatim*, and passed by a majority of the students present. We should here state, that the class attending the meeting in the morning showed a majority for sustaining the faculty in the course of admitting whom they pleased to their lectures; but not supposing any such resolutions would be presented, many of them did not attend the afternoon meeting. Those present who disapproved of the resolutions, immediately appointed a committee to present a minority report, sustaining the faculty, to be presented to the class at a future meeting. We regret exceedingly this little disturbance, and the course adopted by the class. We cannot but think that if they had any real grievances, it would have been better to have approached the faculty in some other way. It may be considered an innovation to admit colored men into our colleges; but when it is remembered for what purpose these were admitted, there really cannot be so much objection after all. But as to the propriety of admitting females to medical colleges in common with males, it is a matter in which there is a great diversity of opinion. We should most decidedly object to

the adoption of the practice, preferring to have all females, who wish to become disciples of the healing art, or otherwise assume the masculine professions, attend separate institutions for their education.

"Since writing the above, we learn that the faculty have announced to the class, that the lady in question, on hearing that there was a feeling against her being admitted to the college, has withdrawn her application. Respecting the colored men, they declined to reject them from the college, under the circumstances—as they have purchased tickets and thereby acquired a right of attendance during the present year."

[We clip the foregoing from the Boston Medical and Surgical Journal, and cannot refrain from expressing the opinion that the education of colored men as physicians and surgeons, for the service of the Republic of Liberia, on the coast of Africa, is a sacred duty, binding on our Medical Colleges in America. We deeply regret that either northern or southern students should any where object to this laudable work, and sincerely hope that the Faculty of Harvard may be sustained for better reasons than that "they have paid for their tickets, and thus acquired the right of attendance for the present year."

The claims of humanity are of paramount obligation, and we see not how any American can be indifferent to the necessities of our own colony in Africa, now emerging into a national existence under circumstances which challenge the admiration of the world. They must have colored physicians, for white men cannot endure the climate, and the want of medical men is one of the most pressing needs of the colonists.

We would respectfully suggest that the generous and magnanimous students now in large cities, should spontaneously meet, and express their readiness to welcome to their lecture-rooms all colored students whom their respective Faculties may see fit to educate for the service of the American Colonization Society.

They would thus do themselves honor, by recognizing the claims of humanity upon the profession of their choice, and at the same time give a gentle admonition to the misguided young men of Harvard, which is justly merited.]—*New York Medical Gazette.*

An appeal to the Medical Society of Rhode Island in behalf of Woman, to be restored to her natural rights as "Midwife," and elevated by education to be the physician of her own sex.—This is the verbose title of a *very religious* pamphlet, or rather Tract, "for the author," who is nameless, and ominously marked "READ AND LEND;" and strenuously recommended to be re-printed and published by subscription or otherwise, with the devout prayer of the author, that "God in his infinite goodness may reward such labor, for promulgating the mighty truth!"

The only "mighty truth" we can discover on reading the pamphlet is the following, viz:

"Dr. T. L. Nichols, of New York, a regular graduate of the Medical College of the University of that State, in a late periodical speak-

ing of his wife, Mrs. Gove Nichols, who is a thorough educated practising physician says: I am proud to say that she has taught me far more in connection with Obstetrics, than I could ever have learned in all our Medical Colleges and Libraries!"

This is a powerful testimony in behalf of CLINICAL teaching, and the tract, if reprinted and published to the extent prayed for, will be an admirable advertisement for this "Mrs. Gove Nichols, the eminent female physician," and her interesting pupil and husband, Dr. T. L. Nichols of New York.

As to the project of "restoring to Woman," any "natural right" of which she may have been deprived, we will go for such restoration in these days of "Women's rights," with all our might, although we confess to the private opinion that women, as well as men, would exhibit higher wisdom by inquiring more into their *duties*, than into their *rights*. Nor have we any objection to the "elevation" of woman, if such it be, to the office of Midwife, or the physician of her own sex, when educated as is here proposed; though if it be a "*natural right*" of the sex as here alleged, her education ought to "*cum by natur*."

But of this pamphlet we must say that we have never read a more indelicate, immoral, indecent, filthy, and caluminous publication. We do not marvel that the author concealed his name, for that it has been written by a man, there is abundant internal evidence; nor, indeed, could any decent woman be found who would father or mother the dirty brat. We doubt whether any woman, worthy the name, will withhold it from the flames, a moment after reading it, lest her sex should be polluted by its presence. And yet a certain class of editors have prostituted the press by commending it. Proh pudor! "It is enough to make one hide his face, and blush to be a man."—[*Ibid*.]

Miss Blackwell, M. D.—The movements of this estimable lady, and intrepid pioneer in the cause of female education, will continue to be a subject of interest with the Medical Profession. A private letter has been transmitted to us by a mutual friend, which we are not at liberty to insert in full, by which we learn she has continued to prosecute her studies in Paris, up to July last. The disease of one of her eyes, contracted from a patient under her observation, has proved a serious calamity, the sight being nearly destroyed.

In July she was at *Grafenburg*, at the hydropathic establishment of *Priessnitz*, partly to try the effects of his system upon the numerous patients congregating there, with a view to ascertain what success is really attained, and to determine how much is to be attributed to the therapeutic action of water, and how much to the general hygienic conditions under which the patients are placed.

She states that she has received a courteous invitation to pass several months in London, every facility for attending the hospitals and schools having been promised; and that it is her intention to avail herself of this opportunity to institute a comparison between French and British practice.—[*Buffalo Medical Journal*.]

New York reprint of the London Lancet.—We do not profess to understand the principle which guides the publication of this reprint. However much we would like to do so, one thing is clear, that we should expect in the usual monthly numbers, the matter which the original contained during the month preceding the day of ostensible republication at New York. But far otherwise is the case, and thus the reprint, far from keeping pace with the original, lags most fearfully behind, and treats its readers to matter months old. We were not aware of this until very lately, and having induced an intimate friend to undertake an analysis of the three or four last numbers, that gentleman has detected the following rather strange anomalies:—

The *August* number (N. Y.) contains papers from the March and April numbers of the original.

The *September* number (N. Y.) contains Guthrie's biography of 15th June. No reviews at all; Macmurdo's lecture on the eye, of May in the original, and Guthrie's lecture for March.

The *October* number contains the biography of Marshall Hall, which appeared in the original of 27th July. Macmurdo's lecture, number 7, of July 6. The review of Spencer Thompson, M. D., on Temperance, &c., 24th Aug., 1850, is entirely different from the original, with many omissions; and in fact many of the papers are so confused in the reprint, that it is almost impossible to compare the two.

Is this right, or is it wrong? If the latter, then is the reprint, not what it purports to be; and if the former, why these omissions, alterations, and delays in the republication of the papers. We say nothing of the entire omission of the *Lancet* editorials, which very frequently have important medico-political bearings. We ask again, why is this so?—[*Brit. Amer. Journ.*

The Largest Liberty.—The trustees of the Memphis Institute, by way of conciliating and uniting all sects and parties in medicine, have adopted the following sage resolutions:

“*Resolved*, That this school of medicine is not to go under any sectarian or peculiar denomination; is not to teach any peculiar system of medicine; but all that is believed will throw light on the nature of disease, or contribute to its alleviation or cure,—and that it professes to be orthodox.

“*Resolved*, That each chair will be expected to inculcate the doctrines which its Professor holds to be scientific truth; and that Professor Cross will teach the Institutes of Medicine as his convictions present, past, and future, shall deem sanctioned by the light of science, and his own high reputation and exalted rank in the profession, and as most conducive to the well-being and character of the Memphis Institute. To each Professor should belong the right to teach his own views and opinions connected with the branch of Medical Science committed to his chair.

“*Resolved*, That in creating the new chair, over which Professor W. B. Powell is to preside, it shall be designated as that of Cerebral

Physiology, Mineralogy, and Geology, and in which Professor Powell shall have the privilege of treating of the external senses, and of so much of the nervous system in general, as he may deem requisite to a proper understanding of the functions and pathology of the brain."

Under the auspices of so liberal a board of trustees, and with such an exceedingly accommodating faculty, who can doubt for a moment the speedy and triumphant success of the Memphis Medical Institute! The school "is not to go under any sectarian or peculiar denomination," and is "not to teach any peculiar system of medicine," yet "each chair will be expected to inculcate the doctrines which its professor holds to be scientific truth." In other words, they may teach any thing or nothing, according to their taste or capacity. Dr. Cross may, if he is so disposed, teach medicine upon scientific principles, while Dr. Powel has equal right to teach humbuggery to his heart's content. Verily this is a rare specimen of the largest liberty. But, seriously speaking, it is the lamest attempt that we have ever seen, to unite things which are as incompatible as light and darkness, and we are sorry to see Dr. Cross in such bad company.—[*St. Louis Medical and Surgical Journ.*

Remedy for Short Sight.—Dr. Turnbull thus describes a process for treating short sightedness. "In the first instance I applied the extract of ginger, which was rubbed for five or ten minutes over the whole forehead, with the view of acting upon the branches of the fifth pair of nerves. Afterwards I substituted a concentrated tincture of ginger, of the strength of one part of ginger to two parts of spirit of wine, decolorised by animal charcoal. The success of this operation was remarkable. In many cases it had the effect of doubling the length of vision. In some persons I found the iris was not much dilated, but very torpid. In these cases I applied the concentrated tincture of pepper made of the same strength, and in the same manner as the tincture of ginger. This I used until I observed that the iris had obtained a greater power of contraction and dilation, after which I had again recourse to the tincture of ginger. This plan of treatment has been attended with the most signal success, and persons who were extremely short sighted have very soon been enabled to lay permanently aside their concave glasses."—[*Boston Medical and Surgical Journal.*

Vaginal Speculum.—Mr. Haslam, of Harvard Place, Boston, is the inventor and manufacturer of an improved vaginal speculum. It is made of glass, and silvered on the outside; the silvering being covered over by gutta percha, makes it, of course, perfectly safe. The inside of the tube is a perfect mirror, and will reflect the light better than a metallic one; besides, there cannot be any danger of corrosion, either by the secretions or the substances used in medication. This speculum has been used by many of our best physicians for a year or two past, and has given the greatest satisfaction. Since the first

ones were manufactured, the proprietor has made improvements upon them, in form, size and covering, but can still afford them at prices extremely moderate.—[*Ibid.*]

Donation to the McLean Asylum.—Hon. William Appleton, of Boston, has given the princely sum of \$20,000 to the McLean Asylum for the Insane, located at Somerville, near this city, of which institution he has for many years been a director, for the purpose of constructing additional buildings for the more perfect classification of the inmates of the asylum.—[*Ibid.*]

Remedy for Sterility.—Dr. E. Williams lately published in the London *Lancet*, some account of a Japanese remedy for sterility which he had used with success. In a subsequent number he states that the communication had brought him upwards of 900 letters requesting a supply! He says that he is unable to supply the demand, but hopes to make arrangements soon that will enable him to do so.—[*Ibid.*]

Operations for Cataract upon Bears.—Mr. White Cooper, of London, has been operating successfully for cataract upon the bears of the zoological gardens, having previously administered chloroform to them.

Professor of Chemistry in Harvard University.—Mr. J. P. Cooke, the Prof. of Mineralogy, &c., in Harvard University, has been appointed to the Chair of Chemistry in the same institution, in place of Prof. Horsford.

We learn from the N. Y. Med. Gazette, that two of the Students of the University School of Medicine have recently died of Erysipelas, and that several others are seriously ill of the same disease.

Dr. Alex. H. Stevens has been elected President of the New York Academy of Medicine.

Dr. C. G. Comegys, of Cincinnati, reports a case of obstinate Sciatica immediately relieved by cauterization of the ear.

We find it stated in the Western Medico-Chirurgical Journal, (published at Keokuk, Iowa,) that the Evansville Medical College offer to admit "Sons of Temperance" at half price—and that the "Sons recommend the School as in every way worthy of public confidence."

SOUTHERN MEDICAL AND SURGICAL JOURNAL.

Vol. 7.]

NEW SERIES.—MARCH, 1851.

[No. 3.]

PART FIRST.

Original Communications.

ARTICLE VIII.

Fissure of the Anus. By JURIAH HARRISS, M. D.,
of Augusta, Ga.

It is somewhat singular that English and American authors should pass so summarily over this affection. In some works it is scarcely regarded, and is certainly not described as a distinct disease. This neglect is doubtless owing either to the circumstance that it is badly understood, or to the fact that its importance is not properly appreciated. It cannot be because such cases are not presented for treatment. This subject has long since arrested the attention of continental surgeons, and they have well defined its character and proposed various remedies for its cure. Those, however, who preceded Boyer, had but confused ideas of the affection, and attributed its symptoms to diseases of the bladder, prostate gland, hemorrhoids, &c. This great surgeon was the first who distinguished it from other ulcerations of the anus, and described it as a special disease. (Tome. 4. cap. 2.) He believed that the contraction of the sphincter, which so uniformly accompanies the fissure, was its cause and not its effect. The reason he gave for this opinion was, that he had frequently seen contraction of this muscle without fissure, but never the fissure without the contraction. And hence the remedy he proposed, with which all surgeons are familiar. M. Blandin, and many others, believe the contraction to be an effect of the fissure, and that this (the fissure) is caused by constipation and the passage of indura-

ted fæces which distend and excoriate the parts. He, however, divides the fissures of the anus into three varieties: 1st, those situated below the sphincter and occupying principally the skin; 2d, those above the sphincter, and situated entirely in the mucous membrane; and, 3d, those that are situated in the mucous membrane, upon a level with this muscle. (Dict. de Med. et Chir. Prat. t. 8.) The first two varieties, he says, are by no means serious and will heal of themselves, or will require but little treatment. The 3d variety, however, is a great annoyance to the patient, and it is therefore important, that some safe remedy should be proposed that would be in some degree certain in its effects. This variety may with propriety be called *true* and the other two *false* fissures, as they by no means present the same characters, nor demand the same treatment. In addition to this, the fissures below the sphincter may be caused by various agents, and would, therefore, present different characters. They may be induced by ichorous discharges from the vagina, or from chancres, &c., and therefore require different remedies.

The 3d variety is *perhaps* a distinct affection, presenting certain symptoms and requiring a special treatment. It is characterized by a *narrow* fissure existing between the folds of the mucous membrane upon a level with the constrictor muscle, and invariably causing intense pain during and after defecation, and according to Boyer, and others, constantly accompanied with contraction of the sphincter. It is important here, to determine whether the contraction of the sphincter ani is the cause or the effect of the fissure, for the character of the agent employed to attain a cure, will of course be determined by the opinion that is entertained of the cause. The correctness of Boyer's views is confirmed by the fact indicated by himself and others, that these symptoms do exist without any fissure, and that they are relieved by an incision of the fibres of the sphincter ani. The subcutaneous incision of the muscular fibres, proposed by M. Blandin himself, would induce the belief that if the contraction is not the cause, it is at least the most important symptom, for this once relieved, the fissure is cured. Indeed, the suspension of the contraction of this muscle, has been the aim of nearly all surgeons who have proposed a remedy.

In admitting, then, the theory of Boyer, the occurrence of the fissure could thus be accounted for. The contraction of the sphincter ani, of course narrows the anal orifice, and the indurated faecal matters passing over a highly irritable surface, tend to augment the contraction and increase the difficulty of their exit. Under such circumstances, the expulsion of the faeces cannot be effected without painful distention, friction, excoriation, and even laceration of the mucous membrane, which has to bear the whole force of the efforts.

Those who oppose Boyer's opinion, assert that it is not possible for this muscle to contract without some appreciable cause. It is however, admitted by all, that some muscles do contract without any known cause; as those of the eye in strabismus, those of the extremities, &c. Why, then, may not this muscle as well as others, be induced to contract by an inappreciable influence? M. Velpeau, in his article (*Dict. de Med.*) takes the intermediate ground. He asserts, that sometimes the contraction is the cause, and at others, the effect.—That it may exist without a fissure, and is then produced by an unknown cause; that it may precede and cause the fissure, or finally, that it may be a consequence of the fissure. In the last case, the contraction would be caused by an irritation occasioned by, or commencing in the fissure. In the first two instances, the contraction of the muscle is evidently the cause of the phenomena that present themselves, and in the third, in which the contraction is consecutive, it keeps up the fissure by the opposition that it offers to the exit of the faeces.

Hence, in every case, the attention of the surgeon should be directed to the condition of the sphincter ani, and, if contraction exist, it should be relieved.

TREATMENT.—Various remedies have been proposed in the treatment of this affection, some of which, were directed to the cure of the fissure, without regard to the condition of the sphincter. The three processes most frequently resorted to, are cauterization, incision and dilatation.

Cauterization would, in all probability, prove beneficial only in the first two varieties cited by Blandin, or in simple fissures without contraction of the muscular fibres. The object of this

process, as declared by Beclard, its author, was to change the nature of the ulcer, and to increase its tendency to heal. It has not been very extensively used. Incision was proposed and regarded by Boyer, as infallible in this affection. In other hands, however, it has not only failed, but in some instances has proved fatal. His operation consisted in making a free incision from the anal orifice through the fibres of the sphincter, including the skin, the mucous membrane and the adjacent tissues. These tissues are exceedingly vascular, and by incising them, a large number of blood vessels, and particularly veins, would necessarily be cut, which would render the patient very liable to phlebitis. This is then, a serious operation, as it is sometimes fatal and does not always afford relief. Velpeau declares that it should be made the last resort.

M. Blandin next proposed and practiced successfully, a subcutaneous incision of the fibres of the sphincter. This, he says, is attended with a very small wound, which always heals by the first intention and does not expose the patient to phlebitis. There is, however, a great objection to this operation, which is, that the muscular fibres are cut with great difficulty, as there is nothing to indicate to the surgeon when all the fibres have been traversed, and when he should withdraw the knife. In consequence of this difficulty, there have been many cases of unsucess attending the operation. Many surgeons still prefer the operation of Boyer.

Dilatation was recommended by Recamier and practiced by himself, Marjolin, and others. His process was to distend the sphincter by means of lint forced into the anal orifice. This operation was very properly objected to, on account of the intense pain that it caused. It subjected the patient to very acute suffering during several days, or during the whole time of its application. For this reason it was abandoned. Dilatation, but upon a different plan, has been revived by M. Maisonneuve with much advantage. The process he recommends, is to introduce the index fingers of both hands into the anal orifice, and to dilate forcibly the contracted muscle, first in the antero-posterior diameter, and then transversely. This simple, and almost instantaneous operation, removes the cause or the most important feature of the disease. If this operation proves to be

effectual, it will combine many advantages. In the first place, there is no cutting instrument, to frighten the patient; in the second, there is no wound to heal; in the third, there is no danger to be apprehended from phlebitis; and lastly, the objection against Recamier's process can not be urged here, as the pain is but momentary, and even this momentary suffering may be avoided by anesthetics. This operation rids the fissure of its complication, reduces it to a simple ulcer or excoriation, and places it in the most favorable condition for healing. These are no trifling advantages and should not be disregarded by the surgeon.

Having witnessed two operations of this kind, I will add a brief account of them from memory:

The first case was that of a middle aged man, who had entered the wards of M. Nélaton (Hospital St. Louis, Paris,) for treatment. He was a man of good constitution and, save this affection, was enjoying excellent health. He stated that he had been thus affected for several months, during which time a variety of remedies had been proposed and applications made, without affording relief—that at first he suffered intense pain during and after the act of defecation, but that recently the pain had increased, and instead of being limited to the period of defecation, had become continuous. So annoying had the pain become, that he could neither stand nor sit, nor could he lie down, save upon the side, with his legs flexed. M. Nélaton administered chloroform and performed the operation of dilatation as above described. The next day the patient was better. The continuous pain had diminished to a very great extent, and he was enabled to go to stool with much less suffering. He continued rapidly to improve, and left the hospital perfectly relieved in four or five days.

The second, was a man in the ward of M. Michon. (Hopital de la Pitié.) He had suffered under the affection comparatively a short time. He, however, experienced excruciating pain during and after defecation. Chloroform was administered, and the operation performed in the mode above mentioned. The following day the patient stated that he was entirely relieved, and could go to stool without the least suffering, and was consequently dismissed.

It will be seen from what has been stated, that although dilatation is not a new practice, the peculiar process of M. Maisonneuve is both novel and important. The object of this paper is therefore to direct attention to this improvement in the treatment of an affection often quite intractable under former management. To relieve the contraction of the sphincter ani has always been the object of surgeons. It was to attain this end that Recamier proposed his mode of dilatation—that Boyer proposed his operation of incision—that Blandin proposed his subcutaneous incision; and finally, that Maisonneuve proposes his new method. The latter operation is the most simple, and is certainly attended with the least danger to the patient. We hope, therefore, that its efficacy will be tested by those in our country who may have an opportunity of doing so.

ARTICLE IX.

Successful Case of Cæsarian Section. By H. M. JETER, M.D.,
of Buena-Vista, Ga.

On the night of the 4th of December last, Mrs. B., aged 30 years, was taken in labor with her sixth child. I was called at 3 o'clock in the morning to attend her accouchement. I found her with weak and irregular pains, and was informed that such had been their character from the commencement of labor at 9 o'clock. Soon after, however, her pains began to increase, and upon examination per vaginam, I found the os tincæ well dilated and the waters collected in large quantity. The back of the fœtus presented, I waited until the mouth of the uterus was more completely dilated, and ruptured the membranes discharging an immense quantity of water. I proceeded immediately to turn, so far as to bring down the breech, making a breech presentation. The difficulty attending the operation of turning was so great, in consequence of the extraordinary size of the child, (it being very large,) that I regarded it hazardous to attempt to complete the operation, and left it in this situation, to the natural efforts of the womb. After about two hours of very hard labor the breech so far advanced as to enable me to bring down the feet. I essayed by every possible

means to assist the efforts of the uterus, by making all the traction upon the inferior extremities of the fœtus that was warrantable, being convinced that the fœtus was dead.

Finding all efforts to make any further advance in its delivery entirely fruitless, I attempted to perforate the cranium, but found it impossible, in consequence of the size of the child, to pass the perforator up to its head. I then eviscerated the fœtus, with the view of passing the instrument up within the cavity of the fœtal thorax to the base of its cranium. This also failed to make room for the operation, without proceeding at random and great consequent hazard to the mother, as I could not insert the hand to give any certain direction to the instrument, the head still being entirely above the superior strait. Embryotomy was therefore determined upon, and after dissecting away the fœtus up to its axillæ, which required about two hours, the mother all the while suffering the most severe labor, but which at this time had ceased to make any impression upon the child, and which induced me to conclude that the uterus had probably ruptured. I found her rapidly sinking, so much so, indeed, that we did not think that she could survive fifteen minutes longer. I determined at once to operate by the *cæsarian* section. I gave my patient a stimulant, and, assisted by Dr. Reese, proceeded to make an incision along the *linea alba* six inches in length, cutting down carefully to the peritoneum, upon dividing which, the head of the fœtus presented, showing that my apprehensions were correct in the womb's having ruptured some time previous to the operation. The head of the child was so large that the incision had to be extended to ten inches in length to admit its passage. The head measured twenty-nine inches and four lines in its longitudinal or occipito frontal circumference, and twenty-eight inches two lines in its perpendicular circumference, being hydrocephalic. The head and remaining portion of the body being removed, the placenta was found also without the uterus within the cavity of the abdomen, and the uterus contracted to about the size of a small cocoa-nut. This being also removed the cavity of the abdomen was left filled with coagulated blood, from the hemorrhage which took place at the time of the rupture of the womb. Having carefully removed the blood, as completely as possible, the wound

was closed by the interrupted suture and adhesive straps, leaving a space of about two inches at its inferior extremity, for the discharge of the fluids that might remain or collect in the cavity of the abdomen.

Stimulants were given, and other applications made, to revive the sinking energies of the patient, which had become almost extinct. Reaction soon took place, and she was cleansed and placed in as comfortable a position as the circumstances would admit. The vital energies having been sufficiently resuscitated, opiates were given freely. She was kept quiet, and rested comfortably during the night and the following day, until about 9 o'clock on the succeeding night, (the 6th,) when she was taken with violent vomiting, which continued, with intermissions of not more than half an hour, until 10 o'clock the next day, at which time I arrived, having been called off the evening before, and could not return sooner.

We succeeded in soon checking the vomiting, and she rested easy, with occasional return of the vomiting during the day and following night.

Dec. 8th. Patient complains of some soreness about the womb, and over the abdomen generally, which is considerably swollen; pulse 132, and very restless. Administered a glyster, which produced two evacuations; gave her to drink small quantities of cream of tartar and lemonade.

Dec. 9th. Patient complains of great soreness and tenderness of the abdomen, which is greatly swollen; tongue dry and red, and great thirst; pulse 140, quick and hard; lochial discharges ceased. Put her upon a treatment of calomel and opium; gave injections of warm milk and water per vaginam, and applied flannel, wet with spts. turpentine, to the abdomen.

Dec. 10th. Soreness and tenderness not so great; discharges from the wound in the abdomen free; lochial discharges also free; pulse 136. Gave glyster, which produced one evacuation.

Dec. 11th. Soreness and swelling still subsiding; pulse 130, more soft and full; lochial discharges and those from the wound in the abdomen continue freely. Gave small quantities of Dover's powder.

Dec. 12th. Had one copious alvine discharge during the night,

of natural consistence—says she feels much relieved since. The discharges all continue free; pulse 130; wound healing kindly. From this time, nothing of importance occurred to require noting. She continued to improve, and by the 18th day after the operation the wound was entirely healed.

I visited her yesterday for the last time, which was the 29th day since the operation, and found her sitting up by the fire, directing the domestic affairs of her family. Mrs. B. is a woman of apparently very feeble constitution, and had been confined to her bed, for two months previous to her labor, with general anasarca of the whole system.

ARTICLE X.

Observations on the Law governing the Distribution of the Striped and Unstriped Muscular Fibre. By HENRY F. CAMPBELL, M. D., Demonstrator of Anatomy in the Medical College of Georgia.

On a review of our knowledge concerning the intimate nature of muscle, we find the following facts comparatively well established:—1st. That a muscle is composed of an immense number of fibres, bound together by areolar tissue in such a manner as to constitute fasciculi, presenting the greatest irregularity of dimensions. 2dly. When one of these fasciculi is torn with pointed instruments, so as to separate it into fine shreds, and examined under the microscope, some of the elementary fibres present a great number of transverse markings, known to microscopists of the present day under the name of *stripes* or *striæ*, giving to the fibre, according to their presence or absence, the name of *striped* or *unstriped* fibre.

In relation to the exact nature of these transverse striæ, there is even now some dispute, and two theories contend for the explication of the appearance. The one advanced by Dr. Barry, in a paper read before the Royal Society,* wherein he attributes the microscopic phenomena to the existence of a spiral fibre enclosed within a tubule of certain form, the windings of this fibre and the intervals between them constituting

* Paper on Fibre. (Proceedings of the Royal Society, No. 51.)

the light and dark stripes. The other and more generally received theory, is that of Mr. Bowman, of Kings College, who describes these striæ, as the result of the linear arrangement of particles, which he terms *elementary sarcous*, within the calibre of a tubule of peculiar tissue, called by him Sarcolemma, and that by the unequal refraction of light upon these particles and their connecting structure,* the striated appearance is presented. In addition to these, there are also longitudinal stripes which mark the lateral collocation of the sarcous particles, and which separate the fibre into its component fibrillæ. The unstriped fibre is paler, less developed, and, as indicated by its name, destitute of the striated appearance characterizing those above described.

The influence which the existence of one or other of these kinds of fibre exercise upon the function of the muscles they constitute, has been variously estimated; the weight of authority tends to corroborate the opinion, that the *striped* fibre is for *voluntary* and the *unstriped* for *involuntary* motion: hence their synonyms voluntary and organic fibres. But to this rule, it is fully agreed by all, there are met several exceptions wherein the striped (or voluntary fibre, ordinarily) enters fully into the composition of muscles known to be entirely involuntary. These exceptions are the heart, the upper portions of the œsophagus, and the pharynx. Now, of these three muscular structures, we find no other disposition made by authors than the bare statement that they are exceptions to the law; *that the striated fibre enters into the formation of voluntary, and the non-striated into that of the involuntary muscles*. The object of the present remarks is, with due deference to the views of others, to attempt an explanation of those exceptions by the substitution of another law, which, we think, fully as apparent as the above, and to deduce therefrom conclusions in regard to the function of those fibres, which will be found to differ from those ordinarily admitted. According to the view we take of the respective functions of the striped and unstriped fibres, these

* It seems more probable that the connecting material is less dense, and fills up every interval; but I do not pretend to determine what may be its nature, or whether it differs chemically from the parts it serves to join.—(W. E. BOWMAN. Philosophical Transactions, 1840.)

three structures no longer form exceptions, which are ever unnatural and inconvenient in the arrangement of the otherwise consistent and unerring laws of nature, but are thus constituted in obedience to a *law*, the most constant and invariable,—which law may be thus enunciated. *Wherever celerity and quickness of action* is required in a muscle, *under any circumstances*, we find the *striped fibre* entering into its composition, *without any regard whatever to voluntary or involuntary motion*; and, further, that the voluntary or involuntary character of the muscle has no influence at all on the distribution of these two kinds of fibre. In the establishment of this view, we find arguments abundant throughout the whole muscular system. All the muscles of voluntary motion, it is true, are constituted of the striped fibre; for these are liable, at any time, to be called upon for the most rapid and energetic contraction. On the other hand, the involuntary muscles are found of an unstriped fibre, with the above-mentioned exceptions; but these exceptions, we think, sufficiently important to invalidate materially the law which refers this distribution to voluntary and involuntary function. In offering the suggestion above made, we have in view the substitution of it as a law which can be of universal application, for one which is embarrassed by exceptions, which this readily and, to our mind, satisfactorily reconciles.

That the heart is strictly an involuntary muscle, no one will, at the present day, pretend to deny; for the few, we may say the single instance on record of its subjection to the will, is not sufficiently well authenticated to obtain a full admission. Its contractions, we know, are (for very important interests) of the most sudden and spasmodic character, and these, attended by momentary relaxations, are continued during the lifetime of the individual. The pharynx and œsophagus, other involuntary structures, are also at any time liable to be called upon for the greatest celerity of action: indeed, at this point in the alimentary canal, did the bolus move with a tardy and gradual descent, death would necessarily ensue from prolonged occlusion of the glottis by the epiglottis, while in impeded deglutition a somewhat spasmodic action seems necessary for the removal of the difficulty; and how fatal would be the result did

the heart's action at all resemble the rhythmical though energetic contractions of the pregnant uterus. Now the function of the womb is such, that of it is required the most energetic and prolonged contractions—contractions that at no time at all resemble in any respect the momentary impulses necessary in the office which the heart performs; so with the bladder, as well as the rectum and the rest of the alimentary tube, besides many other structures, as the dartos, the parenchyma of the spleen,* and probably the female nipple,† with other fibrous textures. These latter requiring but a very slight degree of motion, the fibres are extremely scattered, and resemble more, both in their appearance and the character of their contractions, the yellow fibrous tissue, which is found very widely distributed, than true muscular fibre of either the striped or unstriped variety; and thus will we find, by an attentive observation of the subject, that in proportion as any organ is required in the performance of its functions to exercise rapid or gradual contractions, so will it be constructed of striated or non-striated fibres. This law, so far as we have observed, is *invariable*—without exception—and we doubt not, will be strengthened and borne out by a comparison of the muscular systems of various animals throughout the scale whenever the necessary time and labor is devoted to this kind of investigation.

* W. Stukely, On the Spleen, its Description and History, Uses and Diseases London: 1722. Also, Prof. Kölliker, in an article on the Spleen, in the *Cyclopædia of Anatomy and Physiology*, avows their existence in the Spleen of the Pig and of the Dog.

† The erection of the Penis may be in part owing to the compression exerted on the superficial veins of the organ by a continuation of a structure analogous to the dartos, which is continued over the base of the penis under the skin. The erection of the nipple also occurs on any mechanical irritation, with a motion so very like muscular contraction that a layer of these fibres might perhaps be found under the skin of that region.—*Todd & Bowman's Physiological Anatomy*.

PART II.

Eclectic Department.

A Synopsis of the Spinal System. Being the Croonian Lectures, delivered at the Royal college of Physicians, London.
By MARSHALL HALL, M. D., F. R. S., &c., &c.

LECTURE III.

GENTLEMEN,—I have, in my first and second lectures, treated my subject experimentally and physiologically. I propose in the present one to show you how the principles so unfolded have their part in pathology, and their application in therapeutics and in *practice*.

The first statement which I have to make is full of practical importance. If in any animal you denude the brain, the cerebrum, and the cerebellum, and irritate, puncture, lacerate, or injure the tissue of these organs in any way, however violently, there is absolutely *no movement* of the animal or of its limbs—*no spasmodic or convulsive effect*!

These organs are *in excitor*!

Is it so in the human subject? But a few days ago I was summoned to treat a patient who had just suffered from an attack of decided hemiplegia affecting the face, the arm, and the leg. With this *paralysis* of motion, not the slightest *spasmodic* movement had occurred. The human brain had been lacerated, yet no spasm had been produced. This event is perfectly usual. It is the subject of daily observation.

The same fact has been observed in surgical practice, in accidents, in operations, in regard to the encephalon. The human brain, like the brain of animals, is *in-excitor*.

How excitable the medulla oblongata and the medulla spinalis are, I have already shown you.

From these facts, and from long and careful observation, I draw this conclusion—that

No disease of the cerebrum or cerebellum, LIMITED in themselves or their effects to the cerebrum or cerebellum or both, is attended by spasmodic or convulsive movement.

But another fact is not less certain. Diseases of the cerebrum or cerebellum *do* produce spasmodic or convulsive movements.

How are these facts, apparently so contradictory, to be reconciled and explained?

Observe these engravings of the base of the brain and of the cranium. Here are membranes supplied, according to the discovery of Arnold, by *roots* of the trifacial nerve; here is the intra-cranial portion of the trifacial nerve itself. In this spot

you see the intra-cranial portion of the pneumogastric and its accessory ; and here you see the medulla oblongata, all portions of the spinal or *staltic* system, all most excitable. Now if there be a disease of the cerebrum or of the cerebellum, which, by its contact or pressure, or, if distant, by counter-pressure, can affect any one or more of these structures, spasm, or convulsion will be the result—the inevitable result.

Disease of the cerebrum or cerebellum may therefore induce spasmodic or convulsive affection, through the medium of the staltic or spinal system ; but it can produce these effects in no other manner.

I repeat, then, the observation which I have before enunciated,—

Disease of the cerebrum or cerebellum can induce spasm or convulsion only through the medium of the spinal system.

I may add another important *aphorism*:

It is in the same manner exclusively that disease of the cerebrum or cerebellum can be attended by affection of the spinal system of any kind. It is in this manner, and in this manner only, that dysphagia, and stertor, and other affections of the respiratory movements, and affections of the sphincters can be induced—a fact of immense value in the *Diagnosis* and the *Prognosis* of these diseases.

If in apoplectic affections, dysphagia or dyspnœa is the result, the spinal system is affected ; if, in spite of our remedies one or both of these symptoms continues, the disease will prove fatal.

I beg here to draw your attention to the diagnosis between paralysis and spasm-paralysis. In the former case it may be cerebral or spinal, but if spinal the spinal centre must be either absolutely *destroyed*, or so affected, as by *exhaustion* of its power (as by sexual excesses) as *not* to be affected by *irritation*. In the latter, or spasm-paralysis, there must be *irritation* of the spinal system.

In a recent case of paraplegia, affecting both arms and both legs there had been the perfect absence of all spasmodic symptoms from the very commencement. You will remember, gentlemen,—you can never forget,—the effect of strychnine in inducing exalted excitability and tetanoid paroxysms in the frog. I believed I had, in the case to which I have adverted, a case of impaired spinal excitability. I prescribed the *fiftieth* part of a grain of the acetate of strychnine to be given four times a day. In ten days the patient had recovered, in a marked degree, the power of walking and of writing. The amendment has continued to be progressive.

This case was sent to me by Professor Sharpey. It occurred

in a member of our own profession, Dr. Livingston. It is the *only one* in which I have given strychnine with a satisfactory result.

I imagine the principle which may guide us in the administration of this powerful remedy is this: Is there spasm? then there is irritation, and strychnine could only, by adding to the excitability, add to the effect, of this cause of morbid action. But, if there be no spasm, if there have been no spasm from the beginning, then the paralysis, the paraplegia, may depend on mere nervous exhaustion of the spinal centre, and strychnine may repair this exhausted energy, may restore the excitability and prove a remedy.

The case and the remedy must be equally specific.

I think the *mercurial* treatment appropriate, on the other hand, to the case attended with spasm generally speaking.

The next fact, not less important and diagnostic than the first, is this; no affection of the spinal system, no lesion or irritation, however attended by spasm or convulsion, affects the cerebral system necessarily. Even in tetanus itself, in which every muscle in the frame is thrown into spasm, there is, in the early part of the disease, no cerebral affection. The patient is perfectly conscious. There is no delirium; no coma; pain, dreadful spasm keeps the patient from sleep; but the cerebral system itself is unaffected.

The same phenomenon is observed in other cases of disease of the spinal system, as in all experiments, limited in themselves or their effects, to this system. In chorea, in hysteria, in the paralysis agitans, there is the absence, in the commencement, of *all* cerebral affection.

Hydrophobia, or the effect of strychnine, however violent, is still unattended by cerebral symptoms.

In experiment, irritation of the spinal system from the medulla oblongata to the lowest part of the medulla spinalis inclusive, induces spasm without any evidence of cerebral symptom.

What, then, are the special cases, in which diseases of the spinal system *are* attended by cerebral affection?

In order that the cerebral system may become implicated in affections of the spinal system, limited in their *physical* lesion to this system, another, and a newly detected element must be added.

The nerves and muscles of "The Neck" must be so specifically implicated as to induce the condition which I have designated Trachelismus with its consequences, compression of the veins of the neck, and congestion of the tissue and organs of the head.

This is the *essential* link between affections of the spinal system and affection of the cerebral system!

This contraction of the muscles of the neck is absolutely special. It is not less than that by which, in several cases the tongue is protruded and bitten. It is not less so than that by which the larynx is so frequently closed. It is especially apt to produce turning or twisting of the neck to one side—a frightful *torticollis*.

The *muscles* chiefly affected are the platysma myoides, the sterno-cleido-mastoid, the trapezius, especially the more deeply seated omo-hyoid with the splenii, the scaleni and *other* muscles of the neck, too deeply seated to be observed.

The *nerves* principally involved in this action are a *descending* branch of the facial, distributed to the platysma, the *accessory* to the sterno-cleidomastoid and the trapezius, the *descendens noni* to the omo-hyoid, with the *recurrent* to the larynx—nerves, with the *vagus* the *diaphragmatic*, of such singular course.

By the contraction of these muscles, under the influence of these nerves, the jugular and the vertebral veins of the neck are compressed, and the face, the encaphalon, and the medulla oblongata are congested, with the addition to the other symptoms of those which constitute the *Epileptoid* class—viz., those which affect the encephalon—the cerebrum and the medulla oblongata—a view which constitutes a remarkable step in the pathology of this *Class of Diseases*.

It is as obvious that contraction of the posterior portion of the platysma myoides will compress the external jugular, and that of the sterno-cleido-mastoid and the omo-hyoid, the internal jugular, as that the action of the genio-glossal will protrude the tongue, and that of the constrictors of the larynx will close the glottis.

It is an extraordinary fact, that the *same* nerve should distribute branches to the genio-glossal and the omo-hyoid. Is it thus that the protrusion of the tongue and the compression of the *jugular* vein are accomplished, and the pathognomonic symptoms of epileptoid affection are produced?

How important, with these views, would be a *new* dissection of, and new experiments on, the nerves, muscles and veins of "*The Neck*."

Through this region of *The Neck*, the *Larynx* inclusive, all truly *Epileptoid* symptoms are produced. These may vary from the slightest and most transitory "*oblivium*," to the deepest and even fatal *coma*, with all the agonizing and terrific symptoms which may intervene—from "*le petit mal*" to "*le haut mal*" of French authors.

There is a gentleman, a physician, now amongst you, who has crossed the Atlantic to bring his little boy, a twin, that I

may investigate his malady. The day before yesterday, this little boy was sitting before me, whilst his father and myself were conversing earnestly about his case. Suddenly, I observed a flush to steal over his pallid countenance, the head, eyes, and features to be in a fixed state, the pupils dilated, with loss of recollection or consciousness. In a few minutes, these symptoms disappeared as they came on, and the interesting little patient was as before!

What strange event had occurred to induce these phenomena? What deep and hidden and mysterious agency had been called into sudden play? From careful, and carefully repeated *observation*, I can affirm, that this agency is that of the *nerves*, and *muscles* and *veins* of "*The Neck*!"

This little boy has been, and is still, subject to epileptoid attacks of a most violent character. The difference arises, as in so many instances, from difference of degree of the actions of the muscles of the neck, it may almost be said, from difference in the degree of the torticollis.

Do you ask me what is the rationale of this trachelismus? I reply by asking—What is the rationale of the closure of the larynx, or of the bitten wound of the tongue? All are equally the objects of *observation*. The reason why special muscles are made to contract in each of these cases is unknown, and will probably long remain so.

This *Class* of diseases occurs in all ages—from dentition, in infancy; from gastric, interic, hysteric irritation, in adult age; and from peculiarities of the parturient or the puerperal state.

I class all these cases together. I do so advisedly; I do so to get rid of the formidable, injurious, and often misapplied term, *epilepsy*. I would propose to designate the whole class by the term

Trachelismus.

This term will speedily be as familiar to us as that of Laryngismus; and I will leave it to you, gentlemen, to imagine the advantages, in every respect, of such a change of nomenclature, merely observing, that whilst the very fact and truth of the case is so expressed, the patient is preserved from a term full of painful stigma and ignominy.

The effects of this trachelismus with its phlebismus, divide themselves into two classes,—

1. *The Cerebral,*
2. *The further Spinal.*

The *first* involves every cerebral symptom, from oblivium, "falling," every morbid symptom connected with the *senses*, a frightful scream, coma, delirium, or subsequent mania, transient and paroxysmal paralysis, &c., &c.

The *second*, a bitten tongue, laryngismus, terrific and violent distortion of the features, the neck, the general frame,—general convulsion,—expulsion of urine, fæces, semen, &c.

Whilst the internal cerebral congestion varies, in this manner, in every degree, the countenance is sometimes merely gently flushed; sometimes, with the neck, intensely tumid and livid, and sometimes, especially about the eyelids, affected with minute ecchymosis—the *type* of what is occurring *within*!

Besides the well-known epileptoid affections—"le petit mal," "le haut mal," &c.,—there are other *paroxysmal* affections not usually classed with them. There are paroxysmal paralysis, paroxysmal mania, paroxysmal syncope, and *seizures*, which from occurring in the night, or from home, and passing away, remain "hidden," producing *effects* which are, for a time, mysterious and inexplicable. I have no doubt, aberration of mind or paralytic weakness, the effect of such a *hidden* cause, and I think *crime*, which has been perpetrated without motive, leading to fearful judicial results may have been of this character.

But I think that apoplexy itself and hemiplegia are not the unfrequent effect of such action of the muscles of the neck. In illustration of this remark I may here adduce a singularly interesting extract from the work of Dr. Abercrombie, on a subject which we are now, I think, prepared to understand:

The most simple illustration of the apoplectic state is derived from those cases in which it is distinctly traced to an external cause, and ceases when that cause is removed. A boy mentioned by Zitzilius, had drawn his neckcloth remarkably tight, and was whipping his top, stooping and rising alternately, when, after a short time, he fell down apoplectic. The neckcloth being unloosed, and blood being drawn from the jugular vein, he speedily recovered. Strangulation, when the neck is not dislocated, appears to be simple apoplexy.*

"To such cases as these we may add the numerous examples in which persons fall down suddenly in a state of perfect apoplexy, and very speedily recover under the appropriate treatment, without retaining any trace of so formidable a malady.

"The apoplectic attack, as it occurs in such examples as these, must be supposed to depend upon a cause which acts simply upon the circulating system of the brain, producing there a derangement which takes place speedily, and is often almost as speedily removed. *What* the precise nature of that derangement may be, is a point of the utmost difficulty to determine,

* It is, rather, apoplexy in the first moments only, afterwards it is epilepsy.
M. H.

and perhaps, we have no data upon which it can be determined by legitimate induction."

These data, formerly so obscure, have, I think, been furnished in this lecture.

Dr. Abercrombie adds, in his interesting work,—“This (apoplectic) attack itself occurs chiefly under three distinct forms, which it is of importance to distinguish from each other.

“In the first form of the attack, the patient falls down suddenly, deprived of sense and motion and lies like a person in a deep sleep; his face generally flushed, his breathing stertorous, his pulse full and not frequent, sometimes below the natural standard. In some cases convulsion occurs; in others, rigid contraction of the muscles of the extremities, and sometimes contraction of the muscles of the one side, with relaxation of the other,” &c.

How epileptoid!

“The second form of the disease begins with a sudden attack of pain in the head; the patient becomes pale, sick, and faint; generally vomiting, and frequently, though not always, falls down in a state resembling syncope; the face pale, the body cold, and the pulse very feeble. This is sometimes accompanied by slight convulsion,” &c. “In some cases, paralysis of one side occurs; but in others, and, I think, the greater proportion of this class, no paralysis is observed.”

Here again how epileptoid is the apoplectic or paralytic seizure! The case is, in fact, a formidable kind of “sick headache;” such sick headache sometimes issues in decidedly epileptoid affection. It is the *type* indeed of this *Class* of affections.

But to return to the subject of trachelismus. This is obviously the condition, the necessity for which Dr. Abercrombie plainly felt, as the essential link between a certain class of causes and the apoplectic or paralytic affection. It is not therefore confined to epilepsy. It rather suggests the classification of different forms of Trachelismus according to its more prominent effects, whether these be apoplectic, paralytic, convulsive, spasmodic, syncopal, &c.

It is obvious that the whole *Class* of the diseases of which trachelismus forms a part, must be distinguished from those which arise from, or consist in, organic change of structure of the nervous centres simply.

Disease of the tissues of the cerebrum or of the spinal marrow may be of this latter class. They are recognised by a previous knowledge of anatomy, physiology, and *experiment*, and by observation and experience.

Diseases involving trachelismus are of a very different character. Perhaps I cannot conclude these lectures better than

by a brief view of several of them. I will first present you with a

Sketch of Laryngismus.

Not many years ago this affection was supposed to be a disease of the brain. It is now well known to be a malady of which the *cause* is variously seated in the alveoli, the stomach, the intestine, of which amongst the earlier effects are strabismus, laryngismus, a contracted hand, a contracted foot, spasmodic affection of the sphincters, &c.; and amongst the remoter, distorted features, trachelismus, generally torticollis, actual closure of the larynx, and convulsion more or less general, dilated pupil, insensibility; lastly, congestion of the encephalon, with eventual effusion, as an ultimate *effect*

The course of these events is very interesting. It is the reverse of that which attends primary disease of the encephalon. In this, the cerebral symptoms precede the spinal generally. We frequently see strabismus, and especially vomiting, before any cerebral symptom manifests itself, the cause or source of irritation being probably seated in the arachnoid.

A very interesting question arises here. Are children who have been the subject of convulsive affection peculiarly apt, in remoter years, to become the subjects of epileptoid affection? Does this question involve the question of the *special cause* of convulsive affections, whether dentition, or gastric or enteric irritation.

I am of opinion that the case of convulsive affection arising from fright is to be totally distinguished from all others—(and how many cases arise from this cause unsuspectedly?) It is altogether more difficult to remove, and more apt to return, and more apt to lead to epileptoid affection in after life, than the other cases to which I have adverted.

The effect of emotions of a lighter kind in inducing fits of strabismus and of laryngismus is well known.

The powerful influence of the easterly or north-east wind on the ordinary cases of laryngismus should be carefully observed, as should also the beneficial influence of change of air.

There is one *form* of this malady which should be borne in mind. It is very apt to end in *sudden death*, and this especially in certain seasons, probably those in which the same north-east wind prevails.

Does this wind act on the excitable borders of the larynx, in inducing the repetition of attacks of laryngismus, and of laryngismus in its fatal form? Or is that fatal result a species of syncope, the effect of a morbid influence exercised by the medulla oblongata on the action of the heart, and similar to the syncopal seizure in the adult?

How naturally do these views suggest the proper modes of treatment? All sources of irritation, all the exciting causes, must be carefully removed and avoided.

Sinapisms applied along the spine have proved, in the hands of Mr. Higginbottom, of signal benefit, acting, as I imagine, by subduing augmented excitability of the spinal centre; for it is to be observed, that the nerve irritated in dentition, or gastric or enteric disorder, is, like the wounded nerve in traumatic tetanus, not only a source of continued excitement, but of augmented excitability.

It is singular, that in the convulsive affection of infants the tongue is not thrust out as it so frequently is in those of adults. The *larynx* and "*the neck*," however, are not less affected, and whilst the designation laryngismus (a mere *symptom*) has already been given to the disease, that of trachelismus, not less applicable to it, may be reserved and appropriated to designate the *congeneric* affections of adult age.

Having thus given a sketch of the former interesting ailment, I beg to add

A Sketch of Epilepsy or Trachelismus,

not only of that epilepsy which arises from defective development of the head, and of that which arises from encephalic disease, but also of that form of epilepsy which arises from irritation seated at a distance from the centres of the nervous system.

In the encephalic epilepsy, there is some source of immediate irritation, or of irritation from counter-pressure of the esodic nerves in the membranes, or other nerves within the head, or of the medulla oblongata.

In the epilepsy of distant origin, this irritation is seated in the esodic nerves of the alveoli, the stomach, the intestine, or the uterus.

This irritation, like the injury of the nerves in traumatic tetanus, only in a less degree, constitutes in itself, and in its effect on the centre of the spinal system, undue excitability, and the *predisposition* to the attack; the slightest additional excitation leads to the attack itself.

This attack exhausts or diminishes the excitability in its turn, and with it the predisposition to attack, and the patient is generally secure for a time. But time restores the excitability and the predisposition to attack, and the patient is again in danger.

The immediate exciting cause acts on esodic nerves, through these on the centre of the diastaltic system; thence on exodic nerves, and ultimately on the muscular system; but not on this system *generally*, as in tetanus, but *specifically*, inducing special phenomena.

The character of this muscular action is expressed in one word—*contortion*. The eyes, the features, the head itself, are all variously drawn or contorted to one side; the limbs are turned and drawn in the same manner, and not rendered tense merely, as in tetanus. There is *torticollis*, or what I have since termed *trachelismus*.

This violent action of the muscles of the neck, this trachelismus, compresses the veins of the neck, and arrests or impedes the return of blood from the head, the external tissues, and the encephalon. The features, the integuments of the head and neck, and the encephalon, are livid and tumid, being gorged with venous blood. The effect is precisely similar to that induced by strangulation by a tightened cord. It is chiefly an affection of the jugular veins, though the lower part of the vertebral does not escape. I have hence termed it *sphagiasmus*.

There are three pathognomonic symptoms of this fearful affection: first, the protrusion of the *tongue*, which is frequently severely bitten; secondly, the closure, more or less complete, in the *larynx*, with forcible efforts of expiration; thirdly, *trachelismus* with its consequent *sphagiasmus*, and its consequences on the encephalon, *similar*, doubtless, in every respect, to what we see in the features—congestion, tumefaction, venous lividity, *ecchymosis*, &c., &c.

It is just as difficult to explain any one of these symptoms as the others. They are all equally the subjects of *observation*. The precise muscles affected in trachelismus are not all easily detectible. But the contraction of the platysma myoides, the sterno-cleido-mastoid, the trapezius, and even the omo-hyoid, is frequently either to be seen or felt, especially in thin persons. It is singular that the same nerve, the ninth of Willis, the eleventh of Arnold, should supply the genio-glossal and the omo-hyoid, and that another, the accessory, should supply the sterno-cleido-mastoid and the trapezius, whilst the intermediate nerve should supply the larynx. Has this anatomical disposition any relation to the phenomena and pathognomic symptoms of epileptoid disease.

It is obvious that the anatomy of the neck is fraught with extreme interest to the pathologist and the physician.

Irritation, then, special diastaltic action, trachelismus, sphagiasmus, a gorged encephalon,—such are the links of the chain of causes and effects in epileptoid disease. When the paroxysm is over, and the ligature taken from the jugulars, coma still remains for a time, gradually passes through delirium, perhaps, or through oblivium, into returning consciousness. This recovery is especially promoted by posture and by the sudden inspirations excited by dashing cold water in the face, the de-

gree of excitator influence of which is also a measure of returning healthy condition of the encephalon.

To remove sources of continued excitement of the esodic nerves,—to diminish excitability by exercise and excited inspiration,—to avoid all *causes* of excitation,—to remove residual congestion by sinapisms,—such are the indications for our treatment.

The mind must be preserved free from agitation,—the blood, if impaired, must be restored to its healthy condition,—dental, gastric, enteric, and uterine irritation, must be avoided.

Then, if ever, trial may be made of empirical remedies, such as silver, zinc, copper, Baron Sloet, the valerianates, indigo, cotelydon, sumbul, an issue in the arm, &c., *ad infinitum* :—all which, like mesmerism, homœopathy, &c., act by inspiring confidence in the patient's mind, being accompanied by assurances of their efficacy. Esquirol found that his patients were always better for a time, when he prescribed any new remedy.

But all this is, in reality, untruthful, derogatory from and unworthy of our profession, and belongs to the low art of quackery and imposition.

I will conclude these remarks by a short sketch :—

I was recently called to visit a gentleman in consultation with Mr. Martin. He was affected with continuous epleptoid convulsion of one side—one side of the neck, one arm and hand severely, and one leg slightly; the platysma myoides and the sterno-cleido-mastoid were seen contracting convulsively, whilst the omo-hyoid could be both seen and felt, in the posterior inferior angle of the neck, pulsating with clonic spasm.

We gave half a drachm of ipecacuanha and of the bicarbonate of potass, repeating them in twenty minutes. As nausea, sickness, and faintishness came on, *all spasmodic action ceased!* It returned in a slighter form as they subsided, but was never severe, as before.

The next day there was no spasm, but paralysis of the arm. This gradually subsided in its turn.

It must be admitted that this series of events is of the slightest interest. The convulsive attacks being only hemiplegic, there was only partial insensibility.

I must not conclude without adverting to the subject of *Puerperal Convulsion*. This form of convulsion is, happily, not designated by the formidable name of epilepsy, unless, indeed, it becomes *repeated* after all that relates to the puerpral state is passed by. Yet it is as much epilepsy as the convulsive attack arising from gastric, enteric, or the catamenial uterine irritation.

As in the case of laryngismus, there is one interesting ques-

tion? Is the puerperal convulsion apt to lead to subsequent epileptoid affection? And there is another question of the same kind—Is the patient previously subject to epileptoid affection particularly liable to become affected with puerperal convulsion?

It is obvious that puerperal convulsion, like the laryngismus of infants, and the epileptoid affections of adults, involves as an essential link of the chain of cause and effects, that condition of the muscles of the neck which I have termed *trachelismus*. There are also the closed larynx and the protruded and bitten tongue; the contortion of features, neck, and general frame; the same cerebral symptoms—coma, delirium, hemiplegic paralysis.

What an interesting subject for future elaborate research!

But the ultimate and crowning object of these studies is—the *Prevention* as well as the *Treatment* of these paroxysmal Affections. That this can only be based on a knowledge of their predisposing and exciting causes, and of their mode of action, must be obvious to every one. The physician becomes what he ought most to be—the watcher and the guide of his patient, whom he would lead from *every* danger and to *every* good. How does empiricism fade in comparison with this only just proceeding!

I here, then, draw these lectures to a close. I shall do so by requesting your attention to the following *Recapitulation* of the topics which have been laid before you.

Recapitulation.

1. The facts of Haller, and of Redi and Whytt, have been shown to depend on one and the same motor principles—the *vis nervosa* of the former author acting according to a newly detected *Law*, the *Diastaltic* unknown to him—nay, expressly denied by him.

2. These facts, previously merely *experimental*, having no application in science, have been shown to be the *type* of a *Class of Acts* in Physiology—viz., *all* those of ingestion, retention, expulsion, exclusion.

3. I have established, by new experiments,—

(1.) A diastaltic Law of Action of the *vis nervosa*.

(2.) A Diastaltic Nervous Arc, through which the power acts, consisting of

i. An Esodic nerve,

ii. The Spinal Centre,

iii. An Exodic nerve,

all in essential relation and connexion with each other, and constituting a new species of Anatomy.

4. I have established a *Series* of such Arcs, guarding

-
- (1.) The Orifices, } of the animal frame and governing
 - (2.) The Sphincters, } the Acts,
 - i. Of ingestion,
 - ii. Of Egestion, and especially of the vital function of
 - iii. Respiration, and all the important acts of Generation.

5. I have, in a word, established

- (1.) A Diastaltic Anatomy.
- (2.) A Diastaltic Physiology.

6. I proceeded to show you that the science of *Obstetrics* is a branch merely of Diastaltic Physiology.

7. As might be expected, I have traced the same principles in pathology. I have shown that *all* spasmodic or convulsive diseases are diseases of the diastaltic nervous system, and can *only* be understood when this system is understood, and that they assume chiefly *three* forms—

- (1.) The Diastaltic,
- (2.) The Centro-staltic,
- (3.) The *Cata*-staltic,

as their source of action is seated in an esodic nerve, the spinal centre, or an exodic nerve.

8. I have especially laid before you two propositions in pathology, of great importance :

- (1.) That no disease of the cerebrum or cerebellum can induce *spasmodic* or *convulsive* symptoms, except through the medium of the spinal system ;
- (2.) That no disease of the spinal system *can* affect the cerebrum, except through the medium of the larynx and the nerves, muscles, and veins of "*The Neck*."

9. In short, I have established a new and peculiar diastaltic pathology.

10. I have applied the same principles to therapeutics, the action of certain remedies, the treatment of a certain class of diseases.

But I must conclude. In doing so, allow me to invoke the spirit of cadour amongst you, and ask you, gentlemen, what you think of the diastaltic nervous system? Do you know of any inquiry, any discovery, so extensive, so important in its applications in physiology or in medicine?—any in which so much has been accomplished, and in which so much remains to be accomplished? Then accept my efforts in our common cause in good part. Let it be said hereafter, that amidst much opposition elsewhere, my labours were encouraged and promoted by the Royal College of Physicians of London.

[*London Lancet.*

Observations on the Use of Chloroform in conjunction with Ergot of Rye in Parturition. By THOMAS EDWARD BEATTY, M.D., M.R.I.A., President of, and Professor of Midwifery to the Royal College of Surgeons in Ireland, &c., &c.

The observations contained in the following pages were communicated to the Dublin Obstetrical Society on two different occasions, the last being the monthly meeting held in April of the present year. From the cases detailed, it will appear that I have been an early labourer in the field of investigation respecting the merits of chloroform as an anæsthetic agent in parturition; but I have purposely abstained from publishing the results of my experience in this most interesting and important research, until I had tested the accuracy of my conclusions by repeated investigations. In a matter like the present, where a variety of conflicting opinions are entertained, and where the usual, and even more than the usual amount of scepticism that attends the introduction of every improvement in medicine is found to prevail, it is the imperative duty of all who have had opportunities of ascertaining the value of the newly proposed means, to weigh well and consider in all its bearings the plan, or remedy, before the stamp of approval is placed upon it.

Actuated by such feelings, I have for a long time carefully investigated the value of chloroform as an anæsthetic agent in labour, and I have fully satisfied myself that it is possessed of the powers ascribed to it by the distinguished Professor, Dr. Simpson, who first employed and recommended it for this purpose; and moreover, that its employment, *when properly conducted*, is not attended with any injurious effects upon either mother or child. I have never seen any unpleasant result from it, and I believe that, out of the many thousand cases in which this agent has been employed in parturition, not a single case of death has occurred from its use. The immunity from danger in this class of cases may be ascribed to two causes, viz:—First, the position of the patient, and second, the condition of her stomach with respect to repletion. The horizontal posture is that in which the peculiar effects of chloroform are most easily produced, and in which consequently the least quantity is required to produce anæsthesia. That is the position in which it is always administered to the parturient woman; while in many surgical operations, and particularly in tooth-drawing, the patient is placed in the upright position, when a larger quantity of the drug must be used, and in such cases it is that fatal results have most frequently occurred. With respect to the second cause, viz., the

repletion of the stomach, it has been proved most satisfactorily, that if chloroform be administered when the stomach is full, dangerous effects are more readily produced, and fatal consequences are more likely to ensue. Now in cases of parturition, it seldom happens that the use of chloroform is commenced until some hours have elapsed after the commencement of labour, and during all that time the patient is little disposed to take any food, so that when the time arrives at which it may be deemed prudent to administer chloroform, the stomach is empty, and thus another cause of immunity from danger is secured to the parturient woman. These circumstances have, no doubt, contributed to save the class of cases under consideration from injurious effects, and ought to encourage the timid and wavering to lay aside prejudice, and be guided by sound principles and experience. Let it not be forgotten, however, that the grand desideratum in the use of chloroform is its purity. Without this there is no security against unpleasant consequences. The pure agent is harmless when properly administered; but the impure, with similar precautions in the mode of using it, is sure to cause cough, spasm of the chest, delirium, and excitement, before the anæsthetic effects are produced, and headache, with congestion of the brain, after. Various means have been proposed for testing chloroform, but they were all difficult in their application, and unlikely to be employed by persons in active and extensive practice.

To Dr. W. Gregory we are indebted for the description of a method whereby any one, however engaged in practice or unused to chemical investigations, can for himself test every drop of chloroform he employs by a simple process.

Dr. Gregory's paper was read before the Royal Society of Edinburgh in March, 1850, and was subsequently printed in the *Monthly Journal* of that city.* The paper is of great value, and should be carefully studied by all who are interested in the success of anæsthetic treatment. He ascribes the injurious effects of the chloroform in ordinary use to the presence of certain volatile oily impurities, which must be removed before it can be safely employed. These oils contain chlorine, have a disagreeable smell, and when inspired or smelt cause distressing headach and sickness. It is, therefore, highly probable that when these symptoms occur as they do with some individuals, from the use of chloroform of more than the average goodness of quality, they depend on the presence of a trace of these poisonous oils. The test which Dr. Gregory recommends for these impurities is agitation of the chloroform with

* *Monthly Journal of Medical Science*, p. 414, May, 1850.

sulphuric acid which should be quite colourless, pure, and of the full density of 1.840 at least. This, when agitated with the impure chloroform, becomes yellow or brown, from its action on the oils, which it chars and destroys. Any change of colour is easily seen by the contact with the colourless chloroform that floats above. Pure chloroform gives no colour to the acid. As this is a subject upon which too much stress cannot be laid, I will beg leave to transcribe Dr. Gregory's instructions for the purification of the adulterated drug:

"The chloroform having been tested as above, and found more or less impure, is to be agitated with oil of vitriol (half its volume will be sufficient), and allowed to remain in contact with the acid, of course in a clean, dry, stoppered bottle, and with occasional agitation, till the acid no longer becomes darker in colour. As long as the action is incomplete there will be seen after rest at the line of contact a darker ring. When this no longer appears, the chloroform may be drawn off, and for greater security once more acted on by a quarter of its volume of the acid, which should now remain colourless. It is now to be once more drawn off, and in a dry, stoppered bottle mixed with a little powdered peroxide of manganese, with which it is gently agitated, and left in contact until the odour of sulphurous acid is entirely destroyed, and the chloroform has acquired a mild, agreeable, fruity smell. It has then only to be poured off into a proper phial. It will now leave no disagreeable smell when evaporated on the hand.

"Mr. Kemp has observed, in repeating this process for me, the very curious fact, that as soon as the action is complete, and the oily impurities are destroyed, but not sooner, the chloroform, tested with the acid in a tube, exhibits a strongly convex surface downwards, where it rests on the pure acid, or, what is the same thing, the acid becomes concave at its upper surface. The smallest trace of impurity, not sufficient to affect the density of the chloroform, we have found to render the line of junction horizontal."

We have thus in our power a very simple means of testing and purifying chloroform before employing it; and if strict attention be paid to this most important point, I have little doubt much of the prejudice which still prevails against the use of this remedy will be removed. The purity of the drug being secured, the next important consideration is the manner of using it. I have always administered it on a pocket handkerchief, as was first advised by Dr. Simpson, and I see every reason to prefer this mode. It secures a due admixture of atmospheric air with the vapour of chloroform, a circumstance of great importance in the commencement of the process; for

the slow and gradual admission of the vapour into the lungs, when the drug is pure, produces a weak anæsthetic effect at first, which, however, by a perseverance in the inhalation, becomes more and more intense, but by slow degrees, and in a manner that is quite discernible by the administrator, and can be arrested at any moment by withdrawing the handkerchief. This command over the quantity and quality of the inhaled vapour cannot, I think, be attained in the use of any of the inhalers that have been proposed.

The best and simplest of them is that invented by Dr. Fleming of this city; and in operative surgery, where the deepest form of anæsthesia is required, it answers perfectly, and with a very small consumption of the fluid. I do not propose to enter into a detailed description of the method of using this agent; this has been done already by Dr. Simpson, and by Dr. Murphy of London, to the first of whom we are indebted for the introduction of this valuable addition to obstetric medicine, and to the latter for his able and impartial investigation of the subject. The practical and candid communication of Dr. Denham, published in this Journal, contributed in no small degree to place the value of this remedy in its true light.

I may just state generally that in ordinary cases I commence the administration when the os uteri is nearly dilated; then I pour about two drachms, at first, on the folded handkerchief, which is held at a distance of five or six inches from the patient's face as she lies on her side, and is slowly approached nearer and nearer until the edges of the handkerchief overlap the upper part of the cheek. That in ordinary labour I never produce insensibility, but as soon as the breathing becomes at all loud I remove the handkerchief and suffer the effects to subside, and then recommence the process. By this means the patient is never deprived of consciousness, but she is relieved from the agony of her labour pains, and also from that distressing and continuing ache in the back in the interval between the pains, of which some women complain so loudly. They are conscious of the uterine effort being made, and use the ordinary straining to assist it, but they suffer little or no inconvenience.

To be able to accomplish this with safety to the mother and her infant,—to hear a woman declare immediately after her delivery that her labour was heaven,—is no small triumph of art, and will be a lasting memorial of the genius and perseverance of Dr. Simpson. The stage of excitement which has been observed in some cases wherein chloroform was employed is, I have no doubt, attributable to the use of an impure specimen, and is not to be expected when the pure drug is employed

in the manner just described. As a proof of the happy effect produced upon patients treated after this fashion, I may state that those who have once used the inhalation have called loudly and early for it in subsequent labours.

That injurious and fatal results have followed the use of chloroform in surgical practice I do not mean to deny, though no fatal case has ever occurred in midwifery, principally for the reasons already stated; but in the majority of the unfortunate cases, some satisfactory reason will be discovered to account for what occurred. The upright position, a full stomach, an over dose, or an impure medicine,—these, or some of them, will be found to have been present in such cases, and will be quite sufficient to satisfy a fair and candid inquirer that the blame should be laid on the incautious employer rather than on the agent employed. There is now no excuse if we fall into the mistakes that others have made, and that have been discovered and pointed out by the investigations of patient inquirers. Who now-a-days fears to prescribe opium, arsenic, or prussic acid to remove disease? and who will deny the deadly nature of these substances when incautiously used? All are aware of the powers of these poisons, but have learned by their own experience and that of others how to prepare them, and apportion the dose so as to keep within the limits of danger and yet secure the effects they wish to produce. And so it must be with chloroform: it is far too valuable and too powerful a substance to escape the strict scrutiny of science; our acquaintance with it is short, we cannot be supposed as yet to be familiar with all its properties; and it is no reason that a hasty judgment of condemnation should be passed upon it if it has been awkwardly and rashly employed by some in the infancy of the invention. I do not dwell for one moment upon the proposition that it is the duty of the obstetric physician to conduct his patient through her labour, with as much speed and as little suffering as are compatible with the safety of herself and her offspring. To argue upon a proposition so self-evident would be but waste of time and words. That we are possessed of an agent whereby this great object can be accomplished is proved by the many thousand cases in which chloroform has been employed.

The use and value of this drug in natural and operative cases has been fully set forth in the essays already mentioned, and I do not wish to occupy time and space by quoting from my case book instances of this description; suffice it to say, that I have employed it freely and with the greatest satisfaction to myself and my patients for more than two years. My principle object in this communication is to show how chloroform can be advan-

tageously used in a class of cases that seem almost to forbid its employment, I allude to tedious labours produced by sluggishness of the uterus. If the first effect of a good dose of chloroform be the arrest of uterine action for a short time, a fact admitted by Dr. Murphy, and borne out by my own experience, it follows, as a matter of course, that if the pains be slow and weak, they will be the more surely and effectually interfered with. Now, it has happened to me to have patients under my care who were determined to inhale chloroform during labour, and who were most clamorous to get it at a time when the uterus was indisposed to act with vigour and celerity. To give the vapour, so long as matters thus stood, would have only increased the evils and protracted the labour; but by combining the use of ergot of rye with chloroform the difficulty was quite removed. In order to illustrate this practice, I will set forth some of the cases in which I have derived the greatest advantage from such a proceeding.

CASE I. Mrs. W. was attended by me for the first time in September, 1848, having had several children previously. During the last month of her pregnancy she was in a very distressing state of mental depression, and spoke continually with apprehension of danger and death in her approaching confinement. She expressed a strong desire to be treated with chloroform, and was quite pleased when I promised to let her have it. Labour came on at the proper time, commencing with slight pains, which continued, with little increase in strength or frequency, from an early hour in the morning until night. She was in a state of great alarm and solicitude throughout the day, and remained thus until 10 o'clock, P. M., when, finding no improvement in the character of the labour, I gave her a drachm of ergot of rye in two doses, at an interval of a quarter of an hour. This soon established full uterine action, and in twenty minutes she was in strong labour. She now became very much excited and unruly, dashing herself about in the bed, and could not be prevailed on to remain quiet. To obtain the promised chloroform was her only object, and she clamoured loudly for its administration. Finding that labour was now fully established, and that the os uteri was nearly dilated, I commenced the use of chloroform from a pocket-handkerchief, on which I had poured one drachm of the fluid. This was at 11 o'clock, P. M., and almost immediately a complete calm ensued; she became tranquil and composed, and never afterwards betrayed the least want of temper. Her consciousness never deserted her. She continued to speak rationally all through, and expressed the greatest delight from the inhalation.

She was safely delivered of a living boy at half-past 12

o'clock, A. M. When asked what she thought of the medicine, she said it was heaven, that she was conscious of every pain, and could make the effort to bear down, but felt no suffering. This lady had been subject to intense nervous headaches after all her previous confinements, on which occasions the pain and intolerance of light were so great, that she was obliged to have every ray of light excluded from her chamber, and the most perfect stillness observed in the house. The attacks usually lasted from twenty four to thirty-six hours, when they gradually subsided. On the present occasion, however, nothing of the kind occurred. When I called to pay my visit the following day, I found the shutters open, and the chamber full of light. My patient lay perfectly free from all pain, happy and thankful, saying she could scarcely believe it was all over, so unlike was it to what she used to endure. Her recovery was the most rapid she ever made.

This case affords some points for remark: first, we observe the beneficial effects of ergot of rye in inducing uterine action in a case rendered tedious by inertness of that organ, and thus bringing the patient into a condition favourable for the use of chloroform. It was quite manifest at the time I gave the ergot that the uterus was not disposed to efficient contraction, and that many hours would in all probability elapse before healthy action would commence. By the influence of this drug, a long night of fruitless labour was avoided, and the patient was speedily relieved from suffering. Secondly, a remarkable feature in this case is the immunity from headach subsequently to delivery. This I think must be attributed to the soothing effect of the chloroform on the nervous system, acting as a sedative, and calming the excitement which formerly used to be so distressing.

CASE II. Mrs. F., pregnant for the first time. This lady was most anxious to use chloroform in her approaching labour, and I promised it to her if nothing occurred to cause me to refrain from its employment. When the pains commenced they were very slow and weak, and continued so for twenty hours, at the expiration of which time the os uteri was dilated to the size of a half-crown piece. The pelvis was roomy and the soft parts relaxed. She was most importunate to begin the inhalation; but the pains not being sufficiently strong to warrant its administration, I gave the ergot of rye, which quickened uterine action, and in twenty minutes labour was well established. I then commenced the use of chloroform, under the influence of which she was kept for two hours, when she was delivered of a living boy.

Insensibility was never induced in this lady, she was con-

scious during the whole time, and frequently held the handkerchief herself during the pains, soliciting more chloroform, and expressing the greatest relief and comfort from its use. One ounce of chloroform was consumed in this case.

CASE III. Mrs. C. In this case of first pregnancy, labour began by rupture of the membranes without pain. In six hours there was some faint uterine action, which continued to go on slowly for a long time. At length, at the end of eighteen hours, finding the os uteri soft and nearly dilated, I administered the usual dose of ergot of rye. This soon gave strength and effect to the pains, and I then began the use of the chloroform. It was continued for one hour and twenty minutes, when delivery of a living girl was accomplished, after a labour of twenty-one hours' duration. During the whole period of the inhalation this patient was not insensible, but enjoyed the happiness of being conscious of the uterine action, without feeling its pain.

CASE IV. Mrs. P., second pregnancy. On the occurrence of labour, the pains assumed a well-marked, steady character, and continued so for four hours, at which time the os uteri was nearly dilated, and the vagina cool and moist. Having expressed a desire to use inhalation, she was now permitted to commence, and in a short time the labour pains were suspended. This was followed by sleep, which was most sound for one hour. On awaking from this condition, the pains did not recommence, so I determined upon giving her a dose of ergot of rye. She got a drachm in the usual way, which soon produced a marked change in the pains; they became more frequent and increased in strength, and when well established the chloroform was resumed.

There was now no interruption to the pains; the labour progressed most satisfactorily, and at the end of an hour from the recommencement of the inhalation the delivery was happily accomplished.

In this case the influence of the chloroform in suspending uterine action was most clearly shown; but when the contractions were induced by the artificial assistance of the ergot, it seemed to have no power to arrest them. This is a point of great value, for it shows the importance of combining these two valuable and powerful agents. In cases such as I have related, by causing the action of the ergot to precede that of the chloroform, we secure the patient against a suspension of her labour.

CASE V.—Mrs. A., sixth pregnancy. A severe attack of uterine hemorrhage had attended this lady's previous confinement, two years before her present labour. Being anxious to

prevent a similar event, I gave her a dose of ergot, when she was about seven hours in labour, and the os uteri and soft parts fully dilated. In twenty minutes the pains were very severe, and the vapour of chloroform was now inhaled with avidity. From the first time of the inhalation the pains were materially relieved, and soon disappeared, leaving the patient free from suffering, but not insensible. At times she appeared to slumber for a moment, and then woke again, saying she had a pleasant dream. She said she was conscious of the efforts which would have been pains, but she felt none; the child was born in twenty minutes from the first inhalation, and the duration of the labour was eight hours. The quantity of chloroform used was four drachms.

CASE VI.—Mrs. P., second pregnancy. In this case there was considerable delay in the first stage of labour, owing to the head of the child presenting with the face to the pubis, and their being a very weak and inefficient amount of uterine action. After three hours had elapsed, and no evidence of increasing power being manifested, I administered a drachm of ergot in the usual manner. By its aid the uterus was stimulated to contraction, and the pains were rendered strong and efficacious. As soon as the labour was well established, she was permitted to commence the inhalation of chloroform. The labour progressed rapidly from this time, the head came down well through the pubis, and in half an hour from the first use of the vapour the child was safely delivered. This lady remained conscious during the entire time of her labour. The inhalation seemed to exercise a very marked influence upon the condition of the soft parts, causing them to be relaxed rapidly and freely. The duration of labour in this case was five hours; the quantity of chloroform consumed was three drachms, and the time of its influence was only half an hour.

These cases will serve as a specimen of those that are met with in practice, where weak and slow power in the uterus produces a tedious labour, and where the patient, ill disposed to bear the pain attendant on even such a feeble uterine effort, is clamorous for the inhalation, so long looked for as the great source of relief from suffering. To proceed at once to the exhibition of chloroform in such cases, would certainly protract the labour to a great extent, and might also produce very unpleasant consequences. It has been said, that uterine hemorrhage has occurred more frequently in women who have inhaled chloroform, than is usual with those treated in the ordinary way; this does not coincide with my experience. However, it must strike any one conversant with such matters, that

the uterus most likely to be relaxed after delivery is that in which the pains have been most infrequent and feeble. If chloroform alone be given in such a case, it would be very likely to increase the tendency to relaxation after delivery; but when its use is preceded by that of the ergot of rye, such danger is completely guarded against.

It will be seen in the above cases that in none of them were uterine pains suspended after the dose of ergot had been taken; the labour went on vigorously, notwithstanding the use of the anæsthetic agent; and in all of them delivery was accomplished with the most perfect safety to mother and child. Having thus, I trust, shown how chloroform may be employed in cases that at first appear to be quite unfit for it, let me again impress upon those who may employ these medicines the absolute necessity of making sure that the agents are pure and fit for use. In papers on the use of ergot of rye, published in former volumes of this Journal, I have repeatedly expressed the opinion that when ergot fails to produce its proper effect it is owing to the use of an impure, spoiled, and therefore inert specimen. The ergot is a very delicate medicine, and is readily injured by either age or damp. It should never be kept more than twelve months, and it ought to be preserved in a perfectly dry situation. If good at first, and thus treated, it will not disappoint when brought into operation. The purity of chloroform is of the greatest importance, and this should be carefully looked to by the practitioner himself. He has now an easy method set before him, whereby he can test and purify the drug, and I feel confident that, if attention be paid to these important points, the approval of the profession will be given to the use of anæsthetics in midwifery, and a large amount of human suffering be thereby removed.—[*Dublin Journal*.

On the Treatment of Traumatic Tetanus. By H. R. DE RICCI,
Licentiate of the College of Surgeons of Ireland.

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The want of success which, up to this time, has attended the treatment of this malady, has, I conceive, mainly arisen from its very obscure pathology, but partly also from the random manner in which the many remedies for it have been selected and administered. But now that its physiology has been so beautifully illustrated by Marshall Hall, in his discoveries regarding the reflex functions of the spinal cord, we cannot but hope that a plan of treatment, will be found, which, by

being more rational in its principles, shall also be more successful in its effects.

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Two forms of traumatic tetanus have been generally admitted by all practitioners, the acute and the chronic; the former almost invariably fatal, the latter somewhat more amenable to treatment.

Acute cases of tetanus may be considered as those in which the symptoms supervene before the tenth day after the receipt of the injury, and in which the spasms succeed each other at short intervals, and with great violence; and chronic, those in which the attack comes on at a later period, and in which the spasms are less violent and recur at more distant intervals. But on this all practitioners are not agreed, for many will not acknowledge as genuine tetanus those cases which have followed very quickly the receipt of the injury. I think it well, therefore, before proceeding any further, that we should stop for a moment, and consider what it is that constitutes real tetanus, and what are the diagnostic differences between it and other convulsive diseases, which will enable the careful and accurate practitioner to say, without hesitation, this is or this is not a case of tetanus.

Many affections are laid down in books as being likely to be mistaken for tetanus, and tetanus for them: among the principal we may enumerate hydrophobia, different forms of convulsions, hysteria, and muscular rheumatism of the back of the neck; but the only one which indeed might for a time lead astray, and which does in all its minutiae imitate tetanus, is hysteria. This far-famed mimic of disease will put on *for a time* all the appearances of tetanus, and will deceive even the most accurate; but though it will assume even the most pathognomonic symptom, yet it will persist in it *only for a time*, and thus the practitioner will be enabled to correct his diagnosis. What then is the diagnostic symptom of genuine tetanus, which distinguishes it from all other diseases, and which, by being peculiar to it, must at once characterize as tetanus every spasmodic case in which it is prominent? In my opinion, *it is the unyielding rigidity of the affected muscles*; these, when once rigidified, so to say, by tetanic spasm, never relax, except in recovery or death. In hydrophobia there may be tetanic rigidity during the paroxysms, but in the intervals there are periods of rest, during which there is complete relaxation of all muscles. In hysteria the spastic condition of the muscles will sometimes be continued for a very considerable time; but it ends at last by relaxing; and the same occurs in all those different forms of convulsive affections, which seem more or less to imitate tetanus.

Thus, then, the only disease in which this permanent rigidity of muscle is met with is tetanus; and in my mind we are justified in applying this name to all those spasmodic affections in which this symptom is prominently developed.

Having thus endeavoured to establish the symptoms by which this fearful disorder is to be recognized, I will proceed to examine it in its different stages, and then explain the principles of what I consider to be its rational treatment.

Tetanus seems to be a more frequent consequence of lacerated wounds and contusions of the extremities than of other injuries; but I have seen it arise from a very slight scratch of the nose; and many cases are on record where it occurred after trifling bruises, in which the skin had not been even wounded! Still it is a remarkable feature in this disease that it is certainly more frequent after slight than after extensive injuries, and that it generally bears an inverse ratio to the extent of the effect of an accident.

One of the first symptoms of approaching tetanus is a certain unpleasant rigidity in the back of the neck, which may be easily mistaken for rheumatism, together with a general malaise accompanied with a sensation of weight and a tendency to yawn. The wound, when one exists, will also at times give indication of approaching mischief by the unhealthy aspect it assumes. These and other indications, coming on shortly after the receipt of an injury, should be carefully noted, as it is of the very greatest importance that the first symptoms should be recognized; for it is in this, its first stage, before the spinal cord has assumed that peculiar condition which induces the violent spasmodic action constituting confirmed tetanus, that our treatment will have the best chance of success.

The mode of treatment I shall venture to recommend, will meet, I know, with absolute condemnation from the majority, and with approbation from few, if from any. Yet, as it is based upon sound physiological principles, it will, in my opinion, succeed, if tried sufficiently early. I do not mean by this to say that the plan I propose will cure every case of tetanus, no more than the most approved treatment can cure every case of pneumonia; but I believe that I shall thereby blot out tetanus from the list of those diseases over which the healing art has no control.

It would seem that a wound inflicted on the sentient extremities of the afferent nerves produces, in certain conditions of the body, such an exalted state of the excito-motory system, that the power of the will is insufficient to control this condition; hence, on the application of a stimulus, for example, the vibration of the bed from a heavy footstep in the room, or a mere

attempt at deglutition, involuntary spasmodic motions will be produced, without the patient being in any way able to repress them.

It would also seem that lacerated wounds, contusions, and other injuries of the extremities of nerves, are more often the cause of tetanus, than the clean wounds inflicted by a sharp, cutting instrument; in proof of which we may refer to the rarity of the disease supervening after sabre cuts in battle, or after wounds inflicted by the surgeon in the operating theatre.

In many *post mortem* examinations of fatal cases of tetanus, a nerve has been found partially divided, or lacerated, or stretched across a broken bone; and we have cases on record (though few indeed) in which the division of the principal nerve leading to the part, or the amputation of the part itself, has at once put a stop to all spasm.

I therefore conclude from these facts, that, in the treatment of this disease, we are justified, with the view of removing the exciting cause, to amputate the entire part, or to divide the nerve leading to it. Having once removed the source of the evil, we shall find it less difficult to combat the disease; but before we proceed to consider the treatment, it will be well to investigate what are the causes of the patient's sufferings, and what the causes of his death. The chief and most severe of all his sufferings is the constant recurrence of the spasms, which, in addition to the excruciating pain at the ensiform cartilage, are so harassing in themselves, that did the patient suffer from no other ailment, he should finally sink, worn out by these repeated attacks. We must, therefore, endeavour to check this disordered condition of the nervous system, both by equalizing the powers of the brain with those of the excitomotor apparatus on the one hand, and rendering the muscles incapable of obeying the erratic dictates of the spinal cord, on the other. For in tetanus the energies of the brain are minus, whilst those of the spinal marrow are plus, and we must, therefore, exalt the powers of the encephalon, to enable it to preserve its normally relative position in reference to the excitomotor system: this we effect by the free administration of stimulants, such as wine, brandy, and especially the resin of Indian hemp, which possesses the property of exciting the brain independently of other parts of the human frame. And we obtain a passive condition of the muscles by means of tobacco enemata and fomentations, the relaxing and paralyzing effects of which are well known. The use of the latter powerful remedy might, in unskilful hands, be attended with danger, and its administration should therefore be solely intrusted to an intelligent and careful person, whose business it should be to watch narrowly its

effects; but under certain restrictions it is admirably adapted to fulfil the purpose we require of it, as under its nauseating influence all the muscles gradually relax from their rigidity, and become unable to execute any automatic movement, or in any way to respond to the convulsive impulses of the spinal cord.

Our patient is now in a state of comparative ease; his brain is exalted in its energies, and thus enabled to counteract the spasmodic motions of the spinal cord; and, when it fails, the paralysed condition of the muscles comes to its aid, by refusing to act in compliance with the impulse. The pain at the ensiform cartilage too is relieved, and the patient's forces are no longer exhausted by overpowering convulsions. When once the patient is brought to this condition, his safety is almost a matter of certainty: for what is the cause of death in tetanus? it is either exhaustion or asphyxia, and not any specific action or morbid poison inherent to the disease itself. Asphyxia is caused by the persistent rigidity of the respiratory muscles; exhaustion, by the repeated attacks of spasm. But, by the treatment I suggest, both these conditions are done away with, the sufferer is relieved from any immediate danger of impending death, and his nervous system has now time to correct the morbid functional condition into which it had lapsed, and by degrees will resume its normal functions.

In fine, my treatment resolves itself simply into copying the process of nature; and I believe that successful plans of treatment in every disease have been derived from copying this great teacher. Experience of centuries has shown us that cases of acute traumatic tetanus invariably die when left to themselves; and that cases of the chronic variety generally recover. Let us bring our patient from the former condition into that of the latter, and we shall have him in a position in which his own forces, and the resources of nature, will be sufficient to effect his recovery.

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These are, therefore, the conclusions I come to, with regard to the treatment of this disease:—to endeavour, if possible, to combat it at its outset, and commence by removing the existing cause, either by excision, amputation, or division of the nerve leading to the part; to give stimulants, such as brandy, wine and the tincture of the resin of Indian hemp, in doses of from ten to twenty drops every half hour, so as to produce a slight degree of cerebral excitement; to support the patient at the same time by giving beef-tea and eggs; to pay attention to the state of the bowels, and act upon them with croton oil or any other powerful purgative, as in this disease, as is well known, they are

extremely torpid and sluggish; and to bring him under the influence of tobacco as rapidly as possible. These are the principal means upon which we have to rely; but at the same time, I must not omit to warn every practitioner against the use of opium, belladonna, and every other narcotic, which it has been the fashion to administer indiscriminately in every case of tetanus, both in hospital and in private practice, not so much with the view of curing the patient as of stupifying his intellect, so as to render his exit from this world less painful.

And now, before I conclude, let me add a few words concerning those individuals who, in consequence of some idiosyncrasy, cannot be subjected to the continued influence of tobacco without incurring extreme danger. Those possessing such an idiosyncrasy are easily distinguished, as a few minutes after the administration of the first dose symptoms of the most alarming nature will arise: the countenance will assume a deadly hue and a ghastly appearance, and the pulse at the wrist will become quivering and intermittent. I need scarcely say that in such a case the administration of tobacco should be at once stopped, ammonia applied to the patient's nostrils, cold water sprinkled on his face, and every means used for the excitement of cardiac action.

In these cases, after having discarded the use of tobacco, the main reliance is to be placed in vapour baths, which should be applied without removing the patient from his bed, and continued for a considerable time. The patient will invariably express himself as much relieved by them, and they generally diminish the rigidity of the muscles in a very considerable degree, at the same time that they soothe the patient and allay the spasms.

By adopting such a method of treatment as I have endeavoured to describe, we shall not, as I said before, cure *every* case of acute traumatic tetanus, but we shall certainly give the unfortunate sufferers a much better chance of recovery than by abandoning them to the old routine treatment of calomel, opium, and belladonna.—[*Ibid.*]

The Spirometer.

MR. CHARLES McEuen, of Philadelphia, has recently invented a substitute for Hutchinson's Spirometer, by which may be measured the quantity of air inspired and expired by voluntary effort in a given time. Mr. McE's instrument consists of a cylinder containing water, in which is immersed another

cylinder inverted, into which the expired air finds its way through a mouth tube. This inner cylinder is counterpoised by a weight attached to a cord passing over a wheel of large diameter, and which rotates with the ascent of the cylinder, caused by the entrance of the expired air, and on which a scale indicates the amount that has been introduced. In exhibiting this apparatus to the class, Prof. Jackson, of Philadelphia, made the following interesting remarks, which we extract from the "Medical Examiner:"

Inspiration and expiration are performed by muscular power, and are both voluntary and involuntary actions. The extent to which they may be carried varies in different individuals, and in the same individual at different times. They have a limit which cannot be surpassed; the lungs can never be emptied, by the most strenuous efforts of expiration.

The air in the lungs is, therefore, divisible into two portions. The first, which is a fixed quantity, is that over which the will has no control, but remains after the strongest expiration, and is contained in healthy lungs after death. Its amount must correspond with the size of the thorax. Mr. Hutchinson calls this the residual air.

The second portion is that which is controlled by the will and muscular action. This portion Mr. Hutchinson divides into three sub-portions. 1st. Reserve air, or that portion which, after an ordinary expiration, may still be thrown out by a voluntary effort. 2d. Breathing air, or the portion inhaled and exhaled in ordinary breathing, when at rest: and 3d. Complementary air, or that portion that can be inhaled, by the strongest effort, beyond the amount of ordinary inspiration.

The three last are included in, and designated by the term, "Vital Capacity." It is, in fact, the highest effort of the muscles producing respiration. The spirometer measures the "vital capacity" of an individual, and, it appears to me, is the measure of the muscular respiratory power.

Mr. Hutchinson was struck with the fact, that the vital capacity had no relation to the size of the thorax. On the contrary, he found, by experiment, that persons of the largest thorax possessed a less vital capacity than others with chests much smaller.

In the course of his observations he remarked that there appeared to prevail a very close relation between the height of individuals and their vital capacity. This circumstance was the more strange and unaccountable, as height depends most commonly on the length of the lower extremities, and not on that of the chest or trunk alone.

From observation made on a large number of individuals, taken indiscriminately from various classes of society, amounting to 2150, he arrived at the conclusion, that the vital capacity is a constant quantity, and holds a close relation with the height. From the result of direct examination, in near 2,000 cases, Mr. Hutchinson felt authorized to announce the following rule: "For every inch of height (from 5 feet to 6 feet) eight additional cubic inches of air, at 60° are given out by a forced expiration."

He further states, "here is a guide for the operator, and a rule given that will enable us to compare men of different stature and conditions of health, one with another."

If this result should be found accurate, the spirometer would be unquestionably a most valuable addition, to aid the physician in deciding the state of health in many cases, that are, by our common mode of examination, enveloped in great uncertainty.

The following table shows the relation between height and vital capacity.

HEIGHT.				TOTAL CAPACITY.			
Ft.	In.	Ft.	In.				Cubic Inches.
5	0 to	5	1	-	-	-	174
5	1 "	5	2	-	-	-	182
5	2 "	5	3	-	-	-	190
5	3 "	5	4	-	-	-	198
5	4 "	5	5	-	-	-	206
5	5 "	5	6	-	-	-	214
5	6 "	5	7	-	-	-	222
5	7 "	5	8	-	-	-	230
5	8 "	5	9	-	-	-	238
5	9 "	5	10	-	-	-	246
5	10 "	5	11	-	-	-	254
5	11 "	6	0	-	-	-	262

Before making any further comment on the rule laid down authoritatively by Mr. Hutchinson, I will test by the instrument the vital capacity of some patients affected with pulmonary disease, who are now present.

(Several patients, cases of chronic pleurisy, phthisis pulmonalis in various stages, and emphysema, were tested, the height and age being first ascertained.)

They vary, you perceive, from 80 to 120 cubic inches expired. Not one of the above patients approaches to the normal vital capacity, in accordance with his height and age.

They are from 80 to 200 cubic inches below the standard, according to the table.

I must confess, that I have some misgivings as to the accura-

cy of this rule, and cannot but suspect that another element than that of height regulates the extent of vital capacity, and that element is the muscular force of the respiratory muscles.

I express this only as a suspicion. The extent of Mr. Hutchinson's inquiries, the evident care, labor, and conscientiousness with which he pursued his investigations, entitle them to the highest consideration, and they should not be lightly questioned.

But, in a considerable number of examinations I have made on healthy individuals, of the same height and age, with slight difference of weight, there is manifest such wide difference of vital capacity, that I cannot but hesitate in adopting the rule as universally applicable.

I have, for instance, examined, within 24 hours, three gentlemen in perfect health, one a member of our profession, who have all been and are engaged in active pursuits. They are, respectively, 5 feet 11 inches, 5 feet 11½ inches, and 6 feet in height; the vital capacity of the first two is only 170 cubic inches, and of the last 190 cubic inches. According to Mr. Hutchinson's table they ought to have a vital capacity of 250 to 260 cubic inches.

Now, these gentlemen have a peculiar, and I may say, an American conformation. I am under the impression it is not common in England. They are tall, long limbed, thin, with very slender muscles.

The highest vital capacity I have met with, as yet, is in a young gentleman 5 feet 8 inches in height, in whom it is 280 cubic inches. He is of sanguine temperament, large, bony-framed, and with well developed muscles. So far as about 100 observations have been made, I have not found that uniform relation, as stated in the rule, between height and vital capacity. The differences, from 20 to 100 cubic inches, are too great to be attributed to accidental circumstances. The individuals I speak of are all in high health.

More numerous and extended observations are, however, required, before a positive conclusion on this subject can be justified.

It has occurred to me that the discrepancies between Mr. Hutchinson's statements and my own observations, should they be confirmed by more numerous experiments, may depend on differences of race. The English are far more homogeneous than the Americans. In this country races are mingled, and continue to be more blended every day. As a race the English are bony, muscular, and sinewy. Experiments with the Dynamometer have shown they possess a superiority of muscular force.

In a homogeneous population the average height and weight

would be in accordance with an average development of the muscular system. But in a mixed population the same rule would not apply.

I believe there can hardly be a question as to the very marked difference in the general aspect and structure of the native-born Americans, who are generally a mixed race, and those of the English, Germans, Irish and French.

In examining Mr. Hutchinson's Table A, exhibiting the total capacity of 15 different classes, there are very striking differences to be seen. Pugilists, seamen, fire and police-men, and grenadier guards, have the greatest vital capacity. This is shown in the column of the table for the height of 5 ft. 8 in. to 5 ft. 9 in., and from 5 ft. 9 in. to 5 ft. 10 in.

Table of the mean Vital Capacity of fifteen different Classes.

	5 ft. 8 in. to 5 ft. 9 in.	5 ft. 9 in. to 5 ft. 10 in.
Seamen, - - -	239	258
Fire Brigade, - - -	231	237
Police Metrop., - - -	226	248
Ditto Thames, - - -	250	240
Paupers, - - -	199	262
Mixed Class, - - -	238	246
Grenadier Guards, - - -	233	240
Compositors, - - -	214	231
Pressmen, - - -	245	239
Draymen, - - -	223	245
Gentlemen, - - -	208	236
Pugilists, &c. - - -	243	273
Chatham Recruits, - - -	351	266
Woolwich Marines, - - -	240	246

In this table the vital capacity certainly does not correspond to height as it respects different classes. Those classes comprehending individuals whose occupations require athletic, robust, and picked men, exhibit a vital capacity varying from 20 to 40 cubic inches higher than paupers, compositors, and gentlemen.

This table appears to sustain the conclusion which seems to follow from the observations I have made here with the Spirometer, that it is muscular power, and not height, that governs the "vital capacity."

Successful Treatment of Acute Rheumatism by Lemon-Juice.

By DONALD DALRYMPLE, Esq., M.R.C.S., &c. Norwich.

My attention having been drawn to the treatment of rheumatism by lemon-juice, by my friend, Dr. G. O. Rees, and more recently again by his pamphlet on the subject, I offer a

case selected from many, as illustrative of its power and utility.

Mrs. T——, a small, delicate woman, aged twenty-six, had suffered for several winters past, from severe attacks of acute rheumatism, which lasted during various periods, from five weeks to two months.

She had been treated in various ways for these attacks, and they had yielded to either colchicum, or opium, iodide of potassium, or mercury, or to all combined, but always left the patient very weak, and very susceptible of cold. I was called to her on the 28th of March, 1850, after she had been ill for ten or twelve days. She was suckling an infant of six months old, and had been in feeble health ever since her accouchement, which took place at the seventh month of gestation.

I found her with a pulse at 130; skin hot, and profusely perspiring; tongue white and coated; thirst excessive; urine scanty, clear, and not high coloured; bowels confined; pain intense; every joint set fast; and the patient unable to move an inch without assistance.

From previous experience of the effects of lemon-juice, and from fear of reducing her powers, I determined to use it in this case, and gave her half an ounce every four hours, combined with twenty minims of tincture of hyoscyamus, in a drachm of water. I ordered her lemonade for drink, and arrowroot for diet.

March 29th. She has passed a restless, painful night; no sleep; symptoms unabated; bowels not relieved. Ordered a colocynth pill and a draught of rhubarb, with tartrate of potash, next morning. To continue the lemon-juice.

30th. Bowels freely relieved; the rheumatism unabated; pulse still 130; complains of pain over the region of the heart; the stethoscope gave no indication of pericarditis, but, to relieve the pain, I applied a blister to that region.

31st. Still in great pain; pulse 125; tongue cleaner, and less thirsty; no sleep; bowels purged four or five times. Ordered ten grains of compound powder of ipecacuanha at bedtime. The infant was also troubled with griping and diarrhœa, which was easily relieved by an aromatic astringent.

April 1st. The pains somewhat abated; pulse 120; tongue cleaner; but the joints of the patient still completely set fast. Increase the lemon-juice to six drachms every four hours.

2nd. Decidedly better; pulse 108; tongue cleaner; thirst gone; pain less, and able to turn in bed without aid. The improvement continued through the succeeding days, and on the 5th, the dose of lemon-juice was reduced to four ounces every five hours.

From the 7th instant the patient continued to improve rapidly; her pains left her; her appetite returned; her pulse fell to ninety; and on the 10th she left her bed.

On the 15th she got down stairs, and I ceased my attendance, but I visited her on the 25th of May, and found that she was, in spite of very cold, wet and unpropitious weather, free from all pain, and in her usual health.

Though nothing could be more satisfactory than the progress of this case, I am unable to point out the *modus operandi* of the medicine. As soon as the system came fully under its influence, the pulse fell, the fever abated, the pains subsided, and the appetite returned, yet nothing further noticeable took place. The secretions were but little affected, except that the urine, from being very scanty, became copious, but there was no deposit of any kind in it. Here is a patient who usually has not got quit of her attacks under six or eight weeks, and is then left weak and emaciated, but who, under lemon-juice, is, at the end of nineteen days, down stairs, and very little the worse for her attack.

Should this medicine stand the test of time and hospital practice, and we be able to avoid either half poisoning our patients with colchicum, stupefying them with opium, or enervating them with mercury, a great boon will be conferred on the suffering multitudes of this variable climate.—[*London Lancet*.

Dislocation of the Lower-Jaw.

An important addition has been made to practical surgery in the introduction, by M. Nélaton, of a new, simple and efficacious process for the reduction of dislocation of the lower jaw.

* * * * *

According, then, to M. Nélaton, the obstacle to reduction existing neither in the resistance offered to the condyle by the eminence of the transverse apophysis, nor in muscular action, it must be sought for, not in the articulation itself, but in neighboring parts.

"In front of the tempero-maxillary articulation," says M. Nélaton, "we find the tempero-zygomatic fossa, in which the coronoid process is lodged when the mouth is closed. Before and behind this excavation are placed two eminences, the posterior formed by the transverse root of the zygoma, the anterior by the articulation of the superior maxillary with the malar bone. At the inferior part of the suture which results from the union of these two bones, there exists a tubercle sufficiently prominent, limited within by a notch formed by the smooth edge of the malar process of the superior maxillary bone, and

often on the outer side by a little, elongated, almost oval fossette. This eminence, to which we may give the name of malar tubercle, is situated at about the distance of a centimetre from the coronoid process. In place of this tubercle we have sometimes met a plane surface, and even in certain subjects a notch more or less deep; but the presence of the tubercle is the rule. With regard to the coronoid process this latter presents great differences; very short in some, and elevated scarcely to the level of the condyle, it is found very much elongated in others; sometimes directed upwards, at other times obliquely outwards, so that its summit tends to meet the zygomatic arch; in some instances directed forwards, and distant from the condyle; in others directed backwards, so as to approach it." "These facts well established," continues M. Nélaton, "let us examine the pathological condition. Having, as I have said, undertaken some experiments on the dead body, with a view to verify the prevailing doctrine on luxations of the lower jaw, I have ascertained:—First, as M. Malgaigne observes, that if the condyle of the lower jaw is carried forwards without passing the point which the cavity of the capsule permits it to reach, the displacement disappears forcibly as soon as we approximate the dental arches, the eminence of the transverse apophysis presenting no obstacle to the return of the condyle. Second, that if the anterior part of the capsule be cut or torn, so that the condyle can pass out of it and advance a few millimetres, we remark that the displacement is permanent, not, as is generally believed, because of the elevation of the transverse root, nor by reason of the contraction or tension of the muscles, but because the summit of the coronoid process comes to butt (*arc-bouter*) against the inferior and anterior angle of the malar bone, and is lodged in the little fossette which we have said exists often at this point. The contact of the summit of the coronoid process with the malar bone appears to us, then, to constitute an indispensable condition in the true dislocation; and for this the displacement need not be extreme; it suffices that the condyle be advanced from three to four millimetres. The external lateral ligament remains intact, the capsule alone is torn at its anterior part, and the inter-articular cartilage either accompanies the condyle in its displacement, or remains beneath the transverse root, according as the rupture is either above or below its anterior edge. . . . It results from what precedes that it is not on the condyle that we must fix attention to find the cause which renders the dislocation permanent, but on the coronoid process and the malar bone, since it is in the contact of these two bones that almost all the difficulty of reduction resides."

* * * * *

To succeed, according to the process of M. Nélaton, it is necessary to act either by the interior of the mouth, taking a point of support behind the mastoid processes, or externally, by the operator taking a position behind the patient, and making pressure on the coronoid process, pushing it downwards and backwards, to disengage it from contact with the malar bone, at the same time that the patient opens the mouth. In ordinary cases, a light pressure is sufficient; but if more force be requisite, M. Nélaton advises the head to be supported by an assistant, or a band to be passed around it, in which the operator can engage his index and middle fingers, while the thumb must be brought to bear on the coronoid process.

The method of reduction which we have now described has been before the profession for more than a year. We have seen the success which has attended its use in the author's hands; and when we reflect on the great difficulties frequently presented to the surgeon in attempting to restore the lower jaw to the glenoid cavity, we cannot but feel that M. Nélaton will have conferred a signal benefit on humanity should the rigid test of experience prove that his anticipations of the facility of reduction by the method he has devised are as well founded as we are confident his views of the pathology of this luxation are correct. We shall anxiously await the verdict of the clinical surgeons of Dublin on the subject.

Dislocation of the Cervical Vertebra.

Vrignonneau has reported a case of dislocation of the fifth cervical vertebra, in which his efforts at reduction were crowned with success. It occurred in a man thirty-three years of age, who fell from a cherry-tree on his head, becoming immediately senseless, and remaining so for an hour and a half, after which he complained of intense pain in the vertex and neck. The head was bent forwards, and the entire body motionless. After a time, paralysis of the extremities, the sphincters, and the bladder came on. On the second day, death by suffocation appeared imminent, yet reduction was attempted in the following manner:—The patient's shoulders were fixed by assistants, and traction made on the head by the operator. In proportion as the parts were extended, the voice became stronger, and the breathing freer; as soon as extension appeared to have been carried to a sufficient degree, the head and the upper cervical vertebra were directed backwards, when the under surface of the fifth slipped on the upper surface of the sixth cervical vertebra with a perceptible noise. The unpleasant symptoms immediately disappeared, the man was soon able to resume his occupations, and experienced only some stiffness and impediment in the lateral movements of his neck.

Caoutchouc Urinal.

The construction of a species of urinal in caoutchouc, places in our hands a very simple means for avoiding the unpleasant and often fatal consequences which result from incontinence of urine in certain cases of paralysis, and other affections complicated with profound lesion of the voluntary nervous power. This little instrument is furnished with an aperture of sufficient diameter to embrace and grasp the penis by a little elastic band such as that by which a glove encircles the wrist, besides which there are two small loops by which it may be attached to a suspensory bandage, while the lower part is supplied with a stop-cock. The use of this little apparatus will be found to add much comfort to the patient's existence, and save him from the unpleasantness of bed-sores in many instances.

[*Dublin Journal.*

Anæsthetics used Externally.

We notice in the *Bulletin Général de Thérap.*, a short review of the application of anæsthetic agents in diseases of the skin accompanied with itching. They are particularly applicable, according to M. Cazenave, where there is itching without an eruption. He asserts, however, that he is not deterred from applying them, even when there is an eruption. When an eruption is present, chloroform produces smarting, but is soon followed by marked relief. Hydrochloric ether is preferable to chloroform, as it does not produce so much smarting when applied. The preparation that M. Cazenave employs consists of 2 grammes of hydrochloric ether and 30 of cerate.

Aconite in Lichen and Prurigo.—M. Cazenave uses the following prescription, with great success, in lichen and prurigo:

R. Ext. Aconite,

Ext. Taraxacum, . . . aa 1 gramme.

Make forty pills—one or two to be taken morning and evening. The itching soon ceases, and the eruption rapidly disappears.

A New Adhesive Substance.—Dr. Mellez proposes the following as a substitute for collodion. Dissolve shell-lac in alcohol with a moderate heat, in such proportions as to give it the consistence of jelly. This is simply spread upon suitable cloth. It is not so transparent as collodion, nor does it dry so rapidly.

Miscellany.

Review of Chemistry for Students, adapted to the Courses as taught in the principal Medical Schools of the United States. By JOHN G. MURPHY, M. D. Philadelphia: Lindsay & Blakiston. 1851. pp. 328.

We are indebted to the enterprising publishers, (through the house of J. A. Carrie & Co., of this city) for the above work. As its title indicates, it is not a *treatise* on chemistry, but rather a manual intended to aid the medical student in the prosecution of his chemical studies during the Lecture term. As such it is certainly calculated to be highly useful. It is divided into three parts, the first treating of Physics, the second of inorganic Chemistry, and the third of organic Chemistry. We cheerfully recommend it to the attention of students of medicine.

Diseases of the Uterus.—It appears that the excitement attending the debates of the Parisian Academy of Medicine a few months since, in relation to the frequency of diseases of the uterus, has crossed the channel, and, with accumulated intensity, invaded the halls of the Royal Medico-Chirurgical Society of London, and the pages of the periodical press. The alleged exaggerations of Dr. Bennet, are severely animadverted upon, and the frequent and indiscriminate use of the speculum uteri particularly reprobated as both unnecessary and demoralizing. The monomania which leads some practitioners to see nothing but affections of the uterus in all cases of female disease, originated, we believe, with Lisfranc, who applied the speculum to almost every woman who came under his charge, and who amputated the os tincæ for the most trivial ulcerations or abrasions. His feats filled the Journals, and aspirants to fame or to practice, in the French metropolis, were not slow, speculum in hand, to emulate the distinguished surgeon of La Pitié. The London physicians caught the infection and it was soon transmitted to our own shores. It is even now, in some of our great cities, the most successful species of professional Charlatanism, and ladies of every class flock to these “womb Doctors” from all parts of the Union. We have heard it affirmed that such is the celebrity of one of these gentlemen, that his ante-chambers are daily crowded and that numerous equipagès may be continually seen in front of his residence. It is not surprising that others should desire to share the golden harvest by also making the uterus their “*special study*.”

We would not be understood as undervaluing the labors of those who have so largely contributed to advance our knowledge of uterine affections; but merely as reprobating quackery in the matter. We know too well the immense advantages derived from the use of the speculum vaginae to think for a moment of lending our aid to any effort to discountenance it. In reproducing the letter of Dr. Hall, we do so because it has attracted much attention both in Europe and in our country, and not because we believe it applicable to the respectable professional corps of the United States. Whatever may be the state of things in England, we feel assured that, except in the hands of charlatans, neither has the speculum been abused nor the purity of the sex been tarnished by its use in this country.

On a New and Lamentable form of Hysteria. By MARSHALL HALL, M. D., F. R. S., &c.—I have no doubt that I was one of a considerable number who, at the last meeting of the Royal Medical and Chirurgical Society, (a meeting which will long be memorable in its annals,) wished to express their sentiments on the subject of the use of the speculum vaginae, without having what they deemed the perfect opportunity. I regret that the discussion was not adjourned to another evening.

I think the profession deeply indebted to Dr. Robert Lee for bringing this question forward for discussion. It is not one of mere medical or surgical treatment, but of medical and public ethics; and I confess myself astonished at the light manner in which a vaginal examination was spoken of by one of the gentlemen present at the Society. I think the challenge of Dr. Bennet should have been accepted at once, and that a committee should have been, and should now be appointed, to test the existence or the non-existence of the thousand and one "ulcers" or "abrasions" of which so much has been said of late.

The gentleman to whom I have alluded above, huffed the idea of indecency in making a vaginal examination. There need be no exposure of the person of the patient; surgeons make no scruple about an examination of the rectum, (as if the two examinations could, morally speaking, be compared.) But, if there be no exposure of the person, and if the examination of the rectum be frequently made, is there, at first, no wounding of the feelings, and is there, afterwards, no deterioration and blunting of those feelings, by the repeated daily or weekly use of the speculum vaginae in the virgin, and in the very young even amongst the married? I loudly proclaim that there is such deterioration, and that the female who has been subjected to such treatment is not the same person in delicacy and purity that she was before.

I have known cases of the most revolting attachment, on the part of such patients, to the practice and to the practitioner. I have known them to speak of "the womb" and of "the uterine organs"

with a familiarity which was formerly unknown, and which, I trust will ere long be obsolete. The current of the ideas becomes hypochondriacally directed to these organs. The very mind is poisoned. A new and lamentable form of *hysteria*, I had almost said, of *furor uterinus*, is induced, with this aggravation, that the subject of distress is either concealed by the greatest effort, or explained at the expense of virgin or female modesty.

There is a case of "poisoned mind" in the male sex, induced by the quack doings of the day, relative to the existence of impotency, which all of us must have treated and deplored. A similar case of "mental poisoning" is now being induced in the other sex by the frequent, constant, and undue reference, on the part of the profession (?) to the condition of "the uterine organs."

These latter patients become reserved, and moody, and perverse, and speak unintelligibly in broken sentences; the peace and happiness of the family circle is broken up; subjects are discussed at the domestic hearth which ought never to be mentioned except in the sick room; words which wound are spoken, and thoughts which are derogatory are expressed, by other, perhaps by the male, members of the family.

One poor miserable patient comes to me weekly, thus afflicted. She had been treated by the speculum and the caustic for months, as an out-patient at University College Hospital. I sent her to Dr. Robert Lee twice. Twice that gentleman examined and declared that there was no uterine or vaginal disease. Meanwhile, the miserable patient's mind is absorbed by this ideal malady, and the peace of her husband's home is destroyed.

I sent another patient to Dr. Robert Lee a few days ago, (whom I had never seen,) under similar circumstances, but moving in a different rank of life. The same opinion was given, the miserable patient suffering dire disappointment!

I recently attended a poor curate's wife, who had come to London for medical aid, at, as I suppose, great inconvenience. During my short attendance, this patient was constantly urged by a friend, a titled lady, (the aristocracy always take the lead in quackery,) to send for her physician, who is a strong abettor of the speculum. The course which followed may be imagined, and need not be described. A case of more complicated misery for a husband cannot well be conceived—a sickly wife, afflicted with uterine hypochondriasis, set upon by a titled advocate of the uterine quackery, with straitened resources.

The advocates of the speculum speak of cases which had resisted the efficacy of the usual general and local treatment, and which yielded to the use of the speculum and the caustic. I have seen cases in which, the speculum and caustic having been employed—and unduly employed, as I believe—the patient remained more miserably afflicted in mind and body than ever, and this the effect of that treatment. Whether the former supposition be as well founded as the latter, I will not presume to determine; but I believe the cases in which the young, and especially the unmarried, are afflicted so as

really to justify the use of the speculum, to be rare; and the cases in which the injection of a solution of the nitrate of silver by her own hand may not take the place of the application of this valuable remedy in substance by the hand of the practitioner, to be rare indeed.

I will not advert even to the epithets which have been applied to the frequent use of the speculum by our French neighbours, who are so skilled in these matters; but I will ask, what father amongst us, after the details which I have given, would allow his virgin daughter to be subjected to this "pollution"? Let us then maintain the spotless dignity of our profession, with its well-deserved character for purity of morals, and throw aside this injurious practice with indignant scorn, remembering that it is not mere exposure of the person, but the dulling of the edge of the virgin modesty, and the degradation of the pure minds of the daughters of England, which are to be avoided.—[*London Lancet*.]

Cause of Goitre.—The French Government having assigned to Dr. Grange the duty of making researches into the history of Goitre, with the view especially of determining its cause, Dr. G. has recently made an able report, which is contained in the medical periodicals of that country. The length of the Report will not allow us to reproduce it in extenso; but we will translate a few passages from which may be obtained the most interesting facts.

"In drawing up geographical charts of the distribution of Goitre in France, Switzerland, Savoie and Piedmont, and in looking into the localization of the disease in England, Germany, America and India, I have ascertained most positively that the affection is entirely independent of latitude, altitude, or climate; and that it is unconnected with circumstances of habitation, poverty, &c. Its occurrence seems connected with the presence of magnesia in the aliments and water used, and its absence appears often to depend upon the iodine contained in these ingesta. I am induced from all my observations to conclude that magnesia predisposes to Goitre, and that iodine prevents it. * * * By comparing the charts it is evident that Goitre is endemic wherever magnesian formations prevail. The most distinguished geologists, MM. Elie de Beaumont, of France, Studer, of Switzerland, de Sismonda, of Piedmont, have signally recognized the correctness of these observations. We have invariably found salts of magnesia in the waters and in the ashes of grain used in the affected districts; and whenever Goitre was endemic in an isolated locality of non-magnesian formation, the waters used were found decidedly impregnated with this earth. * * * In most of the Goitre districts, many of the young men, in order to evade enlistment, acquire the disease by

drinking freely from certain springs well known to produce it: all these are strongly magnesian. In those sections of country, some of the rich exempt themselves from the disease by using exclusively rain water kept in cisterns. Mountain water resulting from the melting of snow and ice, alone, never occasions goitre." * * *

Dr. Grange observes that there are in France 450,000 persons affected with Goitre, and from 35,000 to 40,000 with Cretinism. He also relates the interesting fact that the inhabitants of the town of Montmeillan and of the city of Geneva have become exempted from the disease, formerly very common, by using spring and river water instead of that from wells.

The learned reporter suggests as a curative, as well as a preventive means, the habitual use of a minute quantity of Iodide of Potassium mixed with common salt. He would add to every pound of salt used for culinary purposes from one to five grs. of the iodide.

"In Savoie, for example, where the sale of salt is monopolized by Government, they might add to it one ten thousandth part of the Iodide of Potassium, and thus, even without their knowledge, rid of Goitre a population containing 100,000 cases of this disease, for about 1600 francs, the cost of 40 kilogrammes of the Iodide, which should be mixed with about 400,000 kilog. of salt, the quantity probably consumed by 100,000 persons per annum. * * * "I have for 18 months used this plan of treatment in whole families, always with entire success, and without any inconvenience whatever."

It is worthy of remark that our author treats Goitre with much smaller quantities of the Iodide than are usually resorted to. Its beneficial effects in his hands may be attributed to the fact that, thus administered, the remedy produces its constitutional effects without impairing the digestive functions, and may consequently be tolerated a long time. It will be recollected by all who are familiar with Lugol's method of administering Iodine in scrofulous affections, that he lays great stress upon the necessity of giving it *largely diluted* in order to avoid any unpleasant effect upon the gastric surface. It is in this manner that he not only succeeded in obtaining most astonishing results, but also avoided the peculiarly distressing effects so much apprehended by those who used it differently.

What is true in relation to Iodine is equally so with regard to many other active or irritating agents used with a view to their constitutional effect. Arsenical preparations, so valuable as general restoratives of depraved habits, when given in minute doses and largely diluted,

cease to be so when administered in larger quantities and in a more concentrated vehicle. The effect of 12 drops of Fowler's Solution given in a spoonful of water is vastly different from that of a similar dose diluted with a tumblerful of some bland fluid. In the former case the effect is principally local—in the latter, general. Corrosive Sublimate, to be advantageously used, should always be largely diluted. Calomel, when given in large doses, displays its action entirely upon the alimentary surface, provoking a copious exudation of mucus in which it becomes wrapped up and is carried down to be expelled with the fecal matters; whereas if administered in small doses, it remains in the stomach sufficiently long to undergo the changes necessary to its introduction into the circulation and to the development of its constitutional effects.

We do not mean to advocate Hahnemanism by the above remarks, but merely to signalize an important principle in therapeutics.

New Method of Treating Hydrocele.—The "Gazette Médicale de Paris" contains a lengthy communication made by M. Baudens to the Academy of Sciences, upon a new method of treating Hydrocele. This consists in transfixing the sac with a long and slender trocar, the canula of which has a lateral aperture about its centre. The stylet being withdrawn leaves the aperture open in the sac, and therefore permits the escape of the hydropic fluid at both ends of the canula. The sac being thus emptied, is then inflated with air by closing one end of the canula and blowing into the other through an elastic catheter inserted into it for this purpose. The object of this inflation being to add to the irritation occasioned by the presence of the canula, which is allowed to remain like a seton for one or more days, a blunt stylet is passed into the canula in order to close the central aperture and thus to retain the air until some pain be induced.

M. B. dwells with much propriety upon the fact that the irritability of the tunica vaginalis varies so much in different individuals, that it is difficult, and even impossible, *a priori*, to determine upon the activity of the agent that may be necessary to induce the proper amount of irritation. He therefore allows the canula to remain introduced until the proper degree of excitement be induced. In some cases, the mere introduction of air, made once or oftener during the day, will suffice. If this fail, he throws in tepid water one or more times; and finally, if this be insufficient, he resorts to the injection of a weak solution of Nitrate of Silver (about 1 gr. to 4 oz. of water.) Whenever the exudation of plastic lymph and the supervention of some

tumefaction are secured, the canula is removed and the case treated in the usual manner.

M. Baudens claims for his method the advantages of less danger of wounding the testicle and of throwing the injected materials into the cellular tissue, as well as of greater certainty in the result. He has used this method for twenty years—in which time he has operated on 200 cases, and found the average duration of treatment to be 19 days for a complete and radical cure. Many were cured in a fortnight and some in ten days : 39 were cured by atmospheric inflation alone, the canula remaining from one to two days ; 47 required both air and water ; in the remaining 114 cases a sufficient degree of irritation was not secured until after the use of air, water and the solution of Nitrate of Silver. In only 20 cases did the inflammatory symptoms exceed the required limits, and this was readily subdued by the use of cold lotions. Relapse occurred in but two instances.

From the above abstract, it will be perceived that the plan adopted by M. Baudens is well worthy of attention. We regret that the great length of his communication forbids its entire transfer to our pages.

We have long been satisfied that quackery in its multiform manifestations can only be effectually combatted by society at large, and that all efforts of our professional brethren to discredit it, do more harm than good. We are sure to be regarded as parties interested, and therefore to give the quack all the advantages of persecution. When, however, members of the regular faculty are detected in base and dishonorable practices, it becomes the duty of those apprized of the facts to expose them to public indignation, as using their professional knowledge for the perpetration of criminal acts. We hope, nay we believe that the writer of the subjoined letter has exaggerated the extent of the evil he signalizes as common in New England. We feel assured that no physician in our section of the country has ever been guilty of such mischievous conduct. We take the following from the Boston Medical and Surgical Journal :—

Quackery in Abortion.—The medical profession takes rank with the other learned professions ; and is justly regarded by all enlightened nations as one of the most useful, liberal and noble of the sciences. Our profession, for centuries, has been advancing. It has, indeed, accomplished that which its most ardent admirers could not reasonably have expected, and now it is no way inferior to law or theology. This was not so once ; for in by-gone days, *physic* ranked lowest in the scale of the learned professions. Rome had her orators, poets and generals ; England her statesmen, bishops and barristers. They had

their physicians also ; but how comparatively small is the space these disciples of Hippocrates and Sydenham occupy on the page of their country's history, compared with the volumes which contain the writings, discoveries and transactions of the distinguished men of other professions. But when the history of our times shall be recorded, the names of those who distinguish themselves in medicine and surgery will shine as brightly as those of the jurist, the divine, the military chieftain, and others, who have also been useful to their race, and shed lustre and renown upon the nations of their birth place or adoption.

For the last half century the progress of medicine has been, in all civilized countries, remarkable ; but no where more marked than in the United States. The indomitable and enterprising energies of our countrymen have been manifest in this department. The condition of our colleges has improved ; our medical societies have taken, and continue to take higher grounds. The American Medical Association is annually adding rich and ripe sheaves to the great store-house of science ; *these* are constantly taking the place of tares, which have hitherto occupied too much space. This Association has not only for its object the *expurging* of worthless material and the supplying of sound doctrines, but it also aims at the establishment of good regulations and *ethics*, with a view that justice, honorable conduct, and moral integrity, shall govern and preserve the medical men of this country, (thus indirectly but really benefiting the sick and all others throughout the land,) and if possible, eradicate every vestige of quackery with which our country has been scourged.

While the Association, through its committees, has made excellent suggestions, pointed out valuable improvements, and discountenanced quackery in most of its forms and devices, it has not yet struck any decided blow on that most diabolical kind of quackery, that high-handed villany, which characterizes the *abortionist*. That this kind of Charlatanism is rife, and is practised by regular members of the profession, that is, men who have *diplomas*, there can be no doubt ; and I believe that some who are promoted to *office in our medical societies* are of this order of quacks. That such men *are quacks*, no one will question—the *epithet* belongs to the *unprincipled* as well as to the *ignorant*.

It may be thought that the nature of this subject is such as to render it best to be silent. But I take no such view of the matter ; and if I possess no ability in the way of putting it down, I wish to warn the young practitioner, who is about to make his *debut* in his profession, as he values his future usefulness, as he values principle, as he values reputation and a good name, to abstain from the infernal performance under *every* circumstance, let the inducement be what it may. No honorable man of experience will for a moment *think* of such an immoral act ; the unprincipled man will do it—will do *anything*, however mean or vile—for *money*. The young man, while he is waiting for more laudable employment, may be *tempted*. Such are often applied to, to procure abortions, especially if thought to be in need of money.

The applicants should be spurned, and their offers treated with disdain—let their money perish with them. I insist upon it, that this is a dangerous situation for many young men, and if they fall here, just as they are to be introduced into legitimate practice, they fall forever; their sins will surely find them out.

These abortionists seem not aware that the testimony of their dying victims is generally elicited by the attending physician and friends; or that the throes of parturition, the fear of death, or some other circumstance, will draw out all the facts in the case, and that they themselves will henceforth be considered quacks and murderers by many whose respect and esteem they would gladly enjoy.

Need I allude to the moral and physical evils this practice produces? Are they not manifest, *fearfully* manifest, in this community, even within the puritan borders of New England? It increases prostitution and infanticides, and breaks down the constitutions of those who are naturally healthy. Look at the bills of mortality as returned from our large cities; see what numbers die of peritoneal inflammation; mark the increase of *stillborn* children and *premature births*.—(Vide *New York Medical Gazette*, Vol. I., No. 1, p. 6.)

Besides these bills of mortality, the records of criminal courts will furnish sufficient proof that this crime is every day becoming more prevalent. It is humiliating to admit that there are a class of physicians who, Herod-like, have waged a war of destruction upon the innocent. Though their motives are not the same as those which instigated that cruel king, they are no less murderers for that. If there is any difference, they are worse than Herod. He was influenced by popular clamor and bigotry; these quacks do all for money, and such could be hired to burn out the eyes of infant princes.

These men are better known than they would like to be. It is said that a woman cannot keep a secret. Whether this is so or not, the man who procures abortions is generally well known. He needs no hand-bills, placards, or other advertisement; he is soon notorious. Inglorious fame! Who would have such a disgraceful notoriety? Who would thus disgrace his profession; who would sell his claim to honor and principle; who would shed innocent blood for a few pieces of silver? After a man has thus degraded himself, after he has sunk so low, can he expect to retrieve his character? Who ever knew such a man to reform? If he is susceptible to feelings of remorse, like Judas he will go out and hang himself to hide his own shame.

I consider this species of quackery the most abominable and wicked of all. Anything is Charlatanism which is morally dishonest, though it may be practised under cover of a diploma; and therefore that man is a Charlatan, to all intents and purposes, who, like the notorious Restell, becomes the executioner of babes in utero. Such a man is the *vilest* of quacks, and the *meanest* of men.

I shall not stop to give the history of those lamentable cases which have come under my observation, and terminated fatally as the consequence of procured abortion—those fatal cases of puerperal peritoni-

tis, caused by the bloody hands of *doctors* and *M. D.'s*; but if the confessions of the dying are to be relied upon, I know men who have carried on this shameful and iniquitous business, and have not only been the murderers of infants, but the instruments also of consigning their guilty mothers to premature graves, "unhouselled, unannointed, unannealed."

I have heard some of the older members of the profession say that abortions are of more frequent occurrence now than formerly; and they have rightly suspected the increase is owing to criminal hands. I need not remark on the evil consequences of this mischief upon health—the health of American women. I need not attempt to portray its blighting and destroying effects upon the strength of the fair daughters of New England, for their withering results are well understood by the majority of your readers. Various instruments are employed for destroying the integrity of the ovum, and I have been informed that these quacks conceal their weapons from their patrons as if they were something strange or curious. I was told, not long since, by a woman who was operated upon recently in a neighboring city, that the wretch who performed the operation obliged her to take solemn oath not to expose him. She kept her word, for she would not give me his name, but left me to *guess* who he was! Being a true Yankee myself, I suppose I can guess with ordinary exactness. This woman said that at the same time there were several other women apparently waiting for the "slaughter" in an ante-room of the building.

Irregular practitioners, and the women themselves, are addicted to this kind of criminality; but, as a general thing, they have learned their art of some unprincipled doctor, who either purposely or accidentally let slip the secret to the vulgar. The implements which I have heard of as being used by these irregular quacks, are sharpened sticks, goose-quills, wires, &c.; *not* those beautifully-polished, tonsil-lancet instruments, which some of the *regular quacks* wield with so much dexterity and freedom, as "if the assassination could trammel up the consequence, and catch, with his surcease, success." I once found a wire (then bent at nearly a right angle) in the vagina of a young girl who had been in the hands of a regular abortionist. At each extremity of the wire was a leaden ball, about the size of a marble, one end of which had probably been introduced into the *os uteri*, and there left to remain till contractions of that organ should be established. For the information of the villain who was guilty of this double massacre, (should his eye fall upon this page.) I will state that the operation succeeded—succeeded in destroying a *fœtus* of five months, and impairing the health of the girl so that she continued to suffer from uterine disorder, and finally died in about three years afterwards.

Now in view of honoring and improving the condition of our praiseworthy and liberal calling, as well as that of society at large, I ask the co-operation of every respectable physician to aid in putting down everything and every body that shall appear to be cognizant to the offence—the crime of procuring abortions—the *massacre of infants*. I do not think that we should in *any case* expose our patients, those

who place their lives and reputations in our hands. This would be a breach of confidence—a violation of good faith; a principle which physicians have held inviolable both in ancient and modern times. The *perpetrator*, and not the *subject* of the crime, should be made responsible. I leave it for others to prescribe the *method* and manner of checking and rebuking these quacks in their criminal progress. Public opinion, the indignation of the populace, will not be sufficient to meet the exigency which the importance of this matter demands. Public justice is slow, and the people who employ these quacks will not be shocked by any outrage, or be disgusted by any measures, however revolting they may be to ordinary minds, and moral men; for they are in truth nothing better than *accessories*, and without their aid and support this class of practitioners could not live. I would suggest, however, as a starting point towards reform, that medical societies and associations expel these “assassins,” and that each physician take the responsibility of informing against them whenever opportunity may offer. For one, I am willing to join such a crusade, however unpleasant the war may be, and do all that I can in the way *sequari vestigia rerum*. The medical profession is bound to take action in this matter; if it is not done, if proper measures are not resorted to, injustice and disgrace will be charged upon us for affording protection and fellowship to these Charlatans. This evil is not confined to any particular region or section of country; it has at length become general, and is a *national curse*.

Every State should render the offence of inducing premature labor or abortion a *penal* one (unless it shall be done for the safety of the mother, where there is a deformed or contracted pelvis, or where some other cause renders the operation absolutely necessary); it should be a State-prison offence, at least.

The evil is one of such magnitude that I have felt it my duty to make this communication. If by it any one shall be persuaded from falling into criminal quackery, certainly good will come out of it. Or if those who make laws and regulations for medical men shall be induced to render the crime punishable, and this action be taken any sooner because the medical public have thus had their attention directed to the subject, I shall have no cause to regret that I have incurred the displeasure of those practitioners who have been styled *abortionists*, or that I have made the admission, through the medium of your Journal, that there is *criminal quackery in the medical ranks*.

Yours respectfully,

Greenville, R. I., Dec. 27th, 1850.

J. P. LEONARD.

Prof. Beatty on Chloroform and Ergot.—Whilst the Editor of a periodical of this kind is not to be held responsible for the views of contributors to the original department, the case may sometimes be different in relation to his *selections*. We, therefore, occasionally take the liberty of commenting upon articles placed under the Eclectic head. The paper of Prof. Beatty, which we have transferred entire to our pages, is

one of decided merit and peculiarly opportune, where the use of chloroform is becoming so much generalized. It is especially valuable in giving details which should be borne in mind by all who use this potent agent, for "*when properly conducted*," its employment is rarely injurious. The author dwells upon the importance of using a *pure article*, of administering it in a *horizontal position* and with an *empty stomach*, and of not inducing *total insensibility* or *unconsciousness*. There can be no doubt that to these precautions observed by obstetricians, must be attributed the disadvantageous contrast between their results and those of surgeons, especially of dentists.

But, Prof. Beatty proposes to combine the action of Ergot with that of Chloroform, in order to avert the influence which he freely acknowledges the latter agent to exert in lessening uterine contractions. This combination appears to us most felicitous, yet full of mischief in injudicious hands. Looking to the great danger to which the child is exposed by the untimely use of an agent which induces contractions characterized by little or no intermission, we have always regarded it a safe rule not to administer Ergot until the os uteri is fully dilated. Yet Prof. B. does not insist upon the necessity of this condition. In his 1st case, the patient having been in labour from an early hour in the morning until 10 o'clock, P. M., with but slight pains, a drachm of Ergot was given in two doses, at intervals of a quarter of an hour, and it was only after "finding that labour was fully established, and that the os uteri was *nearly dilated*," that chloroform was administered. In this case, then, the Ergot was given *before* the os uteri was "*nearly dilated*." In the 2d case, the os uteri was dilated to the size of a half-crown piece before the use of Ergot. In the 3d case, the Ergot was given when "*the os uteri was soft and nearly dilated*," and in the 4th case, when "*the os uteri was nearly dilated and the vagina cool and moist*." In the 5th case, "*the os uteri and soft parts were fully dilated*," and in the 6th its condition is not stated.

We do not know the exact value the author gives to the terms "*nearly dilated*," and may therefore misinterpret him. If he means to say that the os uteri was nearly *fully* dilated, we have no objection to urge; but if otherwise, we must beg leave, with due deference to such high authority, to submit whether it would not be more prudent to withhold the Ergot until the *complete* dilatation of the os uteri. We firmly believe that the injudicious use of Ergot causes the death of more children than any other circumstance connected with parturition. Indeed, the number of still-births has so much increased in France, since the administration of this article has passed into the

hands of midwives, that the Government has recently submitted to the Academy of Medicine the propriety of forbidding its use altogether or of limiting it to special cases. The Academy recommended that it should be prescribed only by competent physicians.

American Medical Association.—The Committee of Arrangements request all Societies and other institutions authorized to send delegates, to forward a correct list of those selected to attend the next annual meeting, to the Secretary, Dr. H. W. DeSaussure, at Charleston, S. C., on or before the 1st day of April.

In consequence of the resignation of Dr. Stillé, one of the Secretaries, from ill health, all communications intended for the next meeting of the Association, must be addressed to the remaining Secretary, Dr. H. W. DeSaussure, Charleston, S. C.

The Fourth Annual Meeting of the American Medical Association, will be held at Charleston, S. C., on the 2d Tuesday of May next.
[*Charleston Med. Journ.*]

Colored Students in the Medical College.—We understand that the Medical Faculty of Harvard University have signified their intention to exclude *colored men* from their classes hereafter. Although it is highly desirable that colored men should be properly qualified to act as physicians in the flourishing colony of blacks at Liberia, it is doubtless considered by the faculty inexpedient to admit them into our public schools of medicine.—[*Boston Med. Journ.*]

The St. Louis Probe.—The present number closes the first volume, and ends the publication, of the PROBE. During a year's experience in journalism, we have been convinced that neither fame nor funds, can be acquired by conducting a medical monthly, and that many members of the medical profession are miserably poor in pocket, and more are deficient in moral principle, however well they may be imbued with the principles of their profession. We are inclined to believe that a large number, who have received our journal without paying for it, have devoted themselves to the study of scorbutus, with some success; for we must say they have treated us most scurvily, and not a few have shown a thorough acquaintance—not with abstract principles—but with the principles of abstraction, which would entitle them to the consideration of the judiciary. For the kind favors, and warm support we have received, however, from the better portion of our brethren, we return our hearty thanks, and thus take leave of them. Our hearts are so very full, and our pockets so very empty, that we are unable to say more.—[*St. Louis Probe.*]

Small-Pox in New York.—Under the above caption, the Editor of the New York Medical Gazette animadverts with great propriety upon the neglect of the authorities of that city, to take proper steps for preventing the introduction and propagation of a disease so emi-

nently contagious as the small-pox. After stating that there are 40 cases of this malady at the quarantine Hospital, and that it is being daily brought into the city by emigrants, the Editor adds: "The report of the City Inspector for the last week records 8 interments after death by small-pox in this city. As most of the cases appear in its modified form, and the mortality of this malady should not ordinarily exceed 4 per cent., the inference is authorized that there have been at least 200 cases in the city during the week, which is very probably less than the truth." We are inclined to believe this estimate of mortality much too small. The following circumstances are cited in illustration of the indifference of the authorities on the subject:

"Within a very short time, an estimable lady of this city, who adorned the relations of daughter, wife, and mother, was cut down in the vigour of her youth by small-pox, having contracted it by riding home from a steamboat, in a public hack, in which a patient had just previously been sent to the small-pox hospital! Very recently, on entering a Broadway omnibus, filled with passengers, one of the number was recognized as covered with variolous eruptions, and precisely at that stage which is most likely to spread the infection; and yet, immediately beside him, sat a young mother with her infant in her arms, and on the inquiry which humanity prompted, it was ascertained that neither mother nor child had ever been vaccinated! Persons have been seen, not only in all our public conveyances, but at our most fashionable places of amusement, and even in our churches, who, though convalescent from small-pox, were nevertheless walking sources of contagion, the characteristic crusts and scales of recent eruption still disfiguring their persons, and radiating their poisonous effluvia wherever they went."

With such a state of things in our great commercial emporium, and the daily increasing facilities of communication with it, it is not surprising that small-pox should be found now continually recurring in almost every town in the United States and even in many villages. The sanitary condition of New York is of vital interest to the whole country, and no means should be spared by the authorities of that city to prevent the extension of contagious diseases.

Another Medical College.—We have just received the 1st announcement of the Medical department of the University of Nashville, Tennessee. The 1st course of Lectures will commence on the first Monday in November next. The Faculty consists of John M. Watson, M. D., Prof. of Obstetrics and the Diseases of Women and Infants; A. H. Buchanan, M. D., Prof. of Surgery; W. K. Bowling, M. D., Prof. of the Inst. and Pract. of Medicine; C. K. Winston, M. D., Prof. of Mat. Med. and Clin. Med.; R. M. Porter, M. D., Prof. of

Anatomy and Physiol., and J. B. Lindsley, M. D., Prof. of Chem. and Pharm.

New Medical Periodicals.—With the opening of the year we received the Western Medico-Chirurgical Journal, edited by Drs. Sandford and Armor, and published monthly at Keokuk, Iowa; and the Philadelphia Lancet, edited by T. D. English, M. D., issued semi-monthly and containing 8 pages, the two last of which are devoted to advertisements. We have also received the two first Nos. of the Stethoscope, a very neat monthly of 64 pages, edited by P. C. Gooch, A. M., M. D., of Richmond, Virginia. From present indications, it promises to be a valuable addition to Southern Medical Literature, and will doubtless be well supported by the Profession. We cordially wish it a successful career.

Yale College.—The Degree of M. D. was conferred upon eleven candidates at the late annual examination (15th January).

180 persons died of small-pox in Boston, during the last year.

Castleton Medical College.—The number of matriculants at the spring session was 81—at the fall session 72—graduates 64 for the year.

The N. Y. Medical Gazette announces the death of John A. Clements, of Georgia, who was attending the medical lectures in that city.

We learn, with regret, that Prof. J. B. Beck, of the College of Phys. and Surg., and Prof. A. L. Cox, of the New York Medical College, have both been compelled, by ill health, to discontinue their Professional duties for the present session. We indulge the hope, however, that they may be able to resume their labors next fall.

Dr. J. M. Smith, of Saco, Maine, has been convicted of murder in the 2d degree, for the killing of Miss Caswell, in his attempt to produce abortion, and has been sentenced to the Penitentiary for life. Well done!

At a meeting of the Georgia Medical Society, held on Thursday last, the following preamble and resolutions were adopted:

WHEREAS, since our last meeting Dr. JOHNSON B. TUFTS, long a faithful and efficient officer of this Society, has been removed from amongst us—

Resolved, That we deplore his loss, prematurely cut off in the very prime of manhood, as that of one whose professional attainments and high sense of medical honour secured for him our sincere respect, while his upright character, as a man, endeared him to many of us as a cherished personal friend.

Resolved, That a copy of the above be furnished the nearest relative of the deceased, and that it be published in one or more of the public gazettes, and in the Southern Medical and Surgical Journal of Augusta.

J. B. READ,

Secretary Georgia Medical Society.

SOUTHERN MEDICAL AND SURGICAL JOURNAL.

Vol. 7.]

NEW SERIES.—APRIL, 1851.

[No. 4.]

PART FIRST.

Original Communications.

ARTICLE XI.

On the Vis Medicatrix Naturæ. By W. J. SUMMER, M. D.,
of Lexington, South Carolina.

Believing that it is both the right and the duty of the student of any science, to investigate, candidly, fully and fearlessly, any theory held forth by men or books, no matter how long or how generally received, and feeling that he upon whose shoulders is laid the responsibility of life or death, should, above all others, be willing to "prove all things," and hold fast only to "that which is good;" I have ventured to devote this article to an inquiry respecting that principle so frequently invoked by medical writers, and which is perhaps best known as the "*vis medicatrix naturæ*," or vital principle.

The existence of this principle, the medical man is too prone to admit upon mere authority. Few can say that they have investigated the truth of this subject, yet almost all are ready to defend the principle in question; and this fact is not without significance.

I shall endeavor to show that there need not be, and is not any such principle in living beings, as that styled *vis medicatrix naturæ*. If I fail in this, I hope at least to prove, that there are two sides to this, no less than to some other questions which not long since, it would have been considered heresy to call in doubt. I am aware that the attempt to disprove the existence of this almost axiomatic principle of ours, is novel and seemingly adventurous. I can only ask, that this subject be viewed in the light shed upon it by simple scientific truth.

Let us not forget, in the outset, that while there are many truths relative to the human body, which are beyond all doubt being attested by the evidence of our own senses, the idea of a *vital principle* is purely a theory, a supposition, which no man can say he knows to be true, but which we infer by the exercise of our reasoning powers. Thus, we have all seen many solid substances belonging to the human system, and, among them, such an one as phosphate of lime in the bones: and, consequently, we know that all these do exist. But, if an individual tells me he knows that there is such a thing as a *vis medicatrix naturæ*, I may ask him how he knows?—has he ever seen it? No! Heard, smelled, tasted, or felt it? No! Then he does not know; he merely infers, theorizing from certain phenomena which he has witnessed in the system; and what he would call a fact, is only an inference—a theory. But being a theory, it may be erroneous; and hence, until established, must be received with due caution.

If now we look through the medical works of the past and present time, we are struck with the diversity of names and offices with which this supposititious principle has been honored. Thus, Hippocrates, who was the father of this medical monstrosity, and above whose level of vision, we, it seems, have not been able to raise ourselves after a lapse of two thousand years, christened his bantling *Physic*, i. e. *Nature*; and, subsequent to it, he had other principles, *Dynamics*, or *Powers*. Aristotle called it the *moving* or *generating* principle. Van Helmont named it *Archeus*. It was Stahl's *anima*; Bœrhaave's *impetum faciens*; Hunter's and Hooper's *vital principle*; Darwin's *sensorial energy*; Rush's *occult cause*, (occult to him, no doubt, not because different from ordinary physical causes, but because he could not in this instance comprehend their operation); Cullen's *autocrateia*, or *vis med. nat.*; Whytt's *sentient principle*; Broussais' *vital chemistry* (and here we seem to have an approximation towards the truth in the case); Culpepper's *vital spirits*, &c. By others, it has been designated the *living principle*, *living powers*, *powers of life*, *vital force*, *vital essence*, *vis insita*, *vis vitæ*, *vis conservatrix*, and so on to the end, if indeed there be any, of the chapter.

Notwithstanding the marked discrepancies thus existing be-

tween the advocates of this principle, we find them generally agreeing upon the following points: First, that the *vital principle* superintends and controls the involuntary functions of the system; secondly, that it resists the ordinary chemical affinities between the elements of tissues during life; thirdly, that it *tends to resist* the encroachments of disease, and to repair injuries received by the organism.

The question now arises, do the phenomena exhibited by living bodies, require any such principle as the *vis medicatrix naturæ* for their explanation? If not, is not such a principle an useless burden upon our science? And can a principle in medicine be useless, without being at the same time a positive evil? Does not the doctrine alluded to blind the physician as to the true action of remedial agents, by teaching him that he *cannot cure disease*, but can only throw in a *may-be assistant of nature*; while he ought to know that chemical affinities reign in living as well as in dead matter; and that he can, if he will, learn what those elements are of which the diseased tissue possesses too much or too little, and remove or furnish them accordingly? While physicians thus err with regard to the very object and action of remedies, no matter how honest their convictions or how zealous their efforts, thousands of victims must pay the forfeit of their ignorance. Nor are these views unsupported by authority—it is gratifying to find a confirmation of them in Carpenter's Elements of Physiology.

I now ask attention to the four following propositions:

First. *The properties of chemical compounds vary according to their composition.* The air we breathe,—that happyfying compound, the nitrous oxide, or laughing gas,—and the corrosive and deadly nitric acid, are each composed of precisely the same elements, nitrogen and oxygen, merely united in different proportions. Now, this is worth thinking of. Give a man one mixture of these elements and he laughs or rants like a madman, and then recovers and forgets it. Give him another mixture of the very same elements, and lesions, agony, and death follow! What is it, that gives to these almost similar compounds such opposite effects and qualities? I answer,

Secondly. *The new set of actions exhibited by new combinations of material elements, is due to the evolution, in the latter,*

of properties before latent, and the rendering latent of others before sensible; in other words, the conditions in which an element is placed, determine the properties it will exhibit. To illustrate: Carbon is a solid, oxygen a gas: unite them in certain proportions, and we have another very different gas—carbonic acid; combine this with still another gas, ammonia, and we have a white solid, carbonate of ammonia. Here we see a change of properties with every change of condition. Again, oxygen supports combustion and respiration: unite it with sulphur and it will now support neither, but will powerfully corrode; combine this new product with magnesia, and it will cease to corrode, and may be swallowed in large doses with at least temporary good effects. Most wonderful transformations, we are ready to exclaim; and yet there has taken place no real transformation through all these changes. The oxygen, which we should expect by this time to have been forever lost in the compound, may be obtained again, precisely what it was before its union with the sulphur, possessed of all and only the properties of pure oxygen, and ready to combine again, or a thousand times, or an infinite number of times, with sulphur or any other substance, and come out unmingled, unimpaired, genuine oxygen in the end. The same is true of all other chemical elements. Now if this is so, (and we know it is,) then it follows, that even in the sulphuric acid, or in the sulphate of magnesia, the oxygen still remains unchanged and unchangeable, and that it exhibits new properties, not because it becomes any thing more or less than oxygen, but because it is oxygen existing under a new condition. It follows universally then, that when any chemical element passes from the state of a simple to that of a compound, or from one compound into another, it has properties rendered latent which before were sensible, and others made sensible which before were latent. Thus, it is a property of oxygen, while a simple, to produce flame when in contact with combustibles; but combine it with hydrogen, and although it remains, as I flatter myself I have plainly shown, genuine and unchanged oxygen; yet, no sooner does it enter upon this new condition of a hydrogen-compound, than the former property of supporting combustion becomes latent, and another, the very reverse of

this, becomes sensible. If it be objected that the result is due to the hydrogen, which does not, even when uncombined, support combustion, we will take another property of water against which such an objection cannot be urged. Water manifests the property of combining with the metallic salts, almost without exception, as part of the process of chrysalization; while neither of its component elements can be made to perform an office at all analogous to this.

Thirdly. *The human body is entirely and solely composed of chemical elements.* It is built up of solid and fluid atoms like the tree, the mountain, or the great globe itself. We are masses of matter, and, so far as science is concerned, we are nothing else! It may be matter of surprise that I insist so urgently upon this point. It may be said that this is a truth which no one doubts or questions. But it is one thing to admit the truth of an assertion—quite another to appreciate the bearing of what we admit. I declare the body to be a great chemical compound. “Granted,” says one, “nobody thinks of questioning it: at least, nobody who has not the hardihood to belie the evidence of his own senses.” But, when I take another step upon the ground thus granted me, and declare that the actions of the body depend on the properties of its chemical constituents; and that, consequently, a fever succeeds a chill in the human mass for the same reason that a new colour comes out when new compounds are formed in a chemical experiment—i. e., because the simple laws of nature compel such results to follow such causes under such circumstances,—the theorist straightway objects: “Fever, I admit,” says such a one, “is a certain condition of a certain chemical mass, the body; but it cannot be produced by simple chemical laws—it is the work of the *vis medicatrix naturæ*.” Now here is an inconsistency stubborn enough to prove the death of any system. *It is granting a material mass, yet denying that it is governed by the laws of matter.* Men say their bodies are material, are but “clay,” are “dust;” yet seem not to know what this means when they have said it. What, then, does it mean? This: that a flesh-clad skeleton is a thing like an alkali, or any metallic base, save in one sole circumstance, and that is its being a higher species of compound—the highest manifestation of affinities; no longer

dead-chemical but vito-chemical. With acids and other substances we can dissolve it, decompose it, form new compounds out of it, just as we should do in experimenting upon soda, or carbonate of lime, or fibrin, or blood; varying the mode of experimentation, of course, according to the different chemical condition of the elementary constituents in each particular case. The action of acids, alkalies, oxygen, chlorine, etc., upon the tissues, living or dead, is fixed, definite, chemical; and in many cases well understood, and will yet be understood in all. What then are the essential elements of the human fabric? They are these thirteen: carbon, hydrogen, oxygen, nitrogen, phosphorus, sulphur, iron, chlorine, sodium, calcium, potassium, magnesium, and fluorine. Copper, and a few others, are occasionally found. "But," says an objector, "these substances in the body are combined according to vital laws; they are vital compounds." This brings me to my last proposition:

Fourthly. *The component elements of animal bodies are held together by no other force than that of ordinary chemical affinity.* In fact, when we have admitted the body to be a mass of chemical elements, we have conceded every thing claimed in this proposition. What! is not each department of nature under the control of its own unalterable laws? And are the stubborn affinities implanted by the Creator in any variety of matter, to yield to the domination of an unknown, intangible, invisible, indescribable *something*, which is yet confessedly a *nothing*;—an immateriality, a nonentity, which, save a name, and a shrine in the brain of the theorist, claims no element of existence?

"But," say the advocates of the potent vis, "how happens it, that the elements of the body are united in such peculiar proportions, forming such peculiar compounds?" Let us see. Does it require any more "*vital force*" or "*principle*," to hold together carbon and oxygen in the fibrin of animal flesh and blood, in the ratio of 40 equivalents to 12, than it does to unite the same elements in morphine in the ratio of 35 to 6, or to unite chlorine and oxygen in hyperchloric acid in the ratio of 7 to 1; or, in fact, more than it does to unite oxygen and hydrogen in water in the simple ratio of 1 to 1? Certainly not. There are, indeed, in fibrin and morphine, other elements be-

sides those just named, but the principle would be the same were there a score of them. "Animal fibrin," it may be said, "has six different elements and casein five." I answer, so has a certain platinum-salt six elements, and common crystalized alum five; yet I suppose no one will claim any great share of "*vital principle*" for these latter.

Again, if we allow that hyperchloric acid exhibits one set of properties, among which is that of sourness, and morphine another set, among which is that of stupefying; shall we not admit that fibrin may exhibit still another set, among which is contractility?—and this, too, without the necessity of calling to our aid any other than ordinary physical principles. Instead of being content to explain the vital phenomena thus simply, however, the advocates of the *vis med. nat.* invoke the aid of a principle *absolutely inexplicable* in itself, or in its connexion with the organism which they say it directs. Once more, take one equivalent of carbon and two of oxygen—unite them chemically, and each is neutralized, saturated, satisfied; and will remain satisfied and combined, until dispossessed of its fellow by some superior affinity. Now, take the proximate elements of gelatin or fibrin, of blood or nerve, and are they not in every instance, neutralized, saturated, and satisfied, by their mutual attractions, as certainly and as perfectly as in the case of the most orthodox chemical compound? And will they not remain combined until forcibly separated by some superior affinity? "Why, then," it may be asked, "does the body suffer so rapid a decay after death?" I answer again, on purely chemical principles. It is a physiological fact, that among the particles of all the tissues, some are constantly becoming dead, and in this state are selected and carried off by some of the excretory organs. Now, when death of the body occurs, such of these particles as have but just become effete, ceasing at once to be eliminated from among the normal matter of the tissues, remain in contact with them, act upon them as ferments or putrefactives, and thus hasten the process of decomposition. This principle is strikingly exemplified in cases of complete suppression of urine. Of the *urea* contained in this fluid, Dr. Watson says, "it is a mere excrement, which, in health, is removed from the blood by the kidneys. When it is not so

carried off, it accumulates in the blood and *acts as a poison*, especially upon the brain.”—(Watson’s Pract. of Phys., p. 867.) The effects of this *poisoning* he tells us are “coma and death.” That the effete matter, in this case, retained within the system, has acted as a putrefactive, is proved by the fact that the patient thus afflicted often becomes gangrenous before death.

Now, if such be the effect of effete matter retained in the system from the cessation of a single secretion, while all the others are active, it is easy to see what consequences must ensue when all the secretions are suddenly and simultaneously checked. The rapidity of animal decomposition after death, has long been considered a certain proof of the existence of a *vital principle*, which is required during life to resist the natural tendency of the organism to decay. Since, however, we have seen that this rapid *post-mortem* decay is really due to a mere *cessation of excretion*, we find here no necessity for supposing the existence of any specific counteracting principle, because nothing needs be counteracted. In life, *decay* goes on, and with it, *parri passu*, comes *reparation*; in death the former reigns alone, and its products not being cast forth, gain strength in the work of disintegration by their own no longer compensated reactions.

From the four preceding propositions, these conclusions necessarily follow: that all the phenomena of the human organism, in health or in disease, are chemical phenomena; that all the animal functions are but modes of chemical action; and, finally, that life is but the sum total of all the properties of matter existing in that form which we call organized.

This doctrine may be startling, but it has a broad and deep foundation. It is not built upon the superficial and treacherous sands of common notions and unlearned prejudice; nor does its corner stone rest upon the delusive basis of some gaudy phantom-cloud of theory; it stands upon simple scientific truth. It discards theory, and appeals to what we know!

The more we examine this position, the more shall we find its truthfulness commending it to us. For example: organization may exist without life; therefore, the latter is not essential to the former. Therefore, again, organization is the pre-existent, and life the subsequent, in the chain of causation. Take

another example: according to the views of the advocates of the vital principle, "*Life is a forced state*, or an assemblage of functions which resist decay." Then life is a *negative state*; a state of opposition to, or absence of, decay. But death all admit to be a negative state. Then here we have two states, opposed as widely as human comprehension can conceive, and yet *both negative*; and, most unluckily of all, an individual may have both lived and died, and yet have enjoyed no positive state, first or last! But this is absurd. Life is positive; we know and feel this, and so do all that live; and death is negative enough, as we also know.

Did time and space permit, I might go on to show the origin of a notion of a vital principle; refer to, and combat, other arguments in its favour, and to the influence which such views exert upon the conduct of the physician. However, I think I have gone far enough to direct attention to this subject—farther disquisition is useless. In conclusion: these "recuperative actions," "reparative processes," "efforts of nature," "sympathies," etc., of which we hear so much in the medical world, should be explained on purely chemical principles. I believe the day is near at hand when they will be so explained. Such things do exist, but we have erred in assigning the proper causes of them. We shall one day give them a more consistent rationale, and more appropriate names.

I have endeavoured to show that no such principle as the *vis medicatrix naturæ* is requisite to an explanation of the phenomena of life; if not requisite it is false, for truth is always requisite to an understanding of the operations of nature; if false, it is detrimental; for, as I said before, it blinds the physician to the true action of therapeutical agents, and destroys his patient. Those, whose high prerogative it is, to have entrusted to their care the health and lives of their fellow-beings, cannot, it would seem, look with indifference upon results like these.

ARTICLE XII.

Treatment of Seminal Weakness, by Veratrine and Strychnine, with Cases. By HENRY F. CAMPBELL, M. D., Demonstrator of Anatomy in the Medical College of Georgia.

To the practitioner, the restricted treatment of Spermatorrhœa is the cause of much embarrassment and dissatisfaction. When a case of this very delicate disorder is presented, the mind at once recurs to the treatment by cauterly to the seminal ducts, so successfully practiced by Lallemand, as the great remedy, and many practitioners, being of timid character or unacquainted with the mode of application, temporize with or refuse to prescribe for the case. The treatment by cauterization is, perhaps, of all modes, the most valuable now employed for the generality of cases, and we find it commended by every writer on the subject, since the time of its introduction. Its efficacy, however, depends upon the particular pathological condition of that portion of the urethra immediately surrounding the mouths of the seminal ducts, which is generally the result of inflammation or irritation commencing in parts adjacent to or sympathetically connected with this locality. Thus gonorrhœal or blenorrhagic inflammation of the urethral mucous membrane, by extension of the phlogosis, will involve this portion; congestion or irritation, as of piles or ascarides in the rectum, or enlarged or otherwise diseased prostate, will often be found to determine the condition necessary for the occurrence of the seminal waste. In a similar manner, also, do we constantly observe an indulgence in the pernicious habit of masturbation give rise to the same irritation by the frequent and prolonged excitement to which the entire organ is subjected.

In all of the above cases, cauterization, either immediately or after preliminary treatment, is attended with the most beneficial results, and without it, all other remedies are either entirely nugatory, or afford but a temporary palliation of the disease.

Now, it will be observed that in the above condition of the parts, the seminal loss depends most frequently, so far as the ejaculatory orifices are concerned, upon a hyperæmic state of the surrounding mucous membrane, which, by the alterative effect of the cauterly, is either mitigated or effaced, and thus a

healthy condition of the parts established. But there is another pathology which occasionally obtains, differing entirely from any of the foregoing, indeed exactly averse to them in every respect; for here the difficulty depends upon an *atonic* and enfeebled innervation of these organs—a relaxed state of all the parts connected with the secretion and elimination of the seminal fluid, resulting undoubtedly, from a partial paralysis of the nerves distributed upon these secernent surfaces. From this condition there often arises impotency of the most incorrigible character—it may be brought about in various ways: a prolonged state of subacute inflammation, venereal excesses, masturbation, or the drainage of those parts consequent upon seminal losses produced by other causes, and thus a case which in the beginning depended upon *exalted innervation*, in process of time, will owe its continuance to an entirely opposite condition—*nervous atony*, as the following case will illustrate:

CASE I. T. J., a young man, aged about 27 years; had been the subject of seminal losses from an early age. His nervous system had become very much impaired—indeed he was affected with chorea. The losses continued daily, and on account of general prostration he was confined constantly to his bed. He had been treated for the chorea, but no attention, so far as we could learn, was given to the seminal waste. When application was made to us for treatment, the frequency of his pollutions was really fearful, they occurred generally at night, and without erection. He would find the semen on his linen in the morning, or on waking during the night, and was unable to say when the discharge took place. Being aware of the length of time the disease had existed, and also of the very low condition of his general nervous system, we were induced to view his case as one depending on nervous atony rather than inflammation or excitement: hence our treatment.

℞. Of Strychnine, gr. 1.

Gum acaciæ and water, q. s.

To make twelve pills. Dose, one pill in the morning and one at bed-time.

Also—℞. Of Veratria, grs. 30.

Lard, ʒi.

Make an ointment, with which rub well over lumbar and sacral regions, twice daily.

It became necessary to reduce the strength of the ointment, after a few days, on account of the pain experienced during its application. At the expiration of eight or ten days, the case was so much improved, as regarded the seminal losses, that the treatment was omitted. Pollutions, however, recurred, and recourse to the same treatment again put an end to the discharges. When we last heard of the case, the emissions occurred but once or twice a month, and were attended by erections and waking from sleep. He was furnished with a supply of the pills to take on any return of the disease. Although his strength increased, and he much improved in every respect, he will probably never be free from a tendency to this disorder.

In addition to the causes above enumerated, there are others which act more suddenly and directly in bringing about the same atonic condition of these organs, of which case 2nd is an example.

CASE II. A. W., a gentleman of excitable temperament, aged about 42 years, had been subject to seminal losses of rather more frequent occurrence than natural.

On recovery from an attack of the Dengue, which prevailed in the city during last summer, and at which time he suffered severely from pain in the lumbar region, he found that the intervals between the pollutions were very short, and that they were invariably unattended by erections; indeed, he said that he had not experienced a full erection since his recovery, then about six weeks. He had become nervous and chilly, his hands were cold and clammy, and although a man of great vigor and robustness of constitution ordinarily, he was now enervated and dejected. He had been treated previously to his application to us, and had taken a preparation of the muriated tincture of iron and cantharides in conjunction with the cold bath to the loins, and regular daily exercise. When we saw him he was suffering from irritable bladder produced by the cantharides; he passed water frequently, and could retain but little on the bladder. The cold bath, he said, "chilled him through, and the exercise (sawing wood) fatigued him beyond all endurance,"

though his principal cause of uneasiness was his inability to produce an erection—his entire *impotency*, which he considered, as yet, premature in a man of robust constitution.

Treatment.—Prescribed one-twelfth of a grain of strychnine, in pill, three times a-day, and the application of veratrine ointment, 30 grs. to the ounce of lard, morning and at bed-time, to the loins and sacrum. After a continuance of this treatment, for about a month, he entirely recovered, both from the impotency and the seminal losses. Here the cause of the atony is fully apparent; the spinal irritation in the lumbar region, during the attack of dengue, had doubtless destroyed the tone of those nerves connected with that portion of the cord,—nerves which assist in forming the hypogastric plexus, and supply the organs of generation; hence their partial paralysis.

In the foregoing, we would not be understood as recommending the treatment of spermatorrhœa by veratrine and strychnine, as a substitute for any of the modes of treatment now in use, but have suggested it as applicable only in such cases as above described, wherein the disorder depends entirely upon impaired or deficient innervation.

ARTICLE XIII.

A Case of Urinary Calculus, attended with peculiar circumstances and treated by Lithotrixy. By L. A. DUGAS, M. D., Prof. of Surgery in the Medical College of Georgia.

The following case is reported because of certain peculiar features presented during its progress. The patient, Mr. John L. B., of Hall county, Ga., is 30 years of age, was kindly directed to my care by Dr. Richard Banks, the distinguished surgeon of Gainesville, and arrived here on the 5th of February last. Having suffered from early childhood with phymosis and an almost complete closure of the orifice of the prepuce, (which he believes was congenital), the difficulty of voiding his urine caused this to distend the prepuce into a considerable bag, to accumulate enormously in the bladder, to stagnate in the pelvis of the kidneys, and to induce very great impairment of the general health. The preputial orifice was so small as not to

admit, without much difficulty, the introduction of a knitting needle; the urine was therefore never passed off in a jet, but the patient was subjected to all the inconvenience of a continual stillicidium; he had frequent and violent attacks of nephritic pains, attended with protracted chills, fevers, and the usual concomitants of retention of urine. Yet it was not until the 20th year of his age that he sought professional aid and was circumcised by Dr. Banks. From that time his health improved rapidly; but he continued subject to occasional paroxysms of severe nephritic pains, which now became confined to the left side. These pains would extend down along the course of the ureter and continue one or more days, leaving him in a debilitated state, from which he would, however, soon recover. He is not aware of ever having passed gravel or any thing like calculous matter, although his urine would sometimes present a very copious sediment.

This state of things continued until the middle of April last, when, although in good health and not having had any nephritic pain for about three months, he felt a calculus drop into his bladder. Attending to his usual avocations, he stepped out to urinate, did so without any difficulty whatever, and when in the act of buttoning up his garment, distinctly felt something fall into the bladder. He immediately mentioned the fact to a friend, and added that "it must be a stone, for its fall produced a sensation like that of a buck-shot allowed to drop into a bag." A few hours afterwards, on again attempting to urinate, the stream was suddenly arrested by the engagement of the calculus in the urethra—the sensation being so distinct that he instinctively carried his hand to the perineum in order to force it out—but in vain;—and the same difficulty has ever since attended his micturition. These details are given as establishing conclusively the facts that he did know the *precise moment* at which the stone came into the bladder, and that this occurred so late as about *three months after the last nephritic attack*. He has experienced no pain whatever about the kidney since that. In May he was sounded by Dr. Banks, who readily detected the stone.

On the arrival of Mr. B. here, I examined him, detected the calculus, found it to be small and determined to crush it as soon

as circumstances would permit. The patient was directed to use dilating bougies, to remain quiet, to drink freely of slippery elm tea and super carbonate of soda, and to take a hip bath every night. In a week he was found to be sufficiently prepared, and (on the 12th of February) the operation was performed with Heurteloup's "*brise pierre*," as modified by Charriere. The bladder being filled with tepid water, the calculus was readily seized and crushed three times, without pain. A few fragments were passed off with the water and others during the night with the urine. On the following day, finding the patient very comfortable, without any symptoms of irritation, and very anxious to get home as soon as possible, I again introduced the instrument and crushed the remaining fragments, sufficiently to allow them all to be passed out during the night. He now expressed himself "entirely relieved, and feeling like a new man." The baths, &c., were continued, and on the 16th February, I explored the bladder carefully, without being able to detect any vestige of the stone. The patient was therefore discharged.

The dimensions of the stone were accurately ascertained by the crushing instrument to be about one inch in length and half an inch in thickness. Professor Means having kindly subjected some of the fragments to analysis, informs me that they consisted of Oxalate of Lime. The stone was exceedingly hard, and tested to the uttermost the fine temper imparted to the metal by Charriere's unrivalled skill.

PART II.

Eclectic Department.

On the Physiology and Pathology of the Ganglionic Nervous System. By JAMES GEORGE DAVEY, Licentiate of the Royal College of Physicians, London, &c., &c., &c.

"There are yet great truths to tell, if we had either the courage to announce, or the temper to receive them."—DISRAELI.

CHAPTER I.

On the Nisus Formativus, or the Solar Ganglion. The ganglia of the sympathetic; the nervous structures first formed in the fœtus. Monstrosities. Strictures on the opinions of Drs. M. Hall and Roget. Mr. Lawrence's case of acephalous monstrosity. Dr. M. Hall's division of the Nervous system; his experiment on the frog. The author's experience. Le Gallois. Hunter. Sir B. Brodie's experiments open to objection. Animal organization, its arrangement and adaptation; mind and instinct.

The object of the following papers is to prove that *Life* is the function of the *Solar Plexus*, regarding it as the *root* of the ganglionic or sympathetic system. That the *Solar plexus* is the *impetrum ficiens* of Hypocrates, or the *materia vitæ* of Hunter; that it is the *organ* whose *function* may be represented as the principle or stimulus which enables every other and subordinate part in the animal economy to continue its specific and allotted labour towards the existence of the individual; that both the brain and spinal cord, in common with all the viscera, hold a similar relation to, and dependence on, the *solar ganglion*, as the centre of the ganglionic system, that the iris does to the retina, or the external senses do to particular parts of the cerebral mass; and these opinions the author has privately circulated for the last *ten** years—that is, since 1835—as is well known to many medical friends and acquaintances. I am aware that Dr. Stevens has also advocated some such views as these; but it cannot be doubted that the *priority* is not with him.

INTRODUCTORY REMARKS.

On the Nisus Formativus, or the Solar Ganglion.

The physiologist, if I mistake not, will consider that my position, as explained in the advertisement to the reader, must derive no inconsiderable confirmation, from the circumstance that the solar ganglion is that particular portion of our organ-

* This and the succeeding chapters were written in 1845.

ism which is first formed in utero, and therefore may be really considered as the *germ* of all the phenomena to be afterwards developed.

Nothing can appear more reasonable than that that portion of our organism, from which every other takes its vitality, should enjoy a prior existence. The egg precedes the chick, and the specific vitality of the former is impressed on the latter. The foundation is erected before the house, and the design of the architect, it may be added, is not unfrequently to be anticipated by an early examination of it. Muller says, in his *Physiology*, translated by Dr. Baly, that "Ackermann asserts that the sympathetic nerve is the part first formed in the *fœtus*." Rolando, moreover, declares what has been usually considered as the first traces of the vertebræ at the sides of the spinal cord, in birds, to be the ganglia of the sympathetic nerve. The assertions of Ackermann and Rolando acquire great weight from the testimony of such men as Blumenbach and Gall, both of whom add the authority of their illustrious names, and confirm the former-named physiologists in their opinions. Blumenbach says, "the nervous system," meaning the organic nervous system, "of the chest and abdomen, are fully formed, while the brain appears still a pulpy mass," and refers to Gall's writings for the same views. He adds, "these ganglia and nerves would hardly be formed before the brain and spinal marrow, but for the sake of the organs which they supply, and the functions of which (with the exception of the genitals,) are as perfect at birth as at adult age, while the mind and brain are slowly perfected."*

Ackermann, Rolando, Blumenbach and Gall, all maintain, then, as I have shown, that the ganglionic or organic nerves of the abdomen and thorax are the first formed in the embryo. Now it is extremely unlikely that the development and formation of the solar ganglion—the centre and source of the organic nerves and their anastomosing branches—should be preceded by "*organic nerves*;" their dependence on it, it may be said, is analogous to the dependence of the branches of a tree on its root; and therefore I would claim for the solar ganglion a like precedence. Viewing the matter in this light, it is readily seen that the solar ganglion itself is, in the embryo, nothing more nor less than the *nisus formativus* of Blumenbach, and that, like it, it excites even in its rudimentary existence the elaborated fluids of the successful coition, and like it, it vivifies and

* *Mind and brain!* It is to be hoped that an improved acquaintance with the brain and its uses, will encourage the physiologist no longer to give to a mere function an individual existence. The brain happens to be the last of the bodily organs so perplexed with the spirit of the ancients.

shapes the hitherto shapeless spermatic matter partly into the beautiful containing ovum, and partly into the contained embryo.

The *nisus formativus*, we are told, occur to the genital matter, when this is mature, and committed to the uterus in a proper condition, and under proper circumstances, produces in it the rudiments of conception, gradually forms organs fitted for particular purposes, preserves this structure during life by nourishing the body, and reproduces as far as it can, any part accidentally mutilated.*

The "*nisus formativus*," says Dr. Elliotson, in a note, "produces a being generally resembling the parents, but occasionally different." It is understood, then, that exactly what Blumenbach and Elliotson, in common with other physiologists, claim for the *nisus formativus*, I claim for the solar ganglion. I cannot doubt that it exercises the *architectural* power which is employed in man and animals—from man downwards, through the whole of animated nature, to the very lowest link in the chain of being; that to its peculiar and vital influence must be conceded, upon the grounds before stated, the wonderful and successive metamorphoses or changes which characterize, not only the intra and extra-uterine existence of the human form, but also that of animals, whether oviparous or viviparous, and under circumstances both of normal and abnormal action. No one, I think, would presume to explain the *modus operandi* of this power or influence of the solar ganglion; it is sufficient for my purpose that it exists. This question is involved, equally with that of the *cause* of gravitation or attraction, in a too Cimmerian darkness for my optics to penetrate.

If, from any cause, the organic affinities in progress in the fœtus be interfered with—if the balance which must obtain in the distribution of the imponderable matters in the organic tissues be disturbed, the action of the organism and of the separate organs may be so modified as to give rise to the formation of some one kind or other of monstrosity. An injury done to a seed during its germination is seldom unattended with ill effects. The radicle or the plumule will, the one or the other, suffer, and perhaps both. The injury done may be confined to one of these parts, and yet it may very seriously affect the vital principle in operation, and so modify or aggravate the original mischief. It is just so with the fœtus; any abnormal change in the component matter of any of its parts, may prove irremediable, and the effects of which being then communicated

* Blumenbach's Physiology, translated by Elliotson, p. 492, "Of the '*Nisus Formativus*.'" The word "*nisus*," Blumenbach says, he has "adopted chiefly to express an energy truly vital."

to the solar ganglion, may excite so altered a vital action in it as to prove incompatible with the complete or normal development of either itself or of those parts dependent on it; and hence the existence of monstrosities, of acephalous, and other malformed children, among whom the physical characters of the biped are more or less substituted by those of beings lower in the scale of creation. "Human monsters," says Blumenbach, "are not unfrequently met with who strongly resemble the form of brutes;" and it is "because the '*nisus formativus*,' having been disturbed and obstructed from some cause or the other, could not reach the highest pitch of the human form, but rested at a lower point, and produced a bestial shape." He adds, strangely enough—"On the contrary, I have never once found among brutes a true example of monstrosity which, by a bound of the '*nisus formativus*,' bore any analogy to the human figure."

It may be asked, in the history of monstrosities, did any physiologist ever hear of *one* in whom there was no ganglionic system, no *solar ganglion*? I answer, no! Such an occurrence is wholly impossible. We have all heard of acephalous children, and of beings created without either *brain* or *spinal cord*. What, in such instances sustained intra-uterine life? what enabled the body of the creature to be nourished and developed? What, I ask, was the source of all this vitality? The solar ganglion and its branches, the ganglionic nerves! "In fœtuses, without *brain* or *spinal marrow*," says Blumenbach, "the circulation, nutrition, secretion, &c., proceed *equally* as in others, which, besides spinal marrow, nerves, and ganglion, possess a brain," and for the same reason, he might have added, that after the removal or destruction of the *brain* and *spinal marrow* in animals, the heart still continues to act, and the blood to circulate, provided respiration is artificially supported." The *reason* is just this, the *solar ganglion* and its immediate dependencies are unaffected. With these *facts* before us, then, I must confess I am in ignorance of the "*experience*" which "shows that when the influence of the brain and spinal marrow is intercepted, although the afflux of blood may for a time continue, yet the secretion ceases, and all the functions dependent upon secretion, such as digestion, cease likewise." We are informed, too, by the same author, strangely enough, that "the functions of digestion, circulation, absorption, secretion, and all those included under the class of nutrient or vital functions, are carried on as well during *sleep* as when we are awake," that is, as well during the total *inaction* of the *brain* and *spinal marrow*, when the "*influence*" of both, must of course be "intercepted." Strange *experience* this.*

* Roget's Animal and Vegetable Physiology, vol. ii., p. 358, 60, 361.

Dr. Marshall Hall, in his Lectures, published in 1842, "On the Pathology and Treatment of Nervous Diseases, evidently favours the opinion that the influence of the brain is necessary to the complete performance of the vital or ganglionic functions. "We find, says Dr. Hall, "that idiots with small brains are short-lived;" therefore "the animal functions cannot go on permanently independent of the brain." I take it that the imperfect development of the brain of the idiot must be regarded, only, as an indication of the mal-organized condition of the entire nervous system, including the spinal and ganglionic, not less than the cerebral.

In the very interesting case of acephalous monstrosity published by Mr. Lawrence, in which the brain *only* was wanting, we learn that all the excito-motory functions were duly performed; it gave evidences of pain, and "at first moved very briskly," and the sphincters performed their office. Such, of course, was referrible to the integrity, generally, of the spinal cord. "The child's *breathing* and *temperature* were natural; it discharged *urine* and *fæces*, and took food." The latter set of circumstances indicated the operation of the functions of the solar ganglion and its collateral parts; *and without any aid from a brain*. The ganglionic and spinal nervous systems were natural, and they exercised their respective functions accordingly.

In the remarkable case cited by Dr. M. Hall, of a *fœtus* born "without either brain or spinal marrow, without a particle of either of these organs *yet perfectly developed*," we have a very satisfactory proof of the independence of the ganglionic functions on either the spinal or cerebral nervous systems. The intra-uterine life may be quite perfect, although there may be neither brain nor spinal marrow; but what happens the moment the child is born? asks Dr. M. Hall; it cannot breathe: it cannot live an instant." It dies truly, but not because the brain or the spinal marrow, or both, are directly essential to the continuance of the function of the solar ganglion, but because that central organ, in the absence of the motor nerves commonly supplied to the respiratory muscles, is deprived of one most material agency or power with which it, the solar ganglion, is enabled to perform one of the many indispensable functions required by the animal economy. The blood cannot, under such circumstances, be decarbonized, and the circulation of diseased blood paralyzes the vital energies of every part of the organism. The effect is the same if a person with his cerebro-spinal and ganglionic systems entire, be made to breathe carbonic acid, or any other irrespirable gas. Life, then, in such a case of monstrosity, cannot be said to cease from

the want of any direct *cerebral* or *spinal* influence. The vital actions in the lower classes of animals which have no brain, and in those, too, which have neither brain nor spinal marrow, are not less completely performed than the same in man. Secretion, circulation, digestion, &c., are as elaborately and efficiently executed in the polypus and oyster as in man, and their breathing apparatus is more simple, and independent of any addition to the *organism*, whereby, in man, the nature of his dependence on, and relations to, the external world are explained. His responsibility then came to be understood, and his real position as a moral being to be justly appreciated.

The experiments of Dr. M. Hall, as given in his published Lectures, appear to me to be anything than satisfactory. He introduces the subject thus:—"Until very recently, we viewed the contents of the spinal canal as a cord of cerebral nerves, and the origin of a part of the ganglionic system. Now, gentlemen, it is very possible to remove the cerebrum, the centre of the cerebral nerves, and the *ganglionic system*, and yet leave *another kind* of nervous influence remaining in the animal body. I shall take this early opportunity of showing you a simple experiment. You see here an animal (a frog) from which the head has been separated, and of course I need not tell you that with the head the brain has been entirely removed; all the viscera have also been removed, and with the viscera every portion of the ganglionic system. Now I beg here to repeat, the cerebrum, the centre of the spinal cord of nerves, and all the ganglionic, have been removed from this animal, and yet, when I pinch the extremity, it moves so as to be obviously perceptible at the remotest part of this theatre. Thus, as I said before, we have here removed the centre of the cerebral system, and the entire ganglionic system. The brain, which we know to be the centre of all the sentient and voluntary nerves, has been removed, the ganglionic system has been removed, and yet you observe something remains. Now, gentlemen, that which remains I venture to call—in contradistinction from what has been termed a cord of cerebral nerves, and the origin of the ganglionic system—the *true spinal marrow*. It is plain, in the first place, that it is not a mere cord of nerves; if it were a mere cord of nerves, you might divide it, and then you would intercept its influence. But, if you observe here, this influence passes not only from one extremity to the other, but it also passes from the one set of extremities to the other set of extremities; thus, it is quite plain that there is a nucleus of nervous matter between the two anterior extremities, and another nucleus between the two posterior extremities by which these nervous links are united

and associated in their motions one with another. Having thus, then, clearly laid before you the distinction which I wish to insist upon—namely, that there is not a division of the nervous system into two parts only, but into three, pervading all the different parts of the whole animal frame, I shall venture to term them the cerebral, the true spinal, and the ganglionic systems.”

That Dr. Marshall Hall is perfectly right in considering that the contents of the spinal canal do *not* constitute a cord of cerebral nerves, and the origin of a part of the ganglionic system, there can be, to my mind, no doubt; but I cannot allow this opportunity to pass by me without claiming for the illustrious Gall the honour of being the *first* to render this anatomical fact clear and distinct. *He* it was who demonstrated that the spinal marrow only *communicates* with the brain.*

Dr. Hall has endeavoured, as it appears in the preceding extract, to prove by experiment on the frog, that on the removal of the viscera with the solar plexus and its ganglia, the spinal cord will still continue the excitomotory functions, the head (and of course, brain) being at the same time dissevered from the body, and which circumstance, of course, would prove, as the doctor affirms it does, “that there is not a division of the nervous system into *two* parts, but into *three*, pervading all the different parts of the animal frame.” I have on many occasions performed the experiment on the frog as detailed by Dr. Marshall Hall, but *I* have invariably found that the removal or destruction of the ganglionic system of nerves is fatal to the *life* of the animal. The vital or “*true spinal*” phenomena, in that case, continue no longer than the peculiar contractile irritability of the heart or extremity of the animal after the removal of either from the trunk; proving, therefore, most clearly, that the original power possessed by the spinal cord to perform its peculiar functions is derived from the ganglionic system. If the animal be *not* decapitated, the same result follows the removal or destruction of the solar ganglion, and which could not be the case if the influence of the *brain* were directly required by the spinal marrow. My own experiments, then, prove not only that there are *three* distinct nervous sys-

* Gall, “Sur les Fonctions du Cerveau,” t. ii. p. 77, quoted by Elliotson, in Blumenbach’s Physiology. “It is remarkable,” observes Dr. Elliotson, “how many discoveries of Gall’s that were denied or disregarded have been since made by others, and were frequently contested by two parties, he and his labours been never once thought of.” Mr. Solly in his work “On the human brain,” claims to have discovered that some of the fibres of the anterior columns of the spinal cord proceed backwards to the cerebellum, but it appears from the authority of Dr. Elliotson, that Gall taught and published the very same fact so long since as 1816, and that these same fibres, “decussate exactly like the anterior pyramids.”

tems united in man—viz., a cerebral, a spinal, and a ganglionic, but also that both the first and second are dependent on the third—that they derive not only their very existence and integrity from it, but also perform their respective functions in virtue only of the influence they receive from it, and that they are, as it were, employed by it to establish our dependency on, and relationship to, the external world, of which man forms a part.

The *cerebral* and *spinal* systems of nerves together perform the animal functions, which, in the words of an eminent physiologist, prove us feeling, thinking, and willing beings; they are the actions of the senses, which receive impressions of the brain, which perceives them, reflects upon them, and wills; of the voluntary muscles, which execute the will in regard to motion; and of the nerves, which are the agents of transmission: the brain is their central organ. But the ganglionic system of nerves, with the solar plexus for its central organ, performs the vital or organic functions, or rather, supplies to each viscus the power which enables it to perform its specific functions in the animal economy. Secretion, nutrition, exhalation, and absorption, being, then, under its immediate influence and control, it must preside equally over the brain as the stomach, equally over the spinal cord as the liver. In short, the vital force of the solar ganglion, the centre of the ganglionic system, holds the same relation to the whole organism that *steam* does to the several parts of an engine. The said “*vital force*” and “*steam*” are equally the motive power and it may be said, that to their different states or conditions must be attributed every kind of change, however slight, of which either the animal organism or the machine itself is at any time, and under any circumstances, susceptible.

The observations already made in reference to acephalous and other monstrosities, no less than those which relate to the experiments of Dr. M. Hall and myself, abundantly prove the physiological position I have here taken—viz., that *life*, regarded as the “assemblage of all the functions, and the general result of their exercise,”* has its immediate principle neither in the brain nor in the spinal marrow, nor in any of the viscera of the chest or abdomen, but in the *solar ganglion*; yet it is no less certain, that all these parts or organs are necessary to the maintenance or continuance of life, as it exists in man, and the great mass of the lower classes of animals. The brain, principally, because the mechanical phenomena of respiration seem to depend upon it;—the spinal marrow, because it exercises a guardian power over the acts of ingestion and egestion;—and the viscera of the chest and abdomen, because they are necessary to the formation and circulation of the blood.

* Lawrence's Lectures, p. p. 120, 121.

The only way that I am enabled to account for the discrepancy stated in the experiment of Dr. M. Hall and myself on the frog, is, that the excito-motory action which was produced by pinching or pricking the extremities of the animal, after the removal of the viscera and the ganglionic nervous system, must have resulted from the influence of that remaining nervous principle which exists, for a longer or a shorter period, in any portion of the animal organism, after, even, its removal from the trunk or body to which it originally belonged. It is well known that Le Gallois, Prochaska, and Hunter, taught that the nervous power is generated throughout the whole extent of the nervous system, even to the smallest nerves, and that it can exist, *for a certain time*, in the nerves of any part, independently of its source; and there can be, I think, no doubt of it. I have seen the heart of the *shark* contract vigorously, for even many minutes after its removal from the animal—a fact which proves, in the words of Hunter, that the nerves of a part continue the same action which they receive.*

I may here observe, that the experiments of Sir B. Brodie to disprove the assertions and opinions of Le Gallois, “that every part of the body derives its principle of vitality and irritability from that portion of the spinal marrow from which it receives its nerves,” are open to some objection. The experiments of Sir B. Brodie are contained in the late Dr. Cooke’s work on Nervous Diseases, and are as follow:—

Experiment 1.—Sir B. Brodie divided in a dog the skin and muscles which lie before the axillary plexus of nerves, and afterwards the nerves themselves. He then divided the remaining skin and muscles, the cellular membrane, and every other part connecting the anterior extremity to the trunk, with the excep-

† Since the above remarks were penned, I have many times repeated the experiment of removing the viscera with the ganglionic system of nerves in the frog, and I have found much additional reason to form the conclusive opinion I have—viz., that the excito-motory phenomena demonstrated in the experiment of Dr. M. Hall, must have resulted only from the operation of retained nervous influence in the limbs of the animal, constituting an exception to the general rule which goes to show, that on the removal or destruction of the organic system of nerves, or ganglionic system, the functions of the brain and spinal cord as necessarily cease as do those of the other viscera. I have found, that, if the abdominal ganglia only be removed from the animal, the circulation and respiration, together with the *spinal* functions of the *superior* extremities, will be continued for a time, and much longer than contractions will exist in the heart after its separation from the body; proving, therefore, that the superior or thoracic ganglia have the power, in some degree, to “continue the same action which they receive” from the solar ganglion. Le Gallois cut a young rabbit transversely into halves, and because the strictly spinal functions ceased with the destruction of the cord in either half, he declared that the vital principle was seated in it. It is unnecessary to add, that the destruction of the cord by Le Gallois involved only the loss of *one* among the many indications or external signs of a vital principle.

tion of the axillary artery and veins, so that the vessels were completely insulated, and formed the only connexion between the limb and the trunk. The divided edges of the skin were united by sutures. Twenty hours afterwards, an incision having been made in the fore-arm, the arteries bled freely, and the blood was of a bright scarlet colour. The muscles, by means of the voltaic battery, were readily made to contract, and when several pairs of plates were employed, the contractions of the muscles did not appear to be less powerful than those which arise from the stimulus of the will.

Experiment 2.—Sir B. Brodie removed the whole of the posterior part of the spinal marrow of a frog. The wound readily healed, but the hind legs became, of course, paralyzed. Five months afterwards, the muscles of the hind legs were found still capable of powerful contractions under the influence of the voltaic battery; at the end of six months more, the muscles still retained their contractile power. The frog was then killed. The wound was found completely cicatrized; there was not the smallest appearance of regeneration of that portion of the spinal marrow which had been destroyed.

I must add, that it appears somewhat strange to me ever to expect that animal matter should resist the effects of a power so intense as that employed by Sir B. Brodie. The contractile power of the muscles when subjected to the voltaic battery, I am disposed to consider irrespectively of the source of vitality and irritability.

The above experiments are cited on this occasion, not only because I consider them illustrative of the position of Hunter and other physiologists, as above explained, but in order to show, as particularly as I well can, the precise nature of the phenomena elicited by the experiments of Dr. M. Hall on the frog, as contrasted with the results obtained by myself.

From the preceding observations it will appear that the "*Formative power*" of Blumenbach, or the *Materia Vitæ* of Hunter, or the *Solar Ganglion*, call it by what name we will, is no less universal than the animal organism itself; and there can be no doubt that, like the animal organism, it observes corresponding varieties and gradations of form. It could hardly be expected that the "*Formative Power*" of the dog is similar to that of the man; or that of the first, of the reptile; and so on, through the whole of living things. The germ and its product must possess qualities common to both. It is impossible, perhaps, to demonstrate the *exterior* or physical characters of this "*Formative power*" in man or in animals, peculiar either to the same or to different species of each genus; yet is there an abundance of authority to show that such a specific difference must every-

where exist. The formative power, or the solar ganglion, regarded as the germ of all to be afterwards developed, may be said to represent, in common with the cerebral and other parts, in its successive changes in *utero*, its various and permanent conditions as they exist throughout the animal kingdom, beginning with the polypus, in which every vital action is conducted upon the smallest scale, by the least refined methods, and with the strictest economy of means; its apparatus being the simplest, the agents employed the fewest possible, and its various operations being carried on in one and the same place; proceeding in the mollusca, and in worms and insects, in which relations are exhibited to surrounding objects, and in which animals the senses and voluntary motion gradually make their appearance, the organic apparatus necessary to the exercise of these functions being superadded; ascending through fishes, reptiles, birds, and quadrupeds, in which the powers of sensation and motion become much more energetic, much more active, the internal life at the same time more and more developed, and the cerebral functions more and more numerous and diversified; and ending with *man* himself, in whom, as Blumenbach observes, the successive imposition of cerebral matter has reached its maximum; so that the summit of the nervous system, which corresponds with the forehead and vertex, is much larger in him than in any brute, and his intellect and moral feelings are proportionally greater.

Apropos, the comparative cerebral developement of man and brutes is, as is well known, employed to mark the rank any one of either kind may hold in the scale of creation and intelligence; and very properly so; but it appears to me that the physiologist takes cognizance only of an *effect* on the development of the cerebral organism, which has resulted from the operation of a cause which he has hitherto failed to appreciate. Nothing can be more true than that "the higher we ascend, the more parts exist above the *medulla oblongata*, till, rising from fish and reptiles, through the numerous warm-blooded brutes—all distinguished by the relative magnitude of each cerebral part—we arrive at man," the "*summit of terrestrial objects*;" and it is equally true to my mind, that the successive increase of parts above the *medulla oblongata* is attributable to the operation of a preliminary cause—viz., the *Solar Ganglion, the source of all animal life, whether cerebral, spinal, or organic*. Surely if the ganglionic system be deemed either necessary or competent to preside over the vital actions necessary to the perfect development of the amyencephalous monster of Dr. M. Hall; if the secretion, nutrition, circulation, &c., as carried on in it, in virtue of the said ganglionic system, are sufficient for

its growth and maturity, and for the perfectibility of its several organs, as the liver, spleen, heart, &c., and the consequent exercise of their functions, why should we doubt its powers to preside over the vital actions necessary to the entire fœtus? If the ganglionic system be sufficient to develop and mature a liver, a heart, and a spleen, and to excite them to the exercise of their appropriate functions in the animal economy, there is no reason why it should *not* do the same for the brain and spinal marrow. There is certainly much reason, as I trust has been shown, to feel satisfied that it does so; and that, moreover, to the modified operation of the solar ganglion must be referred all the varieties of life, psychical, and corporeal, which everywhere abound.

To proceed:—On tracing the animal organization from above downwards, we observe generally a gradual diminution or simplification of parts, and which in every case is proportionate to the wants and adaptations of the animal. The decreasing cerebral organism, for instance, marks the downward progress of the animal in the scale of intelligence and feeling. This is seen throughout the vertebrated animals, as the mammalia, birds, reptiles, and fishes. The insect tribes generally can hardly be said to have more than a mere rudimentary brain; they have certainly a medulla oblongata, in which the nerves of the external senses take their origin. The spinal system, so far, is perfect in all its parts, and all the strictly vital functions are in full operation. In the annelida, however, we mark a very considerable falling off: the absence of every thing like a brain, and of the external senses, leaves the animal to exercise only a strictly spinal and ganglionic existence. The excito-motory function, in itself, seems perfect, and like it, the organic functions appear on a par with those of the mollusca. The only advantage of the annelida is probably in its means of progression. One more step downwards, and the ordinary medusa is seen a mere mass of living gelatinous matter, without the least indication of even a spinal life, and nevertheless executing the organic functions of assimilation, digestion, secretion, circulation, nutrition, absorption, &c. This last step of the ladder constitutes the first of the intra-uterine life of the fœtus; and it may be said, that just so many steps as it takes the physiologist to trace the successive and organic changes from man to the medusa, the same are required to trace the fœtus through its various metamorphoses during its intra-uterine existence; and there can be no doubt, as has been before observed, that each diminution or addition of parts, both in man and animals, is but the visible effect of a corresponding variety and gradation of the *nisus formativus*, or the *solar ganglion*, the germ of all to be afterwards developed.

"First chain of being, which from God began,
Nature's ethereal, human, angel, man,
Beast, bird, fish, insect, what no eye can see,
No glass can reach, from infinite to thee :
From thee to nothing." POPE.

It may be added that the possession of the *three* nervous systems in man and in the higher order of animals does not presuppose that those which have only *two* or even *one*, have been insufficiently provided for. The organism of the polypus and oyster is as nicely adapted to their wants and the circumstances which surround them, as is the organism of man.

In many instances among the lower classes of animals it appears that Nature has intended so to economize her means that one part shall serve the purposes of two or even more, and in others she has so concentrated her forces that it is really astonishing. The reproduction of lost parts in the lobster and crab, and many other crustacea ; and the extraordinary activity of certain of the external senses in some animals higher in the scale of organization than the crustacea, almost make one doubt on which side to consider the advantages ! A division of labour is generally regarded as indispensable to perfectibility, but among the lowest classes of animals we find the opposite principle in operation :—thus the solar ganglion in certain animals executes the functions of both the spinal cord and the cerebrum ; for in the absence of the brain and spinal cord, it is occasionally seen that both sensation and voluntary motion exist in the zoophytes. In the manner in which the infusoria and actiniæ pursue their prey, and in their selection of it ; in the facility with which they recede from whatever may prove hurtful to them, and turn aside when they encounter one another, together with the highly sensitive and irritable nature of the organism of the medasuriæ generally, "we can hardly fail," as Dr. Roget truly observes, "to recognize the evidence of voluntary action." Herein we get at the nature of animal *instinct*, and which, in the absence of everything like a cerebrum and spinal cord, must be regarded as a specific function of the ganglionic system. Dr. Roget says, in his chapter on the "Comparative Physiology of the Nervous System," p. 538, "But whatever may be their extent, it is probable that the *sensorial operations* of the zoophytes take place without the intervention of any *common* centre of action," meaning thereby that the *sensorial operations*, so-called, are performed independently of a brain. The same may be said of the mollusca and articulatæ. There are times, moreover, when the *vertebral animals*, as fishes, reptiles, and birds, including the mammalia, and even *man* perform actions of an instinctive and intellectual character, and that, too, *without the intervention of any common centre*

of action. What is the course of that instinctive agency which determines the young among the several classes of animals above enumerated, including man, to seek each its particular means of support. In what originates the very keen *choice* which is displayed by all of them for that which nature has so especially predestined for their respective uses? What determines the peculiar habits and mode of life of any one of them? The immature condition of the brain in the infant being renders it perfectly impossible that it can exercise any, even the slightest, influence in the matter; and no one could venture to affirm that the spinal cord took any part in it. There can be no *reason* to doubt, but, on the other hand, every *reason* to feel assured, that to the same cause of the "*sensorial operations*" of the zoophytes, and of that peculiar instinct which enables the lower classes of animals generally to provide, not only for all the necessities of life, but even to guard against contingencies and anticipate difficulties, whether they relate to the kind of habitation, the mode of progression, or to the kind of food required for their sustenance; in a word, to the same cause, the formative power or solar ganglion, which so beautifully adapts their individual habits, pursuits, and inclinations, to their peculiar organic conditions, and so providently harmonizes the natural laws, must also be referred whatever of *instinct* is, at any time, manifested in *man*, including the vertebrated animals.

The sudden and peculiar shrinking of the *hydra* when under the influence of fear, and the extreme caution and dexterity displayed by the infusory animalculæ in avoiding obstacles of any kind while swimming together in myriads in a single drop, are *instinctive* vital actions, arising from an inherent preservative principle, derived from the solar ganglion, and similar in its nature to that which induces even us, in the *moment* of danger and doubt, to place our extended palm across the præcordia—thus the affected miss, though ignorant of physiology or pathology, and perhaps of all other *ologies*, if either alarmed, or professing to be so, at any sufficient or insufficient cause of personal danger, quickly applies her hand to the præcordia; as if the solar plexus screamed "*take care of me now.*" What more reasonable than to expect that that organ, the sum of whose function may be in one word described as *life*, should preside over actions of the kind mentioned above, so *indispensable as they are to both its integrity and well being!*"

The *instinctive* and *mental* (cerebral) faculties are occasionally seen acting in combination—*e. g.*, the martins which, in a spirit of retaliation and vindictiveness, built up the hole which had access to the nest that certain sparrows had robbed them of, and so buried alive the predatory occupiers in a grave of

their own seeking, afford an instance of a clear and distinct process of thought, of cerebation. The original construction of the nest was instinctive or ganglionic, but the subsequent act certainly cerebral; whilst both were perhaps of a decidedly "*intellectual character*." Dr. Darwin tells the following anecdote:—"A wasp on a gravel walk had caught a fly nearly as large as itself. Kneeling upon the ground I observed him separate the tail and head from the body part, to which the wings were attached. He then took the body part in his paws, and rose about two feet from the ground with it; but a gentle breeze wafting the wings of the fly, turned him round in the air, and he settled again with his prey on the gravel. I then distinctly observed him cut off with his mouth, first one of the wings, and then the other, after which he flew away with it unmolested by the wind." Now, I take it that the mere pursuit and selection of the fly by the wasp as its prey was an act purely *instinctive*, whilst the clipping off of the wings under the circumstances narrated shows *it* to have been cerebral.

The constructive habits of the bee, as shown in the mathematical accuracy with which each cell of the honey-comb is formed, are certainly ganglionic or instinctive in their nature and origin; if the same structure had even been conceived and executed by man, it would have been an act of the brain, and an equal perfection of it could only have resulted from habit and experience. Similar observations will apply to the beaver, and not less so to many birds, &c. Such must be directly seen to be the legitimate and only conclusion, from the fact, that in some animals, without even a vestige of brain or spinal marrow, analogous phenomena are presented to our attention; the ganglionic system in them, as in the amyencephalous monster of Dr. M. Hall, is the only power of any kind they can command; they possess none other, and therefore must it be conceded, that the "*sensorial operations*," so called, are occasionally performed independently of a brain; and what is more, "the actions which are at one time instinctive, may at another spring from a different principle."—[*London Lancet*.

On the Secreting Function of the Colon. By JAS. PAUL, M. D.
(Read before the District Medical Society for the County of Mercer.)

Although great and deserved attention is paid to the secretions in disease, both urinary a *fæcal*, and in a great many of the diseases to which the human frame is liable, particularly in fevers, there is no surer criterion to lead us in our prognostica-

tions, or guide us in our remedial efforts than the appearance of the excretions. I do not know if we are so thoroughly acquainted with the philosophy of the fæcal discharges as we ought to be, or that we view them altogether in the physiological bearing to which they properly belong in the animal economy.

We are, I think, too much in the habit of viewing the excrements merely as an index of the food having undergone the proper and necessary process of digestion, and when we see pieces of undigested aliment mixed up in the fæces, we naturally conclude and say that the substances consumed, whether of potato, apple, carrot, or whatever else has been partaken of, has not been digested.

Even this, however, is not without its use—for although in such cases the pressing symptoms, whether of croupy cough, nervous twitches, or convulsive spasms, are relieved by evacuating the alimentary canal of foreign and irritating substances, it yet enables us to note what portion of the digestive function is complete, whether the deficiency lies in the non-rendering the vegetable food into the saccharine principle, or otherwise, and so to alter the food to that which can be digested, and direct our remedial efforts to that portion of the function which is deficient.

My purpose at this time, however, is not with the function of digestion, but to direct our attention to the fæcal secretions or excretions, and to the colon, or large intestine as a great secreting organ.

Every practitioner is more or less acquainted with the appearance of the secretions as they are passed from the body of a patient laboring under fever—the brownish watery discharges having a cadaverous or fleshy smell, the black or dark green discharges resembling blubber, or the green fat of turtle, having a highly offensive and putrid odor—and the gradual return to the yellowish watery discharges having as convalescence is established, more consistence, and the more genial odor of proper fæces. I do not intend to enter into, neither is it needful that I should; the various appearances of the fæcal discharges in disease, nor the altered appearances caused by various remedial agents.

It has been a question among physiologists of the older school, whether absorption takes place in the larger intestines? On this subject, Blumenbach has the following—"It has been inquired whether lacteals exist also in the large intestines, and their existence has been contended for from the effects of particular injections, nutrients, inebriating, &c., and also by the circumstance that the fæces if retained for any length of time become hard and dry. Although these arguments do not

demonstrate the absorption of genuine chyle below the valve of Fallopius, nevertheless, it is rendered probable by the visible existence of an abundance of lymphatics in the large intestines having the same structure and function with the lacteals, for these absorb lymph from the intestines during the absence of chyle.

“But the very different structure of the internal coat of the large intestines from that of the villous coat of the small, strongly argues that they are not naturally intended to absorb chyle.”—*Blumenbach*, 233.

Our present views of the transudation of liquids through animal texture, will readily enable us to comprehend how absorption may take place, and nourishment be conveyed into the system when thrown into the large intestines, and even only into the rectum by means of injections. Nor is it at all incompatible with physiological facts that absorption and secretion should go on in the same organ, and through the same texture by different sets of vessels.

The same unsatisfactory knowledge, if I may be allowed the expression, exists regarding the functions and uses of the *mesenteric glands* of the colon. Prof. Grant, treating of these organs, says: “There are nearly a hundred of these organs on the human lacteals, and about a fourth part of these belong to the colon; but the changes they effect on the fluids which are incessantly passing through them during life, and even for some time after death, or the uses to which they are subservient in the economy, are still unknown, like the functions of many other obvious parts of our most complicated and wonderful fabric.”—*Prof. Grant's Lectures*, Jan. 26, 1824.

Following up the argument of the absorption of chyle, and its having been seen in the mesenteric veins, Blumenbach says: “The assertion that chyle has been seen in the mesenteric veins requires further investigation and proof; so that I cannot believe that they carry anything more than blood, being *carbonized* and destined for the formation of bile.”—*Blumenbach*, 234.

Here, then, we find the blood loaded and surcharged with that principle, of which a great portion of the *fæces* is composed.

Having thus briefly alluded to the views generally and formerly entertained by physiologists, let us enter more minutely into the structure of that portion of the large intestines in which this most important function is situate. “A part of the *fæces*, however (says Carpenter), may be derived from the secretions of the enteritic mucous membrane, and of its glandulæ; the surface of the former, with its simple follicles,

probably secretes nothing but mucus; but the glandulæ with which it is so thickly studded appear to serve as the channel for the elimination of putrescent matter from the blood. There can be no doubt that a large quantity of fluid is poured out by these glandulæ when they are in a state of irritation from disease, or from the stimulus of a purgative medicine; since the amount of water discharged from the bowels is often much greater than that which has been ingested, and must be derived from the blood.”—*Carpenter*, 501.

For a description of these glandulæ, allow me to transcribe from the same author the following: “The whole mucous surface of the intestinal canal is furnished with glandular follicles of a very similar character; of which some approach those of the stomach in complexity of structure, whilst others evidently correspond with the crypts of ordinary mucous membrane. An innumerable multitude of pores are easily seen by the aid of a simple lens to cover the whole internal surface of the large intestines, and these are the entrances to tubular follicles closely resembling those of the stomach, but more simple in structure. Their cœcal extremities shut against the submucous tissue; towards the end of the rectum, however, they are much prolonged, and constitute a peculiar layer between the mucous and muscular coats; the tubes which are there visible to the naked eye being erect, parallel, and densely crowded. These glands probably from the peculiarly thick and tenacious mucus of the large intestine.”—*Carpenter*, 668.

And of the functions of this glandular structure, the same author observes, “Although the particular use of each variety of the intestinal glandulæ cannot yet be determined, there seems little doubt that their general function is to eliminate from the blood those putrescent matters which would otherwise accumulate in it; whether as one of the results of the normal waste of the system, or as produced by various morbid causes which act as *ferments*, and thus occasion an unusual tendency to decomposition in the solids and fluids of the body. That the putrescent elements of the fæces are not derived from the food taken in, so much as from the excreting action of the intestinal glandulæ, appears from this consideration among others; that fæcal matter is still discharged, even in considerable quantities, long after the intestinal tube has been completely emptied of its alimentary contents. We see this in the course of many diseases where food is not taken for many days, during which time the bowels have been completely emptied of their previous contents by repeated evacuations, and whatever then passes in addition to the biliary and pancreatic fluids must be derived from the intestinal walls themselves. Sometimes a co-

pious flux of putrescent matter continues to take place spontaneously, whilst it is often produced by the agency of purgative medicine. The 'Colliquative Diarrhœa' which frequently comes on at the close of exhausting diseases, and which usually precedes death by starvation, appears to depend not so much upon a disordered state of the intestinal glandulæ themselves, as upon the general disintegration of the solids of the body, which calls them into extraordinary activity for the purpose of separating the decomposing matter."—*Carpenter*, 670.

What I have just read is so comprehensive, and brings the subject so forcibly and powerfully to the mind as to preclude the necessity almost of entering more fully upon it.

My attention was particularly drawn to this subject by the frequent occurrence of immense quantities of the morbid and putrid discharges by stool, in tropical fevers immediately before returning convalescence. At the commencement of the disease, the alimentary canal would be carefully emptied by repeated doses of purgative medicine, the fever would continue, watery stools would supervene; at this period the patient would take the simplest nourishment, and that in small quantities, and in many cases none at all, the stomach rejecting every particle of food exhibited—in the progress of the disease, the patient prostrated and nearly fainting on the least exertion, large dejections would occur of dark-colored gelatinous offensive matter—quarts, and I may say gallons on some occasions, are passed off at repeated operations—and although the patient at this time would be scarcely able to move or speak, yet after such evacuations he would feel more easy—a moisture appear on the surface—the critical moment being seized, and nourishment with wine or brandy exhibited—the patient slumbers, and from that time convalescence progresses.

And what is the result if this dark offensive matter is not thrown off? It is more than probable that the fever will continue, and in more favored climates a slow and dilatory convalescence may ensue, or the whole system becomes corrupted, and *in a tropical climate* putrefaction succeeds almost ere the being has ceased to breathe.

In the epidemic which has so lately made such havoc and run its course in some cities of the Union, causing such fearful mortality, the non-performance of the proper functions of the secreting glands of the intestines is no doubt a principal effect. Without entering into the manner in which the morbid poison of the cholera acts on the system, we see an abeyance of the proper secretions—of bile, urine, and fæcal discharges, and in their stead a watery secretion is ejected, even with force, from the stomach and intestines, without straining, and without pain ;

indeed, so offensive is the presence of this secreted fluid to the stomach and intestinal canal, that the patient can scarcely control its ejection for a few seconds. And this unusual parting with the serous portion of the blood leaves the remaining portion thick, viscid, and incapable of entering the minute or capillary vessels, and collapse is the consequence—but arrest the serous discharges, and once produce a fæcal evacuation with tinges of biliary secretion, and there is every chance of the recovery of the patient. Hence, it is obvious that the secreting organ of the large intestines is seriously affected in this formidable disease. I call it formidable from the fatality attending the visitation, but in my opinion controlable in a great majority of cases where the patient has been timely put under the care of the physician, and remedial and energetic measures have been pursued.

Every practitioner will no doubt bring to his recollection cases in which the patient, even after repeated and free evacuations, will answer to the inquiry regarding his feelings, “I am better—my medicine has acted very well—still I feel as if there was yet something to come away.” Is it not probable that this feeling, indescribable to the patient, not amounting to pain, and relieved by a copious evacuation has been owing to the surcharged state of the mesenteric veins, and the relief the consequence of the active secretion from the *glandulæ* which has been the subject of consideration.

The secretion of the liver is looked for, and the returning appearance of bile in the fæcal discharges is held in great estimation, and properly so, by most, if not by all practical physicians; its proper action is absolutely necessary to recovery from disease and the enjoyment of health. It is not my object to withdraw attention from that most important organ, but to direct more particular attention to the colon as a great secreting organ; that the fæces, which in health may contain that portion of the food which has not been absorbed into the system, more especially when a superabundant quantity of aliment has been consumed, is for the most part secreted by the large intestines, from which the deleterious and disintegrated portions of the organic mass is passed away, and the system freed of much of the superabundant carbon which may not be required for the purposes of respiration.—*New Jersey Medical Reporter*.

On the Functions of the Liver. By M. BERNARD.

M. Bernard in a recent course of lectures delivered at the “College of France,” reiterated his views respecting the functions of the liver, and endeavoured to confirm them by addi-

tional experiments. Although we have already adverted to the various papers he has from time to time published on the subject, a brief recapitulation of the points he believes proved may not be uninteresting, and will be best managed by stating them in the form of propositions.

A. *On the Formation of Sugar by the Liver.*

1. In an animal that has been prevented access to saccharine and amylaceous food, the blood entering the liver contains no sugar, but that which leaves it always contains it. Dogs fed for six weeks exclusively on meat present large quantities.

2. It is not only the blood of the organ, but its tissue also, that contains it in abundance.

3. It is found in the livers of all the domestic animals, in birds, fish, reptiles, and even oysters and snails. Towards the fifth month it is found in the foetal liver, and continues increasing; and it exists even in the embryos of oviparous animals.

4. The quantity calculated from actual measurement to be contained in the liver of a healthy adult, who was guillotined while fasting, was 23 grammes 267 milli-grammes. In a diabetic subject, the liver contained 57 grammes. In the liver of an ox the total quantity was calculated at 243 gr.

5. As a general rule, there is most sugar in the liver of those animals which consume aliments containing sugar. The longer the abstinence the less the quantity.

6. There is more found in adult animals than in young.

7. Although repeated experiments constantly confirm M. Bernard's original assertions that irritation of the olivary bodies of the medulla oblongata induces an almost immediate increase in the quantity of sugar, yet the supposition it did so by irritating the origins of the eighth pair of nerves is erroneous; for if these nerves be divided, the increased secretion still goes on.

8. Whether the left or right olivary body, or the interval between them, be pierced, the quantity of sugar is alike increased in the urine; but in the first two cases the animal turns continually to the left or the right, and in the last progression occurs in a straight line. The quantity produced, within certain limits, is increased with the size of the instrument used. In rabbits the sugar-secretion continues for forty-eight hours after, and in dogs for four or even seven days. The same results follow, whether the animal is fasting or not.

9. During this increased secretion, the animal is in constant agitation; its respiration is accelerated, but its temperature is diminished some degrees. The quantity of urine is increased, besides becoming saccharine.

10. The sugar once produced whether by the liver or by means of aliment, undergoes destruction in the lungs; but its

disappearance is not, like its production, under nervous influence, but is a chemical phenomenon which may take place in contact with air externally to the lungs. The destruction in the lungs gives rise to the production of carbonic acid, which is liberated from the air passages. In animals whose olivary processes are pierced, this is given out in larger quantities, and their blood becomes blacker.

11. This diabetic sugar is especially distinguished by the large quantities which can be destroyed by the lungs, being as 15 to $2\frac{1}{2}$ as compared with grape sugar. Cane sugar introduced into the blood does not disappear by the lungs, but escapes by the urine.

12. Various circumstances may impede or prevent the secretion of sugar by the liver, as severe pain or lesion of the nervous system (except of the olivary bodies). Diseases may produce the same effect. Thus sugar has been found to cease being secreted in diabetics during the paroxysm of ague, in pneumonia, &c.

13. Additional experiments have shown M. Bernard the error of the hypothesis he advanced, that glycosuria was due to an affection of the pneumogastric nerves; and he is now disposed to regard it as due to a special although unknown alteration of the liver itself, which is indeed generally hypertrophied in this disease.

B. *Formation of Fatty Matters by the Liver.*

1. In spite of the great variety of fatty substances, animal and vegetable, the animal that consumes them always produces the same description of fat, owing to the elaboration they have undergone in the economy.

2. To become absorbed, fatty matter must previously have become emulsioned, and the *pancreatic juice* is the fluid by which this is accomplished. Its power of effecting this depends upon an organic matter analogous to ferments.

3. The amount of fatty matter introduced as food is far from explaining the quantity possessed or produced by the individual. There is no fat in the blood which enters the liver, but there is abundance in the blood that leaves it, and therefore its formation within this organ is certain.

4. It is, as in the case of sugar, during digestion that fat is produced in the liver, and after abstinence it disappears. Sometimes it is very abundant, especially in suckling women; whence probably arise the fatty matters of the milk, for the fat of the liver offers most analogy to *butter*. In such women, the blood itself contains much fat.

5. The fat received in the food after decomposition by the pancreatic juice, and the fat from the liver, both enter the

blood, and are not entirely destroyed in the lungs, in as much as the arterial blood still contains abundance. As the venous blood contains hardly any, and the vena cava none, we must conclude that the greater part is destroyed in the general capillary system.

6. The production of fat in the liver seems, like that of the production of sugar, to be under the influence of the nervous system; and if this undergoes violent lesion or perturbation, its production ceases. It is remarkable, too, that in proportion as the quantity of sugar increases after puncture of the medulla oblongata, that of the fat diminishes. The same is observed in diabetes; for from the livers which are loaded with sugar, not an atom of fat can be extracted.

7. Although healthy urine has been shown to contain some fat, yet this sometimes predominates so as to constitute a disease under the name of *fatty or chylous urine*, frequently coexisting with a similar state of the blood. This is probably due to the superabundance of fat secreted by the liver, and constitutes a (so to say) *fatty diabetes*, just as the excess of sugar does a saccharine one.

8. The products of digestion may thus be said to induce three principal diseases—the sugar, diabetes; the fat, the so-called chylous urine; and albumen, the albuminous diseases.

c. *Fibrin formed in the Liver.*

1. The blood entering the liver contains little fibrin, and coagulates with difficulty even when the animal is fed on flesh. The fibrin of the aliments is dissolved by the gastric juice, and converted into a matter analogous to albumen, termed by Mialhe albuminose. But the blood which quits the liver contains much fibrin and therefore the albuminose of the blood of the abdominal veins has become transformed into fibrinous matter.

2. It is during digestion that the blood, traversing the liver, becomes loaded with this abundance of fibrin.

d. *Secretion of Bile.*

1. The secretion of bile differs from those already named, as it is continuous, while they occur only during digestion.

2. The bile is not a mere excrementitious fluid, but influences digestion usefully, contributing with the gastric and pancreatic juices to constitute that most powerful solvent—the intestinal liquid.

3. The bile seems to be essentially endowed with anti-putrefactive properties. It regulates the chemical reactions occurring during digestion, prevents fermentation, and opposes the formation of the gases which result from the decomposition of azotised and non-azotised aliments.

4. When bile is prevented reaching the intestine, putrefactive fermentation, no longer opposed by the acids of this fluid, induces diarrhœa; which may also be induced by a predominance of alkali in the intestines, and may be under such circumstances advantageously treated by acids.

d. *Résumé.* From what has preceded, it is evident that *sugar*, *fat*, and *fibrin* are fabricated in the liver, and that whatever the alimentation may be, this organ transforms it into appropriate matter of nutrition, so that the great variety of food taken does not derange the composition of the blood and prevent its being identical. In a chemical point of view the organ is then of great consequence, and is properly regarded as one of *sanguification*. While in the herbivora, whose aliments furnish much sugar, this secretory organ furnishes least sugar, in the carnivora, who ingest much fatty matter, it secretes least fat. So, too, the liver forms fibrin abundantly, in proportion as the vena portæ contains less. The liver, therefore, while it is an organ of *sanguification*, is always one for *adjusting the equilibrium*.

If the above products furnished by the liver serve to keep the circulating fluid in a composition essentially fit for nutrition, the *bile*, acting in opposite direction, contributes to the same end, by removing the principles that are in excess, especially carbon; so that the liver must be considered not only as an organ of *sanguification* and *equilibrium*, but also one exerting a *depurative action on the blood*.

e. *Circulation of the Liver.*

1. Two powerful causes—the pressure of the abdominal viscera and the venous aspiration—contribute to this; but other contrivances are required to meet the varying degrees of plenitude to which the organ is liable.

2. Certain vessels seen easiest in the horse, communicate directly between the vena portæ and vena cava, conveying a portion of the blood without its having undergone modification in the capillary circulation of the liver, and operating as a kind of *diverticulum*, preventing the organ from being too greatly and too suddenly engorged.

3. Congestion of the liver and heart, in the case of unusual afflux of fluids, is further provided against by the agency of a special *hepatico-renal circulation*, not so distinct in man, in whom the liver is liable to become congested by over-exertion, as in the horse, in whose liver this does not produce the same effect. [We have already (No. X., p. 246) described the apparatus by which this is supposed to be carried on, and expressed our conviction of the fancifulness of the suggestion.]

4. The active and abundant circulation through the liver is

an important source of *animal heat*, the blood leaving the organ having acquired an additional temperature of about 2° Fahr.—*L'Union Médicale*, 1850, Nos. 82, 83, 88, 91, 98, 103, 106, 113, 115.

We think that there is quite enough of probability in some of M. Bernard's assertions, to encourage further enquiry; but we must enter our protest against the hasty and dogmatic manner in which he builds up his conclusions on a very slender basis of evidence. It will be seen, even from his own admissions, that he has done this in regard to the influence which he supposed to be exerted through the eighth pair of nerves on the production of sugar; and the recent researches of Frerichs and Lehman on the Chemistry of digestion (of which we shall give an account in our next Number,) have shown him to have been no less hasty in his deductions on the agency of the pancreatic fluid.]—*Medico Chirurgica Review*.

Electro-Physiological Researches. Seventh Series. By Signor C. MATTEUCCI.

In this memoir, Professor Matteucci, after recapitulating the results of his previous researches on Electro-physiology, published in the 'Philosophical Transactions,' proceeds to the relation of new experiments. He first shows that nervous filaments made to conduct an electric current in a liquid are not capable, like metallic wires, of acting as electroids, and giving rise to electro-chemical decomposition. The solution employed was that of iodide of potassium; the nerves, two large ones, taken from a living animal, each of which was separately attached to the metallic extremities of a pile of fifteen couples. No trace of decomposition followed; and he concludes from hence, that the conductivity of nervous matter is due to the liquid part of the matter itself.

He then gives further experiments on the relative conductivity of muscles and nerves, with a view to ascertain whether, when a current was impelled through a mass of muscles, any part of the current might have passed through the nervous filaments spread through the muscle. For this purpose he inserted the nerve of a galvanoscopic frog into a hole made in a piece of dead muscle, through which he then passed a very powerful current: no contraction followed in the galvanoscopic frog. When muscles still retaining their irritability were substituted for the dead muscle, induced contractions occurred in the galvanoscopic frog during the passage of the current. He concludes that when the poles of a pile of twenty-five or thirty elements are applied to the surface of the muscles of a living

animal, the phenomena produced by the passage of the current must depend either on the *direct* action of the current on the muscular fibre, or on the *indirect* action or *influence* of the electric current transmitted by the muscular fibre to its own nervous filaments, or rather to the nervous force existing in those filaments.

Referring, then, to an experiment related in a preceding paper, in which the lower limbs of a frog, united to the spine only by the lumbar nerves, are placed astride two glasses containing water, with each foot immersed, and in which a current, after traversing the two limbs, and consequently the two nerves, in opposite directions, so modifies at length the excitability of the nerves, that, on opening the circuit, only the limb in which the current has been passing inversely contracts, he shows that if in this state what may be called the "inverse" nerve be touched by a piece of muscle, although the circuit is continued, yet the limb contracts as though the circuit had been broken. In fact, the muscle, by its greater conductivity, becomes traversed by the current in place of the nerve. Again, if after the former part of the experiment has been performed, the portions of nerve which had hitherto been buried among the crural muscles be dissected out, it is easily seen that their excitability has not been affected like that of the lumbar nerves, because the current, in place of traversing them, has traversed only the crural muscles. The nerve has had its excitability modified in only that part of its course in which, being laid bare and isolated, it has necessarily conducted the current.

M. Dubois Reymond ('Comptes Rendus') has related an experiment seeming to lead to the inference that section of the spinal marrow increases the excitability of the lumbar nerves, at least during a certain period of time. In order to test the accuracy of this conclusion on so important a point, M. Matteucci institutes a number of very accurate experiments, in which he measures the excitability of the lumbar nerves after section of the spinal marrow, by means of the apparatus of Breguet, used and described by him in a former paper. His first results show that "the contraction excited in the muscles of a frog, of which the spinal marrow has been divided from twelve to eighteen hours, is *stronger* than that obtained under the same circumstances from the muscles of a frog just killed, without having been previously subjected to any injury to its nervous system." But subsequent experiments have satisfied him that this result depends not on the separation from the spinal marrow, but rather on the repose in which the muscle has been permitted to remain; for without division of the medulla, nearly the same force of contraction existed after the same

interval of time. He finds, indeed, that the only alteration which the excitability of a nerve undergoes by separation from the nervous centres, consists in its being more readily exhausted under the action of stimulants, the longer the period that has elapsed since its detachment.

The author then proceeds to relate the nature of the strict analogy existing between electricity and nervous force. As electricity is developed under the influence of the nervous current in the organs of electrical fishes, so, as a converse of this phenomenon, electricity may develop the nervous force. After adverting to the well-known analogy subsisting in every particular between the phenomena of the electrical organ and those of muscles, he adverts to the old experiment of passing a current through the muscles of the thighs of a living animal, the positive pole being placed now above, now below, so that it may be supposed that the current passes in the two cases of opposite directions as regards the nervous filament distributed in the muscles. He then points out that the effects of a current directed downwards, in the direct course of the nerves, are a strong contraction of the muscle traversed, and also of the *muscles of the leg below*; while the effect of a current in the opposite or inverse direction is *pain*, together with contractions less violent and always confined to the muscles traversed. The *contractions* (especially of the parts below) indicate a current of nervous force propagated towards the muscles, while the *pain* indicates a current towards the nervous centre. Now, bearing in mind that it had been proved by direct experiments that an electric current traversing a muscle never quits the muscular fibre to enter the nervous filaments, it seems clear that the phenomena just spoken of are exclusively owing to the *influence* exerted by the electricity passing through the muscles on the nervous force contained in the nerves; and also that this nervous force acts peripherad or centrad, according to the direction of the electric current which excites it. The great importance of the conclusions drawn from these experiments consists in this, that they lead to the same law which establishes the analogy between nervous force and the electrical discharge of fishes. The paper concludes with some further considerations intended to confirm this law.—[*Philosophical Transactions*, 1850.]

On the Treatment of Rheumatism by Lemon Juice. By G. OWEN REES, M. D., Assistant Physician to Guy's Hospital.

Although the treatment of rheumatic diseases by lemon-juice has received a considerable amount of favourable notice from

the profession, I am still inclined to believe that it has scarcely gained the credit it deserves. This would appear to depend in some degree on its use in cases which experience has now shown me are but ill-adapted for its exhibition, and which are in all probability improperly placed by our pathology in the same category with those forms of the disease in which early benefit is derived from the administration of the remedy. There is much indeed in the history of those conditions designated rheumatic to lead us to hope, that as medical science advances, important distinctions will be made where none are as yet recognized.

From the experience I have now had in the use of the remedy, I feel I have sufficient evidence before me to justify the opinion, that there are certainly two forms of rheumatic disease which cannot be benefited by the administration of lemon-juice.

The first of these is generally observed in cachectic subjects, and for want of a better name I shall call it cachectic rheumatism. It occurs in all classes of life, but more commonly, I believe, among the lower orders. It is more frequent in females than in males. The swelling and redness of the parts affected are less marked than is generally the case in acutely painful rheumatism. The pain is, however, very severe, and occasionally partakes of the neuralgic character. The patient is sometimes anæmic, and on inquiry we may perhaps discover a history of long mental or physical suffering. The skin is perspirable; the pulse weak and rapid; the tongue varies, being sometimes moist and white, and sometimes clean and less moist than natural. In cases of this description I have not succeeded in relieving the patient by the exhibition of the juice; and if occasional relief has been obtained, the disease has shown a tendency to relapse, and become unmanageable under a continuance of the remedy. It may perhaps be well to mention, that in these cases I have derived the greatest benefit from the exhibition of opium in full doses at frequent intervals. The other form of rheumatism in which I have failed to obtain relief by the administration of the juice is that attendant on syphilis. In all the cases in which I have made trial of it among the female out-patients at Guy's Hospital, it has failed to exert any beneficial influence. The nature of the disease, so distinct from that of ordinary rheumatism, never indeed gave me any great hope of success. If we except the diseases above described, and cases simulating rheumatism, but really connected with ordinary dyspepsia, or, as is sometimes the case, with the existence of Bright's disease, my continued experience has but the more persuaded me of the great value of lemon juice as a remedy for rheumatism. Its action is most remarkable causing

cessation of pain and decrease of swelling and redness, such as we can rarely obtain with colchicum, even when administered in large and hazardous doses. That lemon juice sometimes fails to effect this rapidly is certainly true, and that too with respect to cases apparently identical with those in which early benefit has been observed; but the history even of these less favourable instances will generally bear comparison with the results obtained by the ordinary plans of treatment; and it is my full conviction, that since lemon juice has been introduced at Guy's Hospital as a remedy for acute rheumatism, the period during which it has become necessary to confine patients so affected to bed has been very materially lessened.

I am anxious now to direct attention to a class of chronic rheumatic cases, in which I have used lemon juice with very great advantage. I allude to such as are connected with deposit of lithate of soda in and about the smaller joints, and which partake more or less of the gouty character. I have met with great success in this form of disease by the continued use of lemon juice in combination with small doses of the tincture of the sesquichloride of iron, and in several instances have effected absorption of deposits which have resisted all other plans of treatment. A case of the above description was lately reported to me, in which like success attended the administration of the lemon juice alone. The patient, a lady, had been a cripple for several years, and was eventually restored by persevering in the use of the remedy for six or eight weeks.

Lastly, with respect to the dose in which the juice ought to be administered. Experience has shown me that it should be larger than I was at first inclined to consider necessary. In acute rheumatism, from one to two ounces should be given every four or six hours; and should pain be felt in the bowels, or diarrhœa occur, which is very rarely the case, four or five minims of tincture of opium may be added to each dose of the remedy.—[*London Lancet*.

Cases of the Termination of Acute Rheumatism in Suppuration.

By MM. FLEURY, ANDRAL, and TROUSSEAU.

A prolonged and interesting discussion has recently arisen at the Académie de Médecine, upon the occasion of the presentation of a report by M. Martin Solon, upon the proposition of M. Dechilly to treat acute rheumatism by covering all the affected joints with large blisters, a plan, as might be supposed, that met with little favour. The opportunity was taken to enter into the question of the nature and treatment of the disease at large, and several sittings were occupied in discussing this. We

have no intention of following the various speakers, since much that was said consisted of a reiteration of what is already known, or has been sufficiently refuted. M. Bouillaud defended his doctrines with his accustomed ability and want of success; and M. Piorry disputed with him the parentage of the practice of bleeding *coup sur coup*, which every one else has abandoned as mischievous. We will briefly notice a portion of the address of M. Malgaigne.

M. Malgaigne, after expressing his conviction that physicians would be less discrepant in their opinions concerning the disease, if they studied first its monarticular form, as found in the surgical wards; and, adverting to the fact of the utter ignorance which prevailed concerning inflammation, until Hunter studied its phenomena, at the surface of the body, and scattered the former hypotheses of physicians to the winds; stated that suppuration is by no means an uncommon termination of the disease, but that it then comes under the care of the surgeon. He believes that physicians are too easily contented with the removal of the general symptoms, and allow a patient to leave the hospital when a considerable amount of pain or debility of the joint continues. Under favourable circumstances, Nature herself will complete what is sufficient; but, at other times, the patients resort to the surgical wards, "to have their cures cured." He thinks, too, that in appreciating the employment of different remedies, we are too ready to yield them praise as they successively appear, without reflecting upon the natural tendency of this, and of all articular inflammations, to a cure, provided the parts are kept in quietude, and free from the irritation of external agents. He believes that amidst the multiplicity of formulæ, the natural indications are often neglected; one of the principal of which he considers to be, to protect the perspiring surface from the effects of cold, which, in most cases has indeed been the original cause of the disease. The perusal of all modern cases proves the great inattention showed to this point, the patient being allowed to uncover himself to satisfy every want, or undergo any application; and in this way the disease may be rendered obstinate and relapsing.

Some of the speakers laid considerable stress upon the rarity of the termination of rheumatism in suppuration, and this has led to the publication in the medical journals of some examples of it. Professor Fleury, of Clermont, states that, up to 1848, he had always believed with M. Chomel, that rheumatism never so terminated; for, practising in a part of the country wherein the disease is of frequent occurrence, he had never met with a case of such before, although he had frequently had cases transferred to his surgical wards from the physicians.'

under the idea that such was the case. The present case occurred in a youth, æt. 18, of sound constitution, and hitherto of good health. He was admitted into the Hotel-Dieu, at Clermont, 2d October, on account of acute rheumatism of the shoulder and knee, induced by sleeping in a damp chamber. Suppuration was set up, notwithstanding active antiphlogistic treatment, in both joints, the abscesses being left to discharge themselves; and he died on the 12th of November, of purulent infection. On examination, the articular surfaces were found denuded, a metastatic abscess existed in the right lung, but no other disease of any of the viscera was found.

M. Andral read the next case to the Academy. A woman, æt. 67, of feeble constitution, was admitted into La Charité early in July last, suffering from pneumonia, from which she rapidly recovered by bleeding and tartar emetic, and was in a state of convalescence, when, from the effect of a current of air, she became attacked with acute rheumatism of the shoulders, unattended with complication, the attendant fever being great. She was bled, which so enfeebled her as to forbid the repetition; and next day twelve grains of quinine in the twenty-four hours were prescribed. The disease proceeded on to a fatal termination with fearful rapidity, without any complication occurring, any anormal sound of the heart, or any other joint participating. She died on the eighth or ninth day, having exhibited no other symptoms than intense pain in both shoulders, a pulse of constantly increasing frequency, and a state of general anguish and rapid sinking, resembling that observed in acute peritonitis. The most careful examination of every organ failed to elicit any explanation of the issue; nor were there any signs of phlebitis or purulent infection. Both shoulder-joints, however, were found filled with white, homogeneous pus, and the synovial membrane was of the intensest red, the articular cartilage retaining its normal colour. Some of the bursæ in communication with the joints were also filled with pus; but external to the joints all was normal, bones, periosteum, and muscles having undergone no change whatever.

M. Trousseau relates a case also, which occurred in a child æt. 9, who, having always enjoyed good health, and being unexposed to privations, was attacked with intense scarlatina, and, during its prevalence, various of the joints became the seat of acute rheumatism. She died on the fifth day of the scarlatina, and third of the rheumatism. On examination, the various organs were found in a healthy condition, with the exception of the pleura, which contained a considerable quantity of serosity. Both shoulder-joints, and those of the elbow, knees, and ankles, were filled with considerable quantity of greenish, well-formed

pus, accompanied by considerable vascularity of the subsynovial cellular tissue.

M. Trousseau observes, that all practitioners who have much observed disease in children, must be aware what a powerful effect the exanthemata exert in engendering a *purulent diathesis*. If this child had contracted the rheumatism unconnectedly with the scarlatina, it would have probably determined the synovial effusion usually found; but the scarlatina having changed the *crasis* of the blood, developed the suppurative diathesis, and converted a trifling affection into one of an irremediable character. Was not the old woman, whose case is related by M. Andral, reduced, M. Trousseau inquires, by the debilitating treatment to which she was subjected, to a condition analogous to that in which scarlatina so often places children?—*Bulletin de l'Académie*, xv, pp. 774-785; *Gazette Médicale*, Nos. 26 and 32; *L'Union Médicale*, No. 102. *British and Foreign Med. Chir. Review*.

Results of Revaccination in the Prussian Army, during 1849.

During the year 1849 there were 51,637 individuals revaccinated, of whom 39,116 had distinct cicatrices of the former vaccination, 8706 had these in an imperfect condition, and 3815 were destitute of them. The vaccination

Pursued a regular course in	30,457
An irregular one in	8467
And failed in	12,713
Succeeded on repetition in	2862

Thus, then, of the 51,637 vaccinations, 33,319 were quite successful; vesicles running a normal course being produced. This proportion, amounting to 64 per cent., is nearly 1 per cent. more than was obtained in 1847 and 1848.

Of the different forms of variolous disease which appeared in the army throughout 1849, only 62 cases occurred, and were distributed as follows:

	In persons not revaccinated.	Revaccinated without success.	Revaccinated with success.	Total.
Varicella,	2	6	5	13
Varioloid,	14	17	9	40
Variola,	6	2	1	9
	—	—	—	—
	22	25	15	62

In almost every case the disease ran a mild course, and frequently was quite insignificant. One case only was fatal. A recruit vaccinated when a child had not been yet revaccinated, and died on the 10th day.—*Med. Zeitung*, 1850, No. 19.

[We have been careful to record abstracts of these annual official returns, testifying as they do most strikingly to the value of the practice of revaccination, through the agency of which smallpox has now for many years been well-nigh extinguished in the immense Prussian standing army. To the greater prevalence of this practice may in part, also, be attributed the much fewer deaths from variola which take place in Berlin as compared with Prussia at large. Thus, while in 1840 1 death in 227·2, was due to smallpox in Berlin, 1 in 173·7 was the proportion for Prussia. In 1845, the numbers were 1 in 4,562 to 1 in 171, and in 1848, 1 in 1022·6 to 1 in 244·7. While the average of 9 years, 1840–8, gave 1 death from smallpox in 289·2 deaths in Berlin, and 1 in 159·7 in the Prussian States.—*See Med. Zeitung*, No 10. *Ibid*.

On Vaccination and Revaccination. By M. CRANINX.

An interesting discussion has lately taken place at the Belgian Academy of Medicine, upon the subject of vaccination. The following were the conclusions of M. Craninx, the reporter, which were affirmed by the academy:—1. Variola and varioloid are but degrees of the same affection. 2. Simple variola may attack the same individual twice, but scarcely ever in rapid succession (*coup sur coup*). 3. It may also attack persons who have been properly vaccinated, but it is then generally mitigated. (M. Lombard observed that the word “generally” must be dwelt upon; for subjects who, to all appearance, have been well vaccinated, at a distant period occasionally die from variola. He added, that in the dreadful epidemic which has just devastated Liege, this was the case, while none of those who underwent revaccination took the disease.) 4. Variola after vaccination, is almost without example within the next ten years; but it is observed from time to time in those who have been vaccinated for more than twenty years. It is, however, very rare after forty. 5. It is of more frequent occurrence, but, at the same time, milder, in the vaccinated, than in persons who have already undergone an attack of it. 6. Varioloid is observed oftener than variola after vaccination, and is not infrequently observed in children; but it increases both in frequency and severity from ten to twenty-five or thirty years. 7. Variola and varioloid in the vaccinated, not following the same course in respect to frequency or intensity as in the non-vaccinated, the cases in which they are seen among the former cannot all be explained upon the supposition of a faulty vaccination, but upon that of a diminution of the preservative action of vaccination. 8. Perhaps we should admit incomplete vacci-

nations possessed of a less degree of preservative power, and capable of becoming sooner exhausted. In this point of view, sufficient importance is not attached to the general reaction which should accompany the vaccine eruption, indicating the action of the virus upon the general economy. 9. If the protective power of vaccination has become enfeebled by time, if not in all, at least in several individuals, there is not sufficient evidence to show that the vaccine, considered in itself, has lost its efficacy since the first years of its discovery. While there is doubt, it is better to revert to the cow-pox whenever the opportunity presents itself. (Upon this resolution M. Seutin remarked that, believing as he did, that the vaccine lymph which existed is efficient, and that it fails either from not having been taken well, or owing to individual susceptibilities, he considered this conclusion would spread needless alarms. M. Lombard observed, however, that the new cow-pox, recently imported from England, certainly exhibited a more certain and more active effect.) 10. As the immunity conferred by vaccination is not indefinitely absolute, revaccination, at least for a great number of individuals, is rationally indicated. 11. Observation shows, that when it succeeds, the second vaccination produces phenomena very nearly like the first, so that we would, *à priori*, anticipate the same effects from it. 12. Experience has determined this point: it has proved that a recent revaccination preserves from variola and varioloid, and that, practised on a sufficient scale, conjointly with vaccination, it constitutes a sure means of arresting the progress of this malady when it appears epidemically. 13. It succeeds best in proportion as it is most required, that is, the more remote the period is since the individual has had variola, or has been vaccinated. 14. If it does not succeed at a first attempt, it should, if necessary, be repeated several times. 15. During the prevalence of an epidemic of variola or varioloid, it is prudent to revaccinate all those whose first vaccination dates ten years back, and all those whose first vaccination gives rise to any doubt. 16. Revaccination may be performed almost indifferently with the lymph of a primary or a secondary vaccination. 17. It is imprudent to inoculate with the lymph of spontaneous varioloid; nevertheless, in the time of an epidemic, if vaccine lymph could not be possibly obtained, we should be authorized in the employment of this fluid, and to transmit it as we do vaccine lymph. 18. If revaccination is so useful a thing, at least for a certain number of persons, vaccination loses none of its importance; and the government and the profession should exert all their influence to enable the entire population to participate in its benefits.—[*Gaz. Méd.*, No. 27. *Ibid.*

Abortive Power of Collodion in Smallpox.

A case has recently occurred in the wards of M. Aran, at the Bon Secours, in which the good effect of collodion was proved to be as decisive in confluent smallpox as it had been found before in the more simple form. It occurred in the person of an unvaccinated young man, and the collodion was applied to all parts of the face but the lips and ears. Through this transparent covering the progress of the pustules was observed to become at once arrested, while those uncovered continued enlarging. Moreover, a part of the covering having been destroyed without being observed for some hours, the pustules thus exposed immediately began to develop themselves until again arrested by a reapplication. The ears, too, were now covered, and the progress of the pustules stopped there. In a few days the collection peeled off, the skin looking as after erysipelas, but no cicatrices were to be observed, though in other parts of the body they existed in abundance, the eruption having been very confluent.—[*Bull. de Thérap.*, vol. xxxix, p. 369. *Ibid.*

Progressive Muscular Atrophy—a new disease.

To Dr. F. A. Aran, of Paris, is due the credit of first calling the attention of the profession to a formidable affection of the muscular system, to which he has applied the name of Progressive Muscular Atrophy. That a disease of such striking features, and so common as to have been observed by the author eleven times in a comparatively short period, should have been overlooked or not accurately studied before, can only be explained by its great resemblance to certain forms of paralysis, and by the tendency of the age to refer to the nervous system all lesions of motion as well as of sensation. We must confess that this has been our error in one case which came under our observation, and which we now recognize very clearly as belonging to the class of affections treated by M. Aran. The case to which we allude was that of a much esteemed professional brother, who, in the prime of life and the possession of a vigorous constitution, perceived that he was gradually losing the power to flex the thumb of one hand. The loss of the use of the thumb having become complete, the finger next to it began to weaken and also became useless; the middle finger followed next, and thus, successively, all the fingers of that hand

became powerless. The loss of voluntary motion invaded the wrist, then the elbow, and finally all the muscles of the shoulder. When it reached the elbow of this limb, the thumb of the other hand began to give way precisely as the first had done, and the disease progressed in this limb as it did in the other, until both arms were left dangling as lifeless appendages to a robust frame. It is worthy of remark, that such was the slow progress of the malady that its ravages were not complete until the lapse of, I think, two years; that during the whole of this time the patient's general health was perfect; that the sensibility of the affected parts was entirely normal; that he suffered no pain; and that the loss of motion regularly coincided with a complete atrophy of the muscles. The limbs and shoulder blades appear completely emaciated and are soft and flabby to the touch. Although about ten years have elapsed since the occurrence of this affliction, he still enjoys fine health and unimpaired mental powers, and is enabled to discharge the duties of an active practice in the country. Sensibility being yet perfect, he judges of the pulse as accurately as ever, when his fingers are placed upon the artery by the assistant who accompanies him.

Our friend was not only treated by ourselves, but also sought the advice of most of the distinguished practitioners of the United States, in vain. He submitted patiently to the trial of every remedy and mode of treatment that had ever been recommended in paralytic affections, without any modification or check of the disease. M. Aran, as will appear below, thinks that galvanism will sometimes arrest its progress; but it proved unavailing in our case.

The very great length of Dr. Aran's monograph precludes it from our pages, but we translate from the *Archives G n rales de M decine* (Oct. 1850) the following corollaries:

“1. The muscular system may be affected with atrophy, perverted nutrition, and probably a cellulo-adipose transformation of the muscular fibre, independently of any lesion of the central or peripheral nervous system and of impaired circulation.”

“2. This atrophy may be partial, affecting a greater or less number of the muscles of the superior extremities, or general,

invading nearly all the muscles of animal life; but, in whatever form it may occur, it is attended with the singular circumstance that, in the affected limb, some of the muscles will retain their perfect integrity in the midst of a large number of others which have been destroyed and transformed."

"3. This affection usually shows itself first in the upper extremities, and is characterized first by weakness and subsequently by emaciation of the parts implicated, by cramps, by subsultus of the tendons, and by fibrillar contractions. The morbid action terminates in the complete destruction of the affected muscles and most probably in the transformation of these into a cellulo-fatty tissue."

"4. This disease occurs sometimes spontaneously, without evident cause, and in other cases follows violent and continued action of certain muscles. It always affects the young, robust and healthy, and never fails to prove, however limited, a fearful infirmity."

"5. Its duration is usually long, its development slow and progressive, and its termination almost invariably the complete destruction of the muscular tissue affected. It rarely remains restricted to the muscles originally invaded, but generally extends to others in the same limb, or shows itself in the same muscles of the opposite limb."

"6. So long as the muscular tissue of the affected muscles has not been completely destroyed or transformed into a cellulo-fatty substance, the muscular fibre will retain its irritability and electric sensibility. This is an important feature, for it may serve to distinguish progressive muscular atrophy from other affections resembling it, especially progressive paralysis untended with mental alienation and paralysis induced by organic or traumatic lesions of the nerves."

"7. When the transformation of the muscular tissue is complete, no treatment will restore its integrity; but before this, the progress of the disease may possibly be checked by the local application of galvanism to the muscles."

From the above, and especially from the first section of M. Aran's corollaries, it will be perceived that the error into which we, as well as others, have fallen, consisted in attributing to the nervous system a lesion which really belongs to the muscular system. Our author has had the opportunity of verifying his positions by post-mortem examinations, and affirms that he has not been able to detect any lesion whatever of the nervous centres, nor of the nerves themselves, in these cases. We are aware that examinations of the kind are rarely satisfactory in

nervous disorders; yet, when taken in connection with the appearance of the muscular tissue and the history of the disease in question, we feel strongly disposed to assent to the pathological inferences of M. Aran. The subject is one eminently worthy of farther investigation.

Collodion in the Treatment of Symblepharon.

The Bulletin Gén. de Thérap. contains an ingenious plan devised by M. Cunier for the relief of adhesions of the eyelid to the eye. The indication being to maintain the lid everted after cutting up the adhesions, sufficiently long to allow the incisions to heal; this is accomplished in the following manner. In adhesions of the lower eyelid, a narrow strip of linen moistened with collodion is applied just below the eyelashes and parallel with them, so as to secure two bits of thread placed transversely beneath the linen with their free extremities hanging down. A similar strip, with threads beneath, is fixed over the malar bone. When entirely dry, the threads from above are tied to those from the cheek, sufficiently tight to depress or evert the lid, and to maintain it so. In adhesions of the upper eyelid the second strip should be placed upon the forehead, for the purpose of drawing up the eyelid when the threads are tied.

M. C. reports several cases in which this plan was entirely successful. In two of the cases the eversion was maintained nine days, and for three weeks after only a few hours each day. In another, six days sufficed. The eye is to be protected from dust by a thick veil or shade.

Remarks on the use of Bromide of Iron. By EDWARD GILLESPIE, M. D., of Brady's Bend, Pa.

Having been practising medicine for some five years past in the neighbourhood of Freeport, in Armstrong county, in this State, where a considerable quantity of BROMINE is manufactured, I have been led to somewhat extensive employment of a ferruginous compound of this article, the results of which have proved so satisfactory, that they may perhaps be not destitute of interest to the readers of the Examiner. The compound which I employ is a bromide of iron of my own preparation, and this is the only form in which I use the

bromine. It has now in my hands entirely taken the place of iodine, appearing to me to meet all the indications for which that medicine has been exhibited, and with much greater efficiency. Indeed, the bromide has become one of my *indispensables* in practice, hardly subordinate in importance to my calomel or opium.

The cases in which I have been in the habit of using it are scrofulous tumours, inflammations of the glands, both acute and chronic, erysipelas, suppression of the menses, tetter, and in most cases where chalybeates are administered. Enlargement of the parotid, submaxillary, or cervical glands, whether from scrofula or other causes, I have never yet failed to discuss, provided they had not progressed to suppuration; and I may speak from an experience of not a few cases, and many of them of long standing. When suppuration does take place, the discharge soon ceases after lancing, and the orifice quickly heals by applying a solution of the bromide over the surface occupied by the tumour, and keeping a small plaster of basilicon ointment on the orifice. My method of using it in cases of tumors is to apply a small portion immediately over the tumour twice a day by means of a feather, and at the same time to administer it internally, commencing with eight or ten drops morning and evening in half a teacup of cold water, increasing a drop or two every day until nausea is produced, (which may be termed the point of toleration.) The dose is then to be reduced five or six drops and continued. This is the invariable rule I lay down for its internal use. I have given it to advantage in cases of felon, where a red and very painful streak would run up the arm, causing inflammation of the axillary glands with general febrile symptoms. Some of these cases present rather an alarming appearance for a time, but have always terminated favorably in my hands by a strict antiphlogistic treatment, and the use of the solution as a local application to the inflamed arm and glands.

Last spring an erysipelatous epidemic raged in a district of the country north-west of this, proving extremely fatal, and causing considerable alarm in the section where it prevailed. It exhibited quite a preference for the fair sex, nine-tenths of those attacked having been females. The first symptoms were chills, followed by a high grade of fever and sore throat; from 24 to 36 hours after the attack, the throat became so much swollen that deglutition was very painful, and in many cases entirely impeded. The breathing was very laborious. The tongue was also much swollen, so much so as in many cases to protrude from the mouth with an extremely thick black coat on the upper surface. In this stage it was generally called

"black tongue." In from 48 to 72 hours from the commencement of the attack, when it pursued its natural course, the ears became red and inflamed, the redness and inflammation extending rapidly over the face, head, neck and shoulders. The eyes became entirely closed, the nose nearly covered by the swollen cheeks, the lips puffed out to an enormous extent, and the head, to use the expression then common, "as big as a bushel." Perhaps an idea may be conveyed of their appearance by likening it to that of persons drowned in warm weather, whose bodies have remained in the water a week or two, with the face and head painted scarlet after being removed. When they arrived at this point they were vulgarly termed "swell heads."

Occasionally the disease would appear on one ear and extend over the face to the median line. When this was the case, one side of the face would be swollen and the other perfectly natural, which gave the patient a very grotesque appearance. It would not remain long in this situation, however, until the sound ear would become red and the disease would soon hasten across the face to meet its companion.

When it did not appear on the skin or could not be brought out by artificial means, the patient generally died in a short time from suffocation. But it could in nearly all cases be brought out by the use of some highly stimulating lotion or the application of a fly blister to the neck. Still the danger was imminent either from sloughing and gangrenous inflammation of the cellular tissue, or a transfer of the disease to the brain. The patient, by external revulsion, was generally enabled to swallow, which of course was a strong point gained, and by the judicious use of internal remedies and proper local applications he now generally recovered. I must not omit to state that I found the free use of the lancet eminently serviceable whenever the state of the pulse required it.

With regard to local applications, I may say that I experimented with a variety of articles. In one case I applied exclusively the acetate of lead in solution—in another the ethereal tincture of iodine, in another strong mercurial ointment, in another solution of corrosive sublimate, and finally the bromide of iron in solution. The progress of these different applications I watched for a couple of days, the internal treatment being the same in all. The acetate of lead appeared to have no influence on the disease whatever. It progressed as rapidly as though nothing were applied, although the solution was as strong as an ounce to a pint and a half of water, and kept constantly applied by means of muslin dipped in the solution.

The tincture of iodine appeared to do better—it retarded but

did not arrest the disease. It was also strong, half an ounce to a pint of ether. The blue ointment I had tried previously in sporadic cases with a certain degree of success, but in this case it appeared to act but feebly.

The solution of corrosive sublimate had no other effect, than to produce violent salivation in four or five days after its application.

The bromide of iron entirely arrested the disease in forty hours from its first application. I used it in all cases afterwards very successfully, by applying it two or three times per day to the parts affected, and extending it one or two inches over the sound skin, and the constant application of cloths wrung out of a solution of acetate of lead.

I have no very accurate formula for the solution of bromide of iron that I make use of. I put three ounces of bromine into ten ounces of water, then add four penny nails, which are about enough to take up all the bromine. If any are left, after the bromine is all taken up, I take them out, and add free bromine as long as it will be absorbed; if too much is added, I put in a nail or two to take up the surplus. It makes a dark liquid of an acid and styptic taste, which turns brick red when applied to the skin. This is the solution which I use.

[The bromide of iron (which is a hydrobromate of the protoxide) is liable, like the iodide, to partial decomposition from the oxygen of the atmosphere, free bromine being generated, and sesquioxide of iron deposited. It should, therefore, for internal exhibition, be protected by saccharine matter. Mr. Dillwyne Parish, of this city, has made, at our suggestion, a solution of the salt, with sugar, after the subjoined formula:—"Take of Bromine *two hundred grains*; Iron Filings, *eighty-five grains*; Distilled Water, *four and a half fluid ounces*; Sugar, *three ounces*. Mix properly. Dose, ten minims, to be repeated and gradually increased, ten minims containing a grain of the bromide."]—*Medical Examiner*.

On Lupus. By M. CAZENAVE.

M. Cazenave rejects the term *lupus exedens* of Rayer, inasmuch as every lupus is *exedens*, this being the essential character of all forms of it. What has given rise to the term is the fact that there are two principal varieties of lupus, the *ulcerated*, and another in which instead of ulceration there is *hypertrophy*; but this latter form is no less destructive, and after a cure, though no solution of continuity has taken place, there is yet loss of substance and a cicatrix. With Bielt, Cazenave admits the form of lupus which destroys the surface, that which pene-

trates deeply, and lupus with hypertrophy,—forms indeed which are sufficiently well known. There is, however another form more rare, hitherto described incompletely and classed improperly, it being the *erythema centrifuga* of Bielt, and since his time placed among the erythemata by all writers, although in fact it is a lupus. It especially attacks young women when apparently in good health, and affects the face in preference, commencing by a point, and enlarging excentrically. It generally exists in the form of red, round patches, of two or three centimetres in diameter, the edges of which are much raised, and the centre depressed; the redness is of different shades, and disappears under pressure. When cured, each patch becomes a depression (the depression of the cicatrix being proportionate to the given hypertrophy of the spot) implying a prior loss of substance, though no breach of surface had existed; and it is on this ground that M. Cazenave terms the diseased *Lupus erythematosus*. The disease is to be treated by sudorifics, as guaiacum, and locally by the tar ointment, or by more or less irritating resolvents, in proportion to the depth of the central depression. One of the best applications is from 1 to 4 of the iodide of mercury to 30 of oil, applied to the part every day or every other day with a pencil. The pain caused by it is great, but the resolution of the tubercle is frequently soon accomplished.—*Gaz. des Hôp*, No. 91. *Med. Chi. Review*.

Operation for Ingrowing Toe-Nail. By M. BAUDENS.

M. Baudens states, that he has performed the following operation more than 200 times without accident of any kind, little pain being caused and that only for a few seconds. The right hand is armed with a narrow, straight bistoury, held like a knife when cutting a pen. The free extremity of the toe is firmly fixed by the thumb and index finger of the left hand, so as to render the diseased part prominent. The operator now carries the edge of his knife (on the outer or inner side of the phalanx according to the situation of the disease, and equidistant from the root of the nail and the next phalanx) perpendicularly down to the bone, and then inclines it towards himself, shaving the phalanx, and carrying off at one stroke the degenerated soft parts, the diseased portion of the nail, and the corresponding portion of the *matrix*,—the removal of this last being indispensable, in order to render the reproduction of the disease impossible. For two or three days he keeps the parts surrounded by ice (which however, is no essential part of the plan, but his mode of treating recent wounds in general,) when granulations spring up, and eventually a horny description, of

cicatrix, forms an excellent substitute for the nail.—*Gaz. des Hôpitaux*, No. 77. *Ibid.*

Enlargement of the Aponeurotic Orifice of the internal Saphena Vein in cases of Varicosed Veins of the Legs.

We condense from the *Bulletin Gén. de Thérap.* the following particulars in relation to this operation, which was performed a few years ago by M. Herapath, of Bristol, but a full report of which was not made. M. Malgaigne has recently performed the same operation, and reports the case as follows :

J. D., aged 26, was admitted into the hospital in July, 1850. The internal saphena vein of the left side was varicosed, and a varicocele also existed upon the same side. Both these affections commenced at the age of seventeen, and had increased to such an extent as to prevent the patient from pursuing his occupation. The internal saphena was varicosed from the internal malleolus to the point at which this vein passes through the aponeurotic orifice into the femoral vein. There were several tumors along the course of the vein, formed by its tortuosities.

The varicocele was quite large, the testicle a little atrophied, and in the engorged epididymis was a tumor the size of a small nut. The operation was performed on the 21st of July. The patient being placed upon his back, the leg flexed and turned a little outwards, an incision was made over the saphena orifice and parallel with the course of the vein. The parts were carefully dissected until the fibrous band of the orifice could be distinctly felt. A grooved director was passed beneath the fibrous band of the ring, and an incision made upwards through the band. Considerable inflammation and a rupture of the vein ensued. The wound, however, was cicatrized the twentieth day. The patient left the hospital seven weeks after the operation, very much relieved, though not entirely cured. The tumors along the course of the vein had very much diminished and the varicocele entirely disappeared.

Orchitis treated by Laudanum externally.

The *Journal des Connaissances* relates the following cases of Orchitis, relieved by the application of laudanum :

CASE I. M. V., 22 years of age, of good constitution and sanguine temperament, during the course of an acute urethritis was attacked with orchitis of the left side. Compresses, moistened with laudanum, were constantly applied to the part. In two days the pains had entirely disappeared, and upon the seventh the testicle had resumed its normal volume.

CASE II. J. M. B., a coachman, 43 years old, of a nervous, sanguine temperament, received a kick upon the right testicle, from which resulted a traumatic orchitis. Compresses, moistened with laudanum, were applied: the pains ceased the next day, and the swelling in six.

CASE III. M. Mar, aged 27 years, of a feeble and lymphatic constitution and bilious temperament, had a double orchitis developed in consequence of an injury. The testicles were very large and very painful. The pains ceased in fourteen hours under the application of laudanum, and upon the fifth day the patient was entirely relieved.

CASE IV. M. S., aged 26 years, of very nervous temperament, was affected with a blenorrhagic urethritis. The same treatment was adopted. The pains disappeared upon the third day, and a radical cure was accomplished upon the seventh.

CASE V. M. F., aged 51; good constitution, nervous temperament, had orchitis of the right side in consequence of an injury. Same treatment: cessation of the pains in two hours, and a radical cure in four days.

Miscellany.

Notices of New Books.—We are indebted to those enterprising publishers, Messrs. Lindsay & Blakiston, (through Jos. A. Carrie & Co., of this city,) for the following works:

Ether and Chloroform: their employment in Surgery, Dentistry, Midwifery, Therapeutics, &c. By J. F. B. FLAGG, M. D., Surgeon Dentist, &c. Philadelphia: Lindsay & Blakiston. 1851. pp. 189.

This little book is written in an agreeable style, and well calculated to produce confidence in the use of anæsthetics, especially of sulphuric Ether, which the author seems to prefer to all others.

The Medical Student's Guide in extracting Teeth; with numerous cases in the Surgical branch of Dentistry, with Illustrations. By S. S. HONOR, Practical Dentist. Philadelphia: Lindsay & Blakiston. 1851. pp. 76.

We are here presented in a convenient form the principal instruments used for extracting teeth, an estimate of their relative value, and directions for using them judiciously.

The Half-Yearly Abstract of the Medical Sciences. By W. H. RANKING, M. D., &c., &c. July to December, 1850. Lindsay & Blakiston's re-print.

This is always a welcome visiter, bringing in a compact form the doings medical of the last six months. The present number is not as complete as its predecessors, owing to the Editor's domestic afflictions.

We doubt not, however, that the work will sustain its well established reputation.

Importance of Latin and Greek to the Physician.—While it would be preposterous to deny the great importance of a classical and refined education to physicians as well as to the other learned professions, and indeed to all men, we must acknowledge our belief that, under existing circumstances, it is impossible to carry out the recommendation of the American Medical Association in relation to the dead languages. The editor of the *Transylvania Journal* indulges in the following sarcasm on the subject :

“It has always appeared to us as a laughable absurdity for the Association to be so gravely emphatic on the subject of ‘the classics,’ while not a tithe of them can master a chapter of *Viri Romæ*, or stumble through a forced version of the first paragraph in the ‘*pons asinorum*.’ That the amount of Latin and Greek which is usually cudgelled into a boy’s brains, through some mysterious route beginning at the posteriors, may be of no vital disadvantage to him when he has tossed his *Virgil* to the jakes, shaken his fist under the pedagogue’s nose, and turned medical student, we cheerfully admit, with a proud reference to the small stock of Latin maxims which we have preserved from the ruins of a ‘classical education,’ and which we refresh whenever our pedantic vein approaches, by reference to a dictionary of quotations.

“We commenced the study of the profession with a full faith in the extent and profundity of our classical acquirements, and, possibly, might have passed muster before a committee of the National Association ; but we found to our consternation that *Tityrus* and *Melibeus* were asses in the matter of pukes and purges, and, alas ! not a word of squirts and clysters, or of medical reform, was to be found from one end of *Tooke’s Pantheon* to the other, and the naked gods and goddesses were utterly worthless even for the purposes of the dissecting room.

“We shall insist, whenever the Greek and Latin clause is brought before the Association again, upon the members being put through their moods and tenses, and a parsing ‘spell.’

“Gentlemen may therefore anticipate an opportunity of displaying the tattered remnants of schoolboy erudition ; and we would advise every one to stuff the old, and much sworn-at grammar in his port-manteau, and to go through a course of sprouts with some strong armed disciplinarian, in order to revive those early and fresh ‘classic’ memories which are so closely allied to the penitential visitations of the much abused seat of honor.”

Diet Drink.—In the spirit of our motto, we do not hesitate to “adopt what is good wherever we may find it.” We therefore present our readers the subjoined recipe, which has been extensively used by the

unprofessional in this city, and in many instances with the most signal advantage. It is said to have been first introduced by some of the emigrants from the French West Indies many years ago. The profession will recognize in it the principal features of the Lisbon Diet Drink and others of the class. We will not stop to canvass the heterogeneousness of the compound nor the apparent incompatibility of the ingredients. It is especially recommended in scrofulous and syphilitic affections of a chronic character, and also in chronic rheumatism and old ulcers; the calomel being omitted in cases attended with no syphilitic taint. It is certainly far preferable to the various nostrums sold at exorbitant prices as "syrup of sarsaparilla," and which for the most part contain not a grain of sarsaparilla.

Sarsaparilla, cut up and bruised	} of each	-	-	12 oz.
Sassafras root, cut up,				
Guaiacum shavings,				
Mezereon,				
				½ oz.

Pour four gallons of boiling water upon the above ingredients, cover the vessel, let it stand 24 hours, and then add

Carbonate of Potash,	} of each	-	-	6 drachms
Pulvd. Crude Antimony,				
Alum,				
				1 drachm.

The two last articles to be tied up separately in a bit of old linen—then boil until reduced to one gallon. About 15 minutes before taking off the fire, add

Coriander seed,	-	-	-	6 drachms,
Senna,				
Calomel, (tied up in linen,)				
				½ oz.
				1 drachm.

When the tea is perfectly cold, strain through a cloth, bottle, and keep in a cool place. Of this the patient should take ½ pint an hour before each meal. It may be sweetened if desired with sugar-house syrup. The patient should take it at least 6 weeks—and the tea be made fresh whenever the supply is exhausted.

During this treatment the patient should avoid salted provisions of all kinds, spices, and all irritating articles of food or drink; and should also avoid exposure to cold or wet weather. The best diet is plain bread and fresh meats of any kind, save pork.

Physicians in Erie County.—It seems that in Erie County, (Pa.) there are 79 practitioners of medicine. "Of that number, twenty are members of the county medical society. Six are graduates of respectable schools, who are not members. Ten who are neither graduates nor licentiates are considered respectable practitioners, as they

practise the profession to the best of their ability, in accordance with the principles of the orthodox system, without resorting to any of the devices of quackery to obtain business. Thirteen who profess to belong to the regular system are, both by their education and practice, the veriest quacks in the country. Four who practise Homœopathy, Allopathy, or anything else by which they can obtain a share of the 'loaves and fishes.' Two 'Simon pure' Homœopathists. Four Uroscopists. Three females, who are without any education, but who pretend to practise the various departments of the profession. Five 'eclectic or reformed practitioners.' Ten 'Botanic, Thomsonian, or Herb Doctors.' " *Vive la Bagatelle!*

The Ravages of Consumption and other leading Diseases in Boston for five years.—Mr. Simonds, the City Registrar, states that for five years ending 1850, 250 persons have been accidentally killed in Boston, 116 have died of apoplexy, 1,484 of disease of bowels, 261 of disease of bladder, 156 of congestion of brain, 2,838 of consumption, 461 of convulsions, 449 of croup, 101 of cancer, 612 of cholera, 286 of cholera infantum, 148 of canker, 267 of childbed diseases, 310 of diarrhœa, 965 of dysentery, 325 of dropsy, 675 of dropsy on the brain, 137 drowned, 49 delirium tremens, 166 erysipelas, 728 scarlet fever, 738 lung fever, 1,237 typhus fever, 108 brain fever, 224 whooping cough, 296 disease of heart, 103 intemperance, 1,302 infantile diseases, 113 liver diseases, 434 inflammation lungs, 395 of marasmus, 465 measles, 389 old age, 116 pleurisy, 146 palsy, 17 rheumatism, 43 disease of spine, 57 scrofula, 59 suicide, 349 small-pox, 463 teething, 74 tumor, 28 ulcers, and 36 worms.

Mortality of Boston in 1850.—From the Report of the City Registrar for 1850, it appears that the total number of deaths for that year was 3667, being about 1 in 38 inhabitants. 586 died of consumption alone.

Mortality of Lowell in 1850.—The number of deaths in this city during the last year was 492.

Davey on the Ganglionic Nerves.—Having presented our readers the views of Marshall Hall on the cerebro-spinal system, we are happy to have it in our power to offer them those of Mr. Davey in relation to the Ganglionic Nerves. They constitute a sort of complement to the studies of Hall, and will doubtless be read with much interest. Mr. D. is evidently a profound thinker, reasons well, and conveys his ideas lucidly. The study of the nervous system is so important and so attractive that we hope to be pardoned for giving to it so many of our pages.

American Medical Association.—We are requested by the Editors of the Charleston Medical Journal to state that the American Medical Association will convene at Charleston on the *first* Tuesday of May next, instead of the *second* Tuesday, as was erroneously announced in their advertisement.

Medical Society of the State of Georgia.—The annual meeting of this Society will be held at Atlanta, on the second Wednesday of this month (April). Dr. Arnold, of Savannah, was appointed to deliver the Address—and Dr. Le Conte, of Macon, the alternate. The well merited reputation of these gentlemen will secure a literary as well as a professional treat to those who may attend. The facility with which Atlanta may be reached from a great portion of the State will enable a very large number to assemble.

Medical College of Georgia.—The course of Lectures in this Institution was closed on the last day of February, after a session of four months, during which there occurred not the slightest incident to mar the good feeling which prevailed between the Faculty and Students, nor to lessen the high esteem in which the class has ever been held by the community. There were in attendance 159 gentlemen, of whom 127 were from Georgia, 13 from Alabama, 12 from South Carolina, 2 from Mississippi, 1 from Ohio, 1 from Tennessee, 1 from North Carolina, 1 from Kentucky, and 1 from Arkansas.

Fifty members of the class, having complied with all the requisitions of the College, were graduated on the 4th day of March. The Doctorate having been conferred by Ex-Governor Schley, a very appropriate, chaste and creditable address was delivered by Dr. C. T. Quintard, of Cobb county, and a valedictory full of warm-hearted and touching sentiments by Dr. R. E. J. Thompson of Burke county. The following is a list of the Graduates.

J. P. McCord,	Columbia County,	Georgia.
James T. Reid,	Union “	“
Rodney Burke,	Burke “	“
Etheldred E. Jones,	Early “	“
Robert H. Oakman,	Augusta,	“
John G. Mathis,	Burke “	“
Wm. Johnson,	De Kalb “	“
Isaac R. Eskew,	Pike “	“
A. P. Allday,	Lumpkin “	“
F. R. Bell,	Oglethorpe “	“

A. L. Patten,	Floyd County,	Georgia.
Wm. A. J. Anderson,	Cobb “	“
R. W. Oliver,	Scriven “	“
J. T. Groves,	Cobb “	“
Wm. S. Harden,	Savannah,	“
Green H. Hunter,	Warren “	“
J. C. Buchanan,	Talbot “	“
R. E. J. Thompson,	Burke “	“
Isaac B. Hall,	Baker “	“
D’Coucy Antony,	Burke “	“
Jas. M. Young,	Cobb “	“
Sterling B. Simmons,	Augusta,	“
Wm. A. Roll,	Augusta,	“
Hezekiah Witcher,	Meriwether “	“
Jno. G. Williams,	Greene “	“
R. A. Dickinson,	Baker “	“
N. F. Howard,	Lumpkin “	“
M. P. Deadwyler,	Madison “	“
M. Spencer Durham,	Oglethorpe, “	“
Jno. L. Hughes,	Floyd “	“
Benj. F. Bentley,	Lincoln “	“
Sterling Gibson,	Warren “	“
John J. Cooper,	do. “	“
H. J. Galt,	Cherokee “	“
Wm. M. Standifer,	Early “	“
F. W. Schley,	Augusta,	“
Berthier B. Bailey,	Columbia “	“
A. A. Jernigan,	Greene “	“
Z. P. Landrum,	Lexington, “	“
Wm. P. Ragland,	Meriwether “	“
Wm. C. Sheridan,	Talbot “	“
Samuel Boyd,	Washington “	“
Wm. L. Miligan,	Dale “	Alabama.
R. P. H. Heacock,	Taledaga “	“
W. L. Heflin,	Randolph “	“
Jas. I. Lamar,	Autauga “	“
Oliver P. Mangum,	Henry “	“
Jas. H. Latimer,	Laurens District,	So. Carolina.
Wm. T. West,	Edgefield “	“
Thos. J. Young,	Monroe County,	Tenn.

SOUTHERN MEDICAL AND SURGICAL JOURNAL.

Vol. 7.]

NEW SERIES.—MAY, 1851.

[No. 5.]

PART FIRST.

Original Communications.

ARTICLE XIV.

Scarlet Fever—its Symptoms, Consequences, Causes, and Treatment. By H. A. RAMSAY, M. D., of Calhoun, Geo.

The views promulged in this article are derived from facts personally observed in more than four hundred cases of the disease in question. We claim not to be an innovator, but we boldly aver that much error exists in the therapeutic management of scarlet fever, attributable, in all probability, to a misconception of its pathology. It is quite probable many may take issue with our views and inductions; to such, we say, try our suggestions, and if your expectations and my promises are not verified, after a fair and unbiased trial, condemn the practice and repudiate the positions assumed. The term scarlatina which is the technical word for scarlet fever, elicited at one period a long and spirited controversy among medical men; many repudiated it as unmeaning, unclassical and vulgar; among them stand the names of J. Mason Good, Heberden, Huxham, and De Haen, while Willan and Swediaur, equally learned, retained it. This war of words, in reference to terms, had the good effect of drawing the diagnostic lines of scarlatina to a focal point, and ere it ended, it would have been derogatory to any physician to have considered it, as formerly, synonymous with measles. At this day usage and common consent have affixed to the disease the technical term Scarlatina. The history of scarlet fever is interesting alone from its obscurity. There is said to be some smattering allusion to it in

the writings of Paulus Egina, Avicenna, and Rhazes, but as it was common in those days to confound measles with small-pox, and scarlet fever, doubtless, with measles, it is extremely dubious to what they alluded in the vague passages referred to, to establish this point. The probability is strong that scarlet fever was transported to Europe from Africa. The first epidemic we know of was in Spain in 1610, and it raged with violence in Naples in 1618. In 1685, it began its ravages in London, and in 1735, it made its way to America,—here its history is sufficiently known to merit no notice from our hands. As previously remarked, there is great obscurity, hovering over the history of scarlet fever. We have already stated our surmises in reference to it. Some now suppose that Ingrassias wrote an article upon it in 1556; there is no kind of evidence to sustain this presumption, and the most plausible fact as to a description of the disease, is, that Prosper Martianus, an Italian physician, wrote an account of its prevalence in Rome, about the middle of the seventeenth century. We will not pursue these historic details farther; they are of no practical utility, and we will dismiss this part of the subject.

Symptoms.—Most writers upon this affection divide it into, *Scarlatina simplex*—*S. anginosa*, *S. maligna*. Here we join issue with the profession, and denounce the division as non-existing, unmeaning, and unpractical. There is no such division founded upon any facts in the Symptomatology of the disease. From whence then is it derived? We challenge the whole mass of the profession from Maine to Texas, to exhibit a case of scarlet fever free from anginose ailment in some form or other. No man has ever seen such a case, it is a non-descript, in scarlet fever. This name is one of the many errors of modern conventionalism, which is an odium to medicine, and a disgrace to science, working out no practical good, but achieving much therapeutic injury. *Anginose Scarlet fever, a distinct form of the disease!* oh! fie! A leopard may as well undertake to change his spots, as scarlet fever to be exempt from sore throat. And we aver it, frankly and without the fear of successful contradiction, that no man in the South ever saw, or ever will see, a case of scarlet fever with an

entire freedom from anginose disease in some form. This symptom is purely one of the diagnostic signs, and we have no idea the disease ever existed independent of it, and we envy no man his discrimination, who would pronounce a case scarlatinous, in the absence of this symptom. We have a division of the disease, we greatly prefer to any we have seen, but we do not claim for it, entire immunity from error, but it is certainly more applicable and expressive of the various phases of scarlet fever, than the old division of varieties. We present it and act upon it—the reader is free to reject or adopt it, in consonance with his better judgment and tastes. Here it is:

SCARLATINA SIMPLEX.

“ GRAVIS.

“ GRAVIOR.

Scarlatina Simplex.—This is decidedly the mildest form of Scarlet fever, it rarely confines the patient to bed, indeed the symptoms are now and then so simple, that it passes off virtually incognito. The patient complains of a little weariness and is stretchy, slight nausea sometimes, pain about the head, slight redness of the eyes, a gentle increase of pulse, with a burning of the surface and a little soreness of throat; the tongue is furred and covered with red dots; in a day or two, often the first twenty four hours, the face, breast and arms, are covered with a scarlet rash, which is commonly in a short time diffused over the surface. The case passes along for three or four days, and the pulse softens, the rash disappears, the skin begins to peel off, and the patient is well, unless some of the sequelæ should come up, or some other issue arise. The patient does not even retire to bed in many of these cases, but is up about the house, and attending to usual avocations. The most complained of is usually the throat, it is red inside, swelled, and swallowing rather difficult.

Scarlatina Gravis.—This is an exacerbated form of the preceding, accompanied with greater intensity of symptoms, and more retardation of the rash. The patient has chilliness, languor, pain in the head and eyes, pain in the jaws, sore throat, impeded respiration, and difficult deglutition; the fauces and

tonsils are swollen, the tongue furred, some nausea, the pulse quick and accelerated, skin hot and burning ; about the fourth day a scarlet eruption appears upon the face, chest and arms, which soon diffuses itself, intercurrently or distinctly. These symptoms continue for several days, with excited pulse ; thirst, pain in the head, slight delirium at night in some cases, highly colored urine, swelled throat, externally and internally ; the bowels loose or constipated and appetite perverted—when the pulse will probably begin to decline in velocity, if the case is favorable, the skin becomes soft, desquamation ensues, the rash gradually disappearing, and all the functions are restored to their former balance. This form may, however, run into the graver form of the affection, assume all its obstinancy and protean appearances, and confine the sufferer to bed for many days, or even weeks. The patient is commonly confined to the house with this variety ; prudence demands that he should keep in doors, as well as necessity. There is generally a discharge of tough, viscid phlegm in *S. gravis*, which is painful to discharge and hard to extricate from its lodging place ; the nose also discharges a yellowish fluid, which sometimes corrodes the nostrils, and not unfrequently the nostrils discharge blood in the evening paroxysms. While an examination of the throat will exhibit the fauces and tonsils covered with whitish yellowish patches, and also the rash, peculiar to the surface, will be found inhabiting the palate, cheeks (inside), lips, &c.

During the mornings, the paroxysm of fever partially lessens, but in no case have we ever seen a complete remission of fever in this disease, during its exanthematous existence.

Scarlatina Gravior.—This is the most protean form of Scarlet fever ; there is a positive increase of all the symptoms incident to the form just described, with the addition of a considerable typhoid tendency. The throat is sore, the glands swollen, sometimes even with the chin ; the tonsils and fauces covered with dark filthy looking and apparently gangrenous patches ; the breath offensive ; the skin hot and burning ; pulse rapid, often from 120 to 160 ; the efflorescence upon the skin is evanescent, changing from time to time in color and quantity ; the system is prostrate ; hands tremulous ; breathing oppressed ; swallowing quite difficult, and almost impossible ; the skin looks

purplish in some cases, from a retrocession of the rash; the tongue is dark and fissured; the teeth and gums covered, in many cases, with sordes. The patient may be delirious or comatose, oftenest the latter; the cheeks of a dusky red color, the eyes suffused with a mucous like film, the features are often swollen and distorted, the throat will frequently discharge extensive sloughs, the nose will discharge, the ears run offensively, the eyes run and then bleed from the corners; the skin will become echymosed and assume a purpura hemorrhagica appearance, the bowels may take on a diarrhœa, the lungs become inflamed, the brain hyperæmic or softened; in short there is no accounting for what will supervene in every case of scarlatina gravior. We have now given, as briefly as possible, all the most prominent signs of the various grades of scarlet fever: we may have omitted some few general or unimportant symptoms—our limits would not suffice for a minute treatise; but we have endeavored to record all the usual signs of diagnostic importance. The sequel of scarlet fever is attended in a large number of instances with many unpleasant consequences. Probably no affection in the whole catalogue of diseases has so many sequelæ attached to it. Deafness will follow it in all its horrors; neuralgia, with its painful traces, is frequently a sequel; swelling of the glands of the throat and suppuration follow in their train, chronic disease of the bowels is a very common visitor at the shrine, while dropsy occurs in a greater number of cases than all combined; its usual form is anasarca, but by no means invariable, as we have seen ascites and hydrothorax in many instances. There are many anomalous sequences, which will meet the eye of the physician, while it is not necessary to advert to, but when seen, can be easily attributed to a motive principle.

Causes.—The causes of scarlet fever, like its history, are buried in an obscure latitude, and are probably beyond the reach of legitimate investigation. We have our views upon this subject, but they may be erroneous. We believe scarlet fever is dependent upon some morbid, atmospheric principle, which, when taken into the system, expends its poisonous influence upon the glandular and sanguiferous systems, inducing

those glandular enlargements and exanthematous eruptions, which we observe in scarlet fever. These are my crude notions of scarlet fever. We may err. Space will not permit us to discuss the question. If any man doubts our position, let him refute it, or show a more plausible one. Scarlet fever is a disease mostly incident to children; they are not exclusively, however, the victims of attack: we have often seen it in adult life. The disease is common to winter and spring, but is not strictly confined to any season. The worst epidemic we ever witnessed, was in the summer months. The mortality of scarlet fever varies very considerably. We remember going through three epidemics: in the first we lost no case; in upwards of 100, in the second, we lost three; in about 208, in the third, we lost none. In several epidemics, which have passed under the observation of other physicians, whom we knew to be men of prudence, the mortality was immense. The mortality, we think, is mainly attributable to the remedial management. Of course some cases will die of any malady, but many might be preserved by careful discrimination, and a just application of pathological facts. The child bears scarlatina tolerably well—the adult remarkably well. The mildest cases are surest to have the worst sequelæ, as a general rule. A previous attack of scarlet fever ensures a security against future attacks; we have seen a single exception. We would also remark, an attack of scarlatina affords a shield from measles, to some extent, but not vice versa.

The contagiousness of scarlet fever is a mooted point. We are an advocate of the doctrine, although our credulity has been staggered. In 1846 we had it, but no other member of our family, consisting of several, among them, two or three children. We saw another patient have it in a family of fifty or more in number; no one else in the family had it. In our own case, we feel certain we contracted it from a patient we had, although we had passed through an epidemic professionally, and (one before we entered the profession) prior to 1846, intact. The second case, in the family of fifty, we feel confident imbibed the disease from a patient we had near by the family residence. There are many conflicting arguments and facts, pro and con, as to the contagious character of scarlet fever, but we

believe the mass of evidence will preponderate in the affirmative. Scarlatina may prevail sporadically or epidemically; the latter phase is the ordinary appearance it assumes among us. Now it is said, by some writers, that scarlet fever ever has an eruption. This we conceive a mistake; it not unfrequently happens that cases occur without the semblance of an eruption, and with but little efflorescence of the surface. It is true, these cases are rare, but they have doubtless been seen by all men of experience; but we venture again to assert, that no man ever saw a case of scarlet fever exempt from disease of the throat to some extent. To assert the adverse is a fallacy, and not to be received only from the highest authority, and we should question it as a designed subterfuge to gratify some vain presumption, even from that source.

Having now briefly exemplified the causes, and some of the collateral circumstances attendant upon scarlet fever, we will descend to the treatment, with the remark, it has been our primary object, in this paper, to avoid discussion, to pass by the relative opinions of others, and to give our own views and experience in our own style and manner, perfectly regardless of criticism and entirely careless of the opinion of others. We believe our opinions are as near correct, yea, nearer, than any heretofore promulgated: we believe they are not mere vagaries, insusceptible of practical demonstration, but we cordially think they only have to be adopted as a whole to be verified.

Treatment.—Here is the rock upon which we shall probably split with the profession.

The practice of giving calomel, tartar-emetic, bloodletting and cold affusions, to the profession, as remedies in scarlet fever, have produced effects as disastrous as war and famine ever did in some countries, if we consider the number of subjects involved. We have already said the glandular system was largely involved: we apprehend no sane man doubts it. Where, then, is the necessity of giving a remedy to excite it? Calomel certainly expends its influence in a great measure upon the glandular system; scarlet fever being partially a disease of that system, would only be aggravated by the remedy. We never saw a single instance in which scarlet fever was ever benefited

by the mercurial practice, and we do not believe it a safe, prudent, or even an applicable practice, in general. The tartar-emetic practice is equally pernicious—it prostrates, nauseates, produces intestinal disease, and the subject rarely recovers from its effect. The practice of bloodletting is still worse—it reduces the pulse to some inconsiderable degree, deprives the patient of an essential element of cure or at least an adjuvant—*strength*; in short, it does no good. The orifices rarely heal, they inflame, and sometimes become gangrenous; indeed, in none of those morbid affections does bleeding do good, as a common rule. Blisters have a detrimental effect—they aggravate the case, never or rarely get well, the tissues around them are apt to disorganise, and we never saw a case blistered that recovered. No kind of harsh or drastic treatment will go in scarlet fever, it has in every form a debilitated tendency. To *bleed, purge, blister and calomelize, is only to kill*. We are not opposed to mild treatment: we believe it essential, and the only plan of cure; but never add fuel to an already excited and destructive fire, if you wish to suspend its progress. The true plan of treating scarlet fever is the *mild emetic plan*. The puke should be made of common table salt dissolved in warm water; the patient should drink it, and freely, until copious emesis is produced; it relieves the throat of all clogging that may exist, it removes local congestions about the bronchial tubes and their appendages, it never sickens the stomach, and consequently never prostrates the patient; indeed, it may be justly denominated an emeto tonic. The puke should be given at any time when there is difficulty of swallowing, or much enlargement about the tonsils, or much phlegm in the throat; it is superior to all gargles, and probably has some solvent effect about the secretions in the throat. The salt water emetic is never harsh in its effect, and it seems to buoy up the patient, and apparently exercises as much influence in controlling the morbid influence of scarlet fever, as the common salt does in saving meat from putrescency. We have given it, and that indiscriminately, in this affection—we know, indeed, of no countervailing circumstances—and we have even done it with the happiest and best effect. The confidence we have in the potency of salt and water, in scarlet fever, is derived alone from experience, not mere theory; we are

willing to risk our reputation upon the principles of practice we shall here lay down, and we challenge a fair and impartial trial of our deductions. But, says one, would you puke a child who has delirium, or is comatose? We answer, emphatically, *puke it*. The delirium is not inflammation, but simply a mobility of the nervous influence expended upon the brain and emesis will have more effect in restoring it than any thing else, for it is primarily dependent upon the morbid scarlatinous influence, and not a primary affection. In the event the salt is not prescribed, we prefer next the ipecac, combined with mustard, or alone; but when we can obtain the first, we always use it. The emetic practice is the magnum remedium in scarlet fever; it is the anchor sheet of safety; without its adoption, no man can treat it successfully. Simplicity is a paramount principle in the treatment of Scarlatina; it is the opprobrium of the profession, that too many remedies have been used in this malady—they have only increased its fatality, and disgraced the science. Next to emesis, after a due trial of it, and the throat continues to be clogged, we should carefully inspect it, and if we find it swollen internally *free scarification* should be resorted to. We are at a loss to know how any man can go through his cases without resorting to scarification. We have often rescued the little sufferers from an impending death, by this simple remedy, much to the disappointment, and relief of the weeping mother and disconsolate father. But while the abstraction of the matter from the throat and scarification is a balm, we again warn our brethren to avoid the general use of the lancet, not only as pregnant with evil, but *dangerous*. After scarification, and even before, the throat should be well gargled with warm water and vinegar, or a sauce composed of flaxseed, pepper, salt, and vinegar, the patient cannot gargle too often. A great variety of gargles have been suggested, but those we have named are about as effectual as any. After scarification, sometimes there is a considerable effusion about the throat, which even threatens suffocation, the best remedy is emesis, or the blowing of powdered alum in the throat, we have sometimes used a solution of Kreosote and Nitrate Silver, but never with the same success as the alum, and emetic. A most useful and not to be dispensed with adjuvant in the treatment

of scarlet fever is the application of poultices to the throat externally, they should be worn constantly, and should be made of corn meal and cayenne pepper, and kept warm; they induce a distension of the capillary system, thereby removing the local congestions about the throat. The common onion makes a most excellent poultice, so also the rind of the bacon. The bowels of the patient should be kept open with common *epsom salts*, *seidlitz*, *magnesia* or *enemas*: an evacuation should be secured about every other day.

These are the general outlines of the treatment of scarlet fever, we have ever adopted with distinguished success, and we recommend it to our brethren with every confidence of equal success in their hands. The scarlet fever patient should ever be kept warm; his drink warm, not hot; he should avoid cold drinks, and even should sponging be requisite, it should be tepid. After convalescence, he should remain in doors for fourteen or twenty-one days; for under that period he is not free of the contagious principle, and prudence, to avoid those unpleasant sequelæ, demands confinement to home, and an avoidance of exposure. If those sequelæ incident to the disease should arise, they must be treated upon general principles; and if the case should be combined with pleuritis, pneumonia, enteritis, or any other affection, we leave the practitioner free to select his own means—for we are writing upon scarlet fever alone. There is one of the sequelæ of scarlet fever which we will advert to, and we hope with propriety—we mean Dropsy. We have never failed to cure a case of ascites or anasarca following scarlet fever, with the common *sweet shrub* tea, given daily and freely, with an occasional dose of Epsom salts. We use the root: we give it as a common drink, and every other day give a dose of salts; and we have never failed to cure a case. We learned the remedy from Dr. Thomas F. Gibbs, now of Augusta, who is a talented and practical man; he has used it for the last fourth of a century or longer, and is without question, the first physician in Georgia who ever prescribed it.

We have now accomplished our task: we have not, it is true, extended our therapeutic views as far as common upon this disease; we did not deem it necessary, but decidedly unes-

sential. We now think, as before remarked much of the fatality of scarlet fever depends upon the bad treatments adopted, consisting of *calomel*, *tartar*, *bleeding*, *blisters*, &c. We repudiate them and challenge a showing of comparative results. We are a mercurialist justly adopted, we glory in being one, but at the same time we are an eclectic, and stand upon the impregnable altar of philosophic induction and practical facts. Having omitted it in the proper place, we will remark here, that warm mustard pediluvia are good in scarlet fever, and in two instances we derived the happiest results in scarlatina gravior from a bath of sal eratus. We suggest it to our readers, as worthy of trial. The various forms of scarlet fever, are all treated upon the same principles, hence we have not confused the reader with an unnecessary and unscientific distinction of treatment. We will now present tabular views of 60 cases and leave the reader to draw his own deductions.

1. *Proportion of Cases in Adults and Children.*

Adults	have	Scarla.	Simplex,	about	1	in	60
"	"	"	Gravis,	"	1	in	5
"	"	"	Gravior,	"	1	in	60
Children	"	"	Simplex,	"	1	in	4
"	"	"	Gravis,	"	1	in	2
"	"	"	Gravior,	"	1	in	14

2. *The Rash in Scarlatina Simplex appeared as follows :*

	In	11	cases,	on	the	1st	day.
	"	4	"	"	"	2d	day.
Sequelæ	in	8	cases	was		nothing.	
"	"	1	"	"		Deafness.	
"	"	2	"	"		Dropsy.	
"	"	1	"	"		Sore eyes.	
"	"	1	"	"		Glandular.	
"	"	1	"	"		Diarrhœa.	
"	"	1	"	"		Ophthalmia.	

3. *The Rash in Scarlatina Gravis appeared as follows :*

In	14	cases	on	the	3d	day.
"	6	"	"	"	4th	"
"	13	"	"	"	2d	"
"	2	"	"	"	1st	"
"	4	"	"	"	5th	"
"	1	"	"	"	6th	"

4. *The Sequelæ of Scarlatina Gravis are as follows :*

14	cases	were	Dropsical.
5	"	"	Glandular enlargements.
1	"	"	Deafness.
2	"	"	Ophthalmia.
14	"	"	Nothing.
1	"	"	Sore ears.
1	"	"	Rheumatism.
1	"	"	Neuralgia.
1	"	"	Œdema.

40

5. *The Rash in Scarlatina Gravior appeared as follows :*

In 1 case on 5th day. | In 3 cases on 4th day. | In 1 case on 7th day.

6. *The Sequelæ of Scarlatina Gravior were as follows :*

In 3 cases Dropsy. | In 1 case Deafness. | In 1 case Neuralgia.

From the above table, it will be seen that the sequelæ of scarlet fever appear in the following proportionate results :

7.	Dropsy	occurred	20	times,	or	1	in	3
	Deafness	"	3	"	"	1	in	20
	Ophthalmia	"	4	"	"	1	in	15
	Glandular disease	"	6	"	"	1	in	10
	Neuralgia	"	2	"	"	1	in	30
	Rheumatism	"	1	"	"	1	in	60
	Diarrhœa	"	1	"	"	1	in	60
	Œdema	"	1	"	"	1	in	60

8. *The mean time of the appearance of the Rash was as follows :*

In	13	cases	on	the	1st	day.
"	17	"	"	"	2nd	day.
"	14	"	"	"	3rd	day.
"	9	"	"	"	4th	day.
"	5	"	"	"	5th	day.
"	1	"	"	"	6th	day.
"	1	"	"	"	7th	day.

60

This table is not free from error ; in all probability we may have made some miscalculations in estimating the relative statistical facts as to the comparative frequency of the various sequelæ, which the reader will easily correct. It is no small task, to cast up and prepare a statistical table, and make it correct in every word and line, and particularly when one is fretted and called away to attend to his professional calls. The reader, we hope, will render us all proper indulgence for these

inconveniences. We know, farther, the table is incorrect and unreliable in another point of view : the number of cases is not sufficient to establish a correct basis, but we hope other physicians will continue the record, and pile Pelion upon Ossa, until we shall have a full and replete mass of reliable evidence in regard to this interesting disease.

We have now done with the subject, and if our article will be the means of snatching a single individual from the grave, we are amply paid ; and we sincerely trust, if our views and suggestions are carried out, but few will die from the disease.

We will now recapitulate our plan, and leave the subject, for farther conclusions, in the hands of the fraternity.

1st. Puke the patient, through the whole course of the disease, with salt and water, or ipecac. ; never with tartar.

2d. If the tonsils and fauces are much swollen, scarify them freely, and promote the discharge by gargles of warm water.

3d. Keep warm poultices of corn meal and pepper to the throat ; keep his throat gargled all the while with vinegar and water, warm ; if there is much exudation of lymph about the throat, powdered alum should be blown in.

4th. The bowels should be kept mildly open every alternate day, or longer, if not essential, with salts, magnesia, seidlitz, rhubarb, or enemas.

5th. A warm pediluvium should be used every night, and the patient should drink warm sage, balm, or other tea, instead of cold water.

These general regulations, of course, may need some modification in some instances : the judicious physician can, with facility, determine this, and adopt the proper premises. Let the motto be, avoid *drastric treatment* ; too much medication is officious and hurtful.

Surgical Cases—treated by Prof. DUGAS—Reported by HENRY ROSSIGNOL, M. D., of Augusta, Geo.

Having had the opportunity of seeing most of the practice of Dr. Dugas, Professor of Surgery in the Medical College of Georgia, and having free access to his notes, I beg leave to furnish, very briefly, the history of some of the most interest-

ing of the following list of cases, treated during the recent College session, most of which were seen by the Class:

Nov. 2. Diffused abscess of the hand—opened.	Dec. 20. Fracture of radius.
" 3. Caries of tibia—excision.	" 24. White swelling of knee.
" 6. Fracture of radius.	" 27. Extensive syphilitic ulcers of body.
" 7. Lumbar abscess—opened.	" 28. Inflammation of popliteal lymphatics.
" 8. Crushed foot	" 29. Acute ophthalmia.
" 12. Abscess of neck—opened.	Jan. 3. Burn of foot—amputation.
" " Fistulous ulcers at elbow.	" " Pin in œsophagus—removal.
" 13. Carcinoma of knee—amputation.	" 6. Scalp-wound—diffused erysipelas—incisions.
" " Carcinoma of face.	" 7. Ulcer of Knee—amputation.
" " Inveterate incontinence of urine.	" " Lypoma of scalp—excision.
" 14. Large abscess of face.	" " Fibrous tumor of mamma, do.
" 15. Enlarged prostate.	" 23. Nasal polypus—operation.
" 16. Gunblepharon—operation.	Feb 1. Pterygium—operation.
" 17. Gunshot wound of face.	" 6. Double club-foot, do.
" 18. Syphilitic ulcer of leg.	" 7. Nasal Polypus, do.
" 20. Cataract—couching.	" " Stricture—opened perineum.
" 28. Sprained ankle.	" 8. Enlarged tonsils—operation.
" 29. Rigidity of muscles—section.	" 10. Tumor of eye-lid, do.
" " Indolent tumor of arm.	" 12. Urinary calculus—lithotrixy.
" 30. Chronic ophthalmia.	" " Strabismus—operation.
Dec. 4. Fall—contusions.	" 13. Nasal polypus, do. 2d time.
" 6. Nasal polypus—operation.	" 17. Enlarged tonsils, do.
" 10. Otorrhœa—deafness.	" 22. do. do. do.
" 11. Burn of foot.	" 26. Bones lodged in the rectum.
" 12. Urinary infiltration—stricture.	" 27. Nasal polypus—operation 3d time.
" 14. Severe concussion of brain.	" " Clubfoot—operation.
" 18. Burn of foot—amputation.	" 28. Enlarged tonsils, do.
" 19. Cataract—couching.	
" 20. Cancer of tongue.	

CASE I. *Encephaloid Carcinoma—Amputation of Thigh.*—

John, a mulatto, about 15 years of age, the property of Mrs. A., of Taliaferro Co., had been suffering six or eight months with a tumor just above the knee, which continued increasing in size until his arrival here on the 10th Nov. last. His general health was now very much impaired; he was very thin, had no appetite, and presented all the peculiarities of the hectic state. The tumor was not very painful, was oblong, and occupied the anterior surface of the lower end of the femur, being attached to the tendinous insertion of the quadriceps femoris into the patella. Upon percussio it yielded the sensation of a gelatinous mass, which might be easily mistaken for the fluctuation of a fluid contained in a very firm cyst. The limb, at this point, measures nineteen inches in circumference, and the tumor eleven inches in length. The skin was not implicated in the disease, but presented many large and prominent veins.

The encephaloid nature of the tumor having been diagnosticated, the question of amputation presented itself as alone offering any chance of recovery from so formidable an affection. Yet the general condition of the patient was so bad, and the liability of a return of the disease so well known, that the operation was undertaken with but little hope of success. The circular amputation was performed on the 13th Nov., and the section necessarily made very high up, only a few inches below the trochanters. The boy having been put under the influence of chloroform, bore the operation without pain. Adhesion by the first intention was only partial, and spongy granulations springing up around the bone, led to serious apprehensions that the disease was already returning. These, however, were subdued, and the wound slowly cicatrized. We have recently heard that he was still doing well, and learning the shoe-making business. My friend, Dr. Harriss, of this city, having subjected a portion of the tumor to microscopic examination, detected the characteristic carcinomatous cells in great number.

CASE II. *Extensive Sloughing of the Foot, from an old burn—Amputation below the Knee.*—The subject of this case was a negro man called Major, about 40 years of age, and belonging to Dr. James Oliver, of Burke Co. It appears that in early life Major was stricken by the "sweep" of a lumber wagon, which fractured and depressed slightly a portion of the left parietal bone, from which time he became affected with convulsions of an epileptic character. About sixteen years ago, in one of his "fits," he fell in the fire, and sustained a very serious burn of the entire left foot, which has *never healed*; nor has he had another convulsion since. On his arrival here (16th Dec.), the foot presented a hideous appearance, and emitted an insupportable odour. The toes all destroyed; the remainder of the foot and ankle nearly double the size of the other; a vascular, spongy, sloughing surface in lieu of skin, furnishing an abundant discharge of fetid matter, and occasionally bleeding profusely; the body emaciated; the system extremely anæmic from the repeated hemorrhages, which were said to have sometimes amounted to a pint; the tongue as white as cotton; no appetite, and the pulse very frequent, though full, as is usually the case in

anæmia. Such was Major's condition when placed under the care of Prof. Dugas.

It was evident that life could not be prolonged much without a removal of the local disease, and it was equally so, that no local or general treatment offered any prospect of success; yet the sudden removal of an extremity which had been for so many years suppurating thus abundantly, and whose condition had acted so revulsively as to arrest the epileptic attacks, could not be considered without regard to ulterior consequences. In order, therefore, to accomplish what was necessary to the immediate safety of the patient, and at the same time to prevent subsequent evils, the diseased foot was amputated (18th Dec.), and a seton established in the other leg on the following day. Chloroform having been administered for the amputation, the patient continued drowsy for some hours after, unconscious of the fact that his limb had been removed. During the night, and for about a week after, he was more or less delirious. Whether this effect should be ascribed to the chloroform or not, is questionable. The circular amputation was performed below the knee at the usual place: adhesion by the first intention failed, because of the unruly state of the patient, who was continually pulling away the dressing. Under the use of chalybeates the appetite improved, but he was then taken with diarrhœa. Astringents, combined with the chalybeates, obviated this difficulty. As convalescence was fully established, he became extensively œdematous. The stump, however, gradually cicatrized, the seton discharged freely, and he was able to return to his master in about five weeks. I have recently heard that he was doing very well. Whether the convulsions will return remains to be seen.

CASE III. *Extensive Sloughing of the Knee, from an old Burn—Amputation of the Thigh.*—Davy, a negro man, 65 years of age, was directed to the care of Prof. Dugas, by Dr. Beggs, of Columbia Co., and arrived here on the 6th January. He stated, that in his childhood he sustained a very severe burn of the knee, which left an extensive cicatrix occupying the anterior and lateral surfaces of the joint; that, apart from the rigidity it occasioned, he suffered no inconvenience from it

until about six or eight years ago, when the cicatrix became sore, ulcerated extensively, and incapacitated him for labor. The entire surface was very much in the condition of that described in the second case. The sloughing and discharge emitted such an intolerable stench that no one could abide near him; the pain was constant and occasionally excruciating; he could not flex the limb in the least; the ulcerated surface extended about six inches above, and as much below the patella, which was exposed and carious on its anterior surface. The old man's general health, although enfeebled, was by no means so bad as that of Major.

There was no hesitation as to the proper treatment; chloroform was administered and the limb amputated (circularly) at about the middle of the thigh, on the 7th January. Adhesion by the first intention was not complete, but sufficiently so to allow him to recover very soon, and to return to his friends.

CASE IV. Destruction of the Foot by fire, during anæsthetic intoxication by Spts. of Turpentine—Amputation below the Knee.---This is also a case of burn, but under singular circumstances. The negro Reuben, aged about 60 years, had long been in the habit of indulging too freely his appetite for stimulants, and had of late resorted to the use of spirits of turpentine, when he could not procure the more palatable combinations of spirits of wine. The festivities of Christmas week had furnished him a liberal supply of alcoholics, when, on the evening of the 30th December, he added a full potation of spirits of turpentine, and went to sleep upon the floor with his feet near the fire, as is very common with this class of people. On the following morning his fellow servants found him still soundly asleep, with one foot upon the burning wood, his shoe, stocking, and the lower end of the pantaloons having been entirely consumed. He was aroused, and walked out to urinate, saying that he felt no pain in his foot, and that he did not believe it was burnt. On returning into the house, he took another drink of the turpentine and went to bed. The patient being in Hamburg, Dr. Creighton was called to see him, and requested Prof. Dugas to meet him in consultation at noon on the 31st. The old man was found asleep, but was easily awakened, when he

still denied having any pain in the limb. The surface of the foot and leg, half way up to the knee, was completely charred, and the deep seated parts felt as though they had been thoroughly desiccated. No sensation was experienced on plunging a knife into the affected tissue, although he felt it when carried above.

As it was deemed proper to await the subsidence of the effects of the intoxication before proceeding to amputate the limb, this was deferred until the 3d January, when it was removed a little below the knee.

The chloroform did not in this case induce the comatose state, although it was very freely inhaled. It simply produced intoxication; yet insensibility was so complete that the amputation was effected during his conversation with the bystanders, and without his knowledge, for he was quite surprised when informed that the foot held up to his observation was his own. Prof. Dugas states, that he has repeatedly observed that it is very difficult to produce the comatose effects of anæsthetics in persons addicted to intemperance.

On examining the amputated extremity, it was found that the tissues of the foot and leg, up to about three inches below the section were completely dried and resembled jerked or smoked beef. Above this they were tumid and infiltrated with serum.

An opiate was given Reuben at bed-time, but he passed a very restless night, being much annoyed with strangury, and seeming still somewhat intoxicated. On the following day he evinced symptoms of approaching mania *â potu*, with occasional hiccough. Alcoholics, opiates, and broth, were administered; he seemed to improve a little; but as the strangury subsided he became troubled with incontinence of urine; mania *â potu* was not developed, but he remained flighty; the hiccough increased; his appetite failed; the energies of the system gradually sank; and he died on the 13th January, the stump having only partially healed.

This case is remarkable; it illustrates the extent to which the taste may be depraved by intemperance; it establishes the new fact that spts. of turpentine may induce complete insensibility; and it shows the serious and persistent deleterious effects of this agent upon the urinary apparatus as well as upon the

general system. Reuben never appeared to be entirely relieved from the intoxication during which he was burnt.

CASE V. Fall from a height of fifty feet, without serious injury.—On the 4th day of December, Prof. Dugas was called to see a negro man (Nace), who had just fallen from the scaffolding of the fourth story of the new cotton factory, a height of about fifty feet. The patient was found upon the floor, apparently very much chilled, (although well covered,) perfectly rational, with good pulse, but complaining bitterly of intense pain in the back, which he thought he had broken. Upon a careful examination, no other injury was detected than a contusion of the dorsal region, one of the forehead, and one of the occipital region. He was bled, took an anodyne, and was quite well in a few days.

It seems that the impetus of the fall was broken by his coming in contact with beams at different distances.

CASE VI. Fall from a height of twelve feet, producing excessive Concussion of the Brain.—On the 14th of December, at 8 o'clock A. M., Mr. P.'s negro man Lewis fell from the sleepers of one story of the cotton factory, down to the floor beneath, about twelve feet. Prof. Dugas saw him about half an hour after the accident, and found him in a state of insensibility and total unconsciousness, with surface very cold, (the weather was intensely cold,) pulse almost imperceptible, pupils contracted, and stertorous breathing. His friends had tried to make him drink, but he could not swallow; no calls could arouse him in the least, but severe pinching would induce a retraction of the limb. He was put to bed, covered up warmly, and had hot bricks applied to his feet. The stertorous breathing gradually subsided during the day, his pulse became better, and at sunset he was able to swallow water when put into his mouth. On the following morning he would groan when called loudly; and in a week he began to speak freely, but incoherently. The pulse was still feeble and small, the reaction never having been febrile in the least. A blister was now applied to the back of the neck, and a mild saline cathartic administered. His previous condition had not permitted the use of any depletions, and

revulsives of heat and mustard had alone been resorted to. His general health and appetite improved rapidly after the tenth day, and he was sent home (in the country) on the 31st December. His mind, continued, however, very much alienated for a fortnight, and then resumed its normal state.

This case offers a striking contrast with the preceding one. In that, a fall of fifty feet occasioned contusions, but no serious consequences—in this, a fall of only twelve feet, attended with no visible contusion whatever, was followed by excessive concussion of the brain, by the most alarming train of symptoms, and by temporary insanity.

This was one of the best cases for studying the differential diagnosis of concussion and compression we have ever seen. In his clinical lecture upon it, Prof. D. took occasion to dwell at length upon this important subject. The stertorous breathing, the unconsciousness, the immobility of the limbs, and the great degree of insensibility, simulated the phenomena of compression. But the surface was cold; the pulse was almost imperceptible, instead of being full, strong and slow, as in compression; when severely pinched on either side the limb would move; the features were not drawn to one side. There was here a resolution of the whole system under the depressing influence of the shock sustained by the entire encephalic mass. While compression produces hemiplegic disturbances, those occasioned by concussion affect the whole system equally.

CASE VII. *Ulcerated Lipoma over the Occiput—Removal.*—On the 7th of January, Mr. Oakman's negro man, Ben, (about 50 years of age,) was presented to Prof. D.'s surgical clinic. He had long carried upon the occipital region a tumor which gave him but little inconvenience until the last few weeks, when its surface became ulcerated, and was being continually injured by his hat. The tumor was now about the size of a hen's egg, discharged a very offensive matter from its entire surface, and bled occasionally when irritated. From its induration and general aspect, the diagnosis was doubtful, especially as the surgeon had not seen it previously. Its removal was, however, determined upon, and effected by a double elliptic incision, including all the ulcerated portions of the scalp. A small vessel

was ligated, and the edges of the wound drawn by adhesive strips up to within an inch of each other. Cicatrization gradually took place without any circumstance worthy of notice. The true nature of the tumor was revealed by microscopic inspection, by Dr. Harriss, who detected no sign of carcinomatous cells.

CASE VIII. *Fibrous Tumour of the Mamma—Extirpation.*—This case occurred in the person of Elizabeth, a servant of Dr. W. H. T., about 22 years of age, and the mother of several children. Some three or four months previous to the operation, and without any known cause, a tumor was observed in the mamma, which, although not painful, had grown so rapidly as already to have reached the size of a small hen's egg. There were no enlarged glands in the axilla. The extirpation was effected on the 7th January, under the influence of chloroform. Some hemorrhage supervened in the afternoon, but nothing worthy of note afterward. The tumor was found to be purely fibrous, and will therefore probably not return.

ARTICLE XVI.

Injuries of the Cranium—Trepanning. By HENRY F. CAMPBELL, M. D., Demonstrator of Anatomy in the Med. College of Georgia.

From the delicate structure of the brain, its abundant supply of bloodvessels, the proneness of its membranes to inflammation and withall its importance to the animal economy, injuries of the cranium have been ever regarded among the most serious to which the organism is liable. And yet experience and an attentive observation of cases have left no rule by which we can prognosticate with certainty the result, or estimate the amount of danger attaching to any particular case; the slightest blow on the head, the simplest incision of the scalp, however trivial it may at first appear, will sometimes unexpectedly assume the most alarming aspect, and finally terminate in the death of the patient from a propagation of inflammation to the brain or its membranes. On the other hand, how much

do we find this delicate organ capable of resisting—what amount of injury will it *not* sustain and yet recover, without even the manifestation of damage. It is only necessary to refer to the records of Surgery and we find ample verification of this assertion.* Hennen reports many cases wherein the brain has been penetrated by balls, (in one case the removal of the ball was not effected) without causing the death of the patient or even producing any great amount of cerebral disturbance. In the second volume of the *Lancet*, Dr. Cunningham, of Hailsham, reports a very remarkable case of a boy 14 years old, who, on the bursting of a pistol, received the whole breech into the substance of the brain through an opening in the frontal bone. He lived twenty-four days in a semi-comatose condition, and in the post-mortem examination the wound in the brain was found perfectly healed, and the iron breech, weighing nine drachms, was resting on the tentorium. But the case which more than all others is calculated to excite our wonder, impair the value of prognosis, and even to subvert our physiological doctrines in relation to this important portion of the organism, is that of Dr. Harlow, reported recently by Prof. H. J. Bigelow, (in the *American Journal of Medical Sciences*), in which an iron crowbar, three and a half feet in length and one and a quarter inch in diameter, passed through the left hemisphere of the cerebrum, and yet the patient perfectly recovered with only the loss of an eye.

The following case, although not by any means so extraordinary as the above, is still sufficiently remarkable to possess, we hope, some interest to the profession—first, on account of the amount of injury sustained by the brain, without a fatal result, and secondly, because it, in a measure, serves to corroborate the physiological possibility of Prof. Bigelow's truly wonderful case; for it will be observed that very nearly the same region of the brain sustained the injury in each instance.

Fracture, with extensive injury of the brain and membranes.
—Tony, a negro carpenter, aged about 45 years, was brought into our infirmary, wounded in an affray with two other negroes. He had a stab in the left thigh, of no serious moment. The

* Military Surgery.

principal injury sustained was that of a chop on the head with an axe. On examination, it was found that the axe had cleft the lower portion of the parietal bone just above the attachment of the temporal fascia, penetrating deeply into the right hemisphere of the brain. The two portions of the skull were widely separated, being nearly an inch apart; between them the movements of the brain could be distinctly observed. The wound was nearly six inches in length, extending from near the vertex to within an inch of the supra-orbitary ridge. The membranes of the brain were cut through and the medullary substance of the cerebrum had been sliced in the direction of the blow. The middle meningeal artery was severed, and yielded a profuse hemorrhage, which of course was external. The temporal arteries also had bled very freely. We found him sitting upon a chair before the fire, with his head bound up in a handkerchief; he spoke rationally, said they had tried to kill him, and recounted correctly the particulars of the affray. He complained of no pain, but said he was weak from loss of blood, though his pulse indicated no great degree of enfeeblement. His appetite was quite remarkable; we found it necessary to order out of his hands a dish of bread and bacon that his wife had just brought him, and which he said he could eat, as he was very hungry.

After proper cleansing and arresting the hemorrhage from the great meningeal artery, by the pressure of a small pledget of lint, the wound was dressed by adhesive strips with a compress and bandage. We found it impossible to bring in contact the two sides of the opening in the skull, and after approximating them as closely as practicable, the edges of the wound in the scalp were brought together over the fissure.

On the second dressing the wound had adhered pretty firmly, with the exception of about two inches; from this opening, which was enlarged by a slight slough, the sharp edge of the outer portion of the skull protruded. The suppuration was very profuse. The impossibility of covering this portion of bone, and the fear that the exposure and continued suppuration would produce extensive caries or be detrimental to the already injured brain and membranes, determined us on removing this piece of bone with the saw. After dissecting the temporal muscle and fascia from

their attachment, assisted by my brother, Dr. Robert Campbell, we removed, with Hey's saw, a portion of the lower border of the parietal bone, two and a half inches in length and three fourths of an inch in width. The operation was completed without other hemorrhage than that from the branches of the temporal artery cut by the incision in the soft parts, though the piece of bone removed was traversed in three places by furrows for the branches of the arteria meningea magna, which however escaped during the application of the saw. After trepanning, the flaps were replaced and secured by adhesive strips with compress and T. bandage. On the fourth day after the operation, he complained of pain in the occipital region and some stiffness in the back of the neck; these symptoms, however readily disappeared after free evacuation of the bowels and the administration of an opiate. This resulted probably from the constrained position of the head, and not from any tetanic tendency. The wound healed rapidly till within a few days of the discharge of the patient from the Infirmary, when suppuration became more copious, and we extracted a small piece of detached bone from the wound, which after this became a firm and healthy cicatrix.

A case very similar to the above is related in a work of one of the earlier writers, Glandorp, wherein a man had the skull laid open by a sabre cut, losing even a portion of the brain, and yet he recovered; though he was affected for some time with paralysis.*

The remarkable feature in cases like the above, is, that notwithstanding the extensive injury done the brain, the very organ of sensation and volition, yet not the least impairment of intellectual power is observed during their whole progress. From an attentive consideration of such cases, and a compari-

* Hominem, ablata etiam insigni cerebri portione, supervivere posse, sequens testatur observatio. Viro alicui robusto impingebatur gladio vulnus circa anteriorem capitis partem, ex quo primis diebus *Pareus meus* Ludovicus Glandorp *chirurgus* magnas ossium portiones, tandem & tantam cerebri partem, quantam vix poterat capere medietas putaminis ovi, excepit. Huic adhibuit remedia, quæ inferius præscribentur. Convulsionibus interim correptus fuit, quæ tamen septima die eum deseruerunt: ubi suborta est paralysis oppositi lateris; a vulnere curatus evasit & post duos annos peste obiit: vini portionem exiguam, post curam, ut & clamores ac fonitus turbaum, tympanorum, ac tormentorum bellicorum vix perferre poterat.—*Matthiæ Glandorpii*, Spec. Chirurg. Observ. v., p. 26.

son of them with others of a somewhat different nature, viz., cases of compression, we have been long impressed with a belief, that the brain can much better resist, physiologically, the effects of actual laceration of its substance, even to a very great extent, than a comparatively small amount of compression.

That a very small degree of compression will produce great disturbance of function the daily observation of every practitioner will fully establish, yet certainly in the deposition of an apoplectic clot, the brain cannot sustain as much physical injury as when it is broken and lacerated with tamping pins and hand axes. But, strange to say, in the latter cases no intellectual impairment whatever is manifested, while in the former coma, paralysis, and often death, are the common consequences of a small amount of pressure. What then is the explanation of this unexpected difference in the results? We can only surmise an explanation: It is probable that the laceration of the cerebral mass affects only that portion of the brain actually impinged upon; in this, there may or may not reside some endowment important to the undisturbed exercise of the various functions, as volition, sensation, consciousness, &c.; if important, we have functional manifestation of injury in those organs over which that portion presides; if unimportant to these functions, of course these manifestations are not observable. On this view, we would very naturally expect to find lacerations of the brain without functional manifestation, as the effect of these lacerations does not, it appears, extend beyond its immediate vicinity.

Now, in injuries with compression, the effect is by no means so restricted; we cannot compress one portion (however unimportant) of an organ like the brain, without exerting more or less compression on parts that are of the utmost importance. That equilibrium of pressure so indispensable to the healthy exercise of function is thus destroyed in *all* parts of the brain, and of course a correspondent impairment of function is the result; which does not necessarily occur in simple laceration.

ARTICLE XVII.

Case of Permanent Stricture of the Œsophagus. By PAUL F. EVE, M. D., of Augusta, Ga.

During the course of lectures in the University of Louisville, Ky., I was invited by Prof. Rogers to see, with him, a case of *dysphagia constricta*, which had been under his care for a few weeks. The patient was a mulatto boy, aged 3 years, who, some four months previously, had swallowed, through inadvertence, a portion of caustic potash. In its deliquescent state he had taken it for candy. The act was immediately followed by alarming symptoms, but which unfortunately were attempted to be combatted exclusively by domestic remedies.

When Dr. Rogers first saw the case, the dysphagia was so great that fluids could with difficulty be swallowed; and a bougie was now at once arrested in the *œsophagus* by an apparently permanent stricture. Various attempts were subsequently made to reach the stomach, but without success. We were not certain that any nourishment ever entered it. The patient's constant cry was for water, which he would swallow down to the obstruction, retain it a few minutes, and then reject it from his mouth. He rapidly emaciated. Ice-cream, milk and water, beef tea, &c., were recommended; and if none of these could be gotten down, nutritious enemata to sustain his system.

The stricture was situated six inches from the dental arches—below the most usual seat for such affections—which is the connection of the pharynx with the *œsophagus*.

The middle of December last, this patient becoming daily more feeble, was presented to the class at the college clinic, with the view to an operation, should one be deemed advisable. He was now reduced almost to skin and bones; neither could his pulse be discerned at the wrists. It was not until he arrived at this low condition that his master consented to consider the question of *œsophagotomy*. It was decided in consultation not to operate, and the death of the patient was predicted as probable during the first cold spell of weather.

About ten days after this, a post-mortem revealed a permanent contraction with thickening of the tissues of the *œsophagus*.

gus—the diameter of the strictured portion being reduced to about a line for an inch and a quarter, and which was also quite tortuous in its course. The stomach was contracted and reduced to a very small capacity; but the ileum, to our surprise, was largely distended with fæces.

It is highly probable that an attempt at œsophagotomy would have failed.

This is another case added to several noticed in our Journals, of permanent stricture of the œsophagus produced by caustic preparations.

PART II.

Eclectic Department.

On the Physiology and Pathology of the Ganglionic Nervous System. By JAMES GEORGE DAVEY, Licentiate of the Royal College of Physicians, London, &c. (Continued from p. 224.)

“There are yet great truths to tell, if we had either the courage to announce, or the temper to receive them.”—DISRAELI.

CHAPTER II.

On the Physiological Uses of the Solar Ganglion, &c.

In the earliest periods to which the history of man refers, we find that no question has more particularly interested the philosopher than that of LIFE. The wish to draw aside the veil from nature, to display the very essence of the vital properties, and to penetrate to their first causes, has ostensibly characterized the labours of many of the greatest men of both ancient and modern times. (*Vide* Lawrence, Lectures, p. 166.)

The investigation of the physiology of the nervous system, says Dr. Cooke, in his work on Nervous Diseases, seems to have been at all times a favourite study. We have some notices of it in the works of very ancient writers: Hippocrates, Plato, Aristotle, and others, have speculated upon this subject, though in obscure and confused language. By these early writers, the brain, the heart, and the blood were each successively claimed as the seat of life and sensation. Aristotle asserted that the heart is the organ *first formed*. Inasmuch as the philosophy of the ancients, especially Platonism, soared above, or, to speak more correctly, below the level of nature, (*vide* Serres on the “Laws of the Development of Organs or Transcendental Anato-

my applied to Physiology,") it became reserved for subsequent investigators to afford any real explanation of the vital phenomena, and their relation to the organism. Aristotle, in his "History of Animals," was the first, I believe, to give any degree of attention to the study of those organic forms so necessary to be understood as the groundwork of all physiological knowledge. To Aristotle succeeded Galen, whose work ("De usu Partium") must be, at the present day, considered as highly interesting and instructive. In reference to the labours of Aristotle and Galen, Serres observes:—"The method of Aristotle, essentially descriptive, neglected the function for the form; that of Galen essentially rational, neglected the form for the function. The first of these methods carried in its train the descriptive sciences; the second led to the general sciences: the truth thus lay in their combination, and to *Haller* we owe the merit of first discovering this fact. He founded his arguments and opinions upon form and function combined; thus embracing in his method the descriptive as well as the general sciences."

Among the more modern investigators in the science of physiology Harvey no doubt stands second to none other. The discovery of the circulation of the blood, in point of importance and utility, can never be surpassed; and however much we may object to the physiological inferences which Harvey deduced from it, yet we cannot doubt the great claim of Harvey on our esteem and admiration.

What very materially retarded the progress of physiological knowledge was the custom, until lately, of treating and considering the nervous system of man *en masse*—that is, without reference to its natural divisions. This is plainly seen to be the case, if we refer to the physiological writings of Gall, Le Gallois, or Bichât also of Dr. Wilson Phillip.

The brain, spinal cord and organic or ganglionic system of nerves, have each in the very able hands of these writers been rescued more or less from that chaos, perplexity, and doubt, with which their predecessors and contemporaries were accustomed to regard the organs and their respective functions in the animal economy.

The labours of the immortal Gall cannot be sufficiently commended. This great and illustrious philosopher has practically demonstrated the absurdities and chimeras of the metaphysical school. He it is who has given the death-blow to the visionary speculations of a Descartes, a Leibnitz, and a Malebranche; or, to speak of the present day, of a Chalmers, a Roget, a Copland, and of many like them.*

* Nothing can be more interesting than to trace the progress of truth from the days of Bacon, Hobbes, Locke, and Condillac, all of whom advocated the

On his successors rest the responsibility of rescuing alike the criminal from perdition, and the legislator from shame. To the cerebral physiologist must the philanthropist look for the introduction of so improved a system of education as shall insure the use, and not the abuse, of the cerebral faculties in man. The unfortunate lunatic is safe only in his hands, because he alone can understand the nature of his disease, and thus be enabled to make choice of the necessary remedies. Cuvier acknowledged, that to Gall and Spurzheim we were, in his day, indebted for almost all we knew of the anatomy of the cerebro spinal system. Gall was certainly the first to describe the spinal cord as an organ distinct from the brain, and to show that it did not arise from it, as was taught in the schools. (*Vide* Translation of Blumenbach's Physiology, p. 201)

The labours of Le Gallois certainly contributed to advance our physiological knowledge of the spinal cord, although he, in so far as he erred not, to a great extent, it would seem, did but echo the previous discoveries of Gall. M. Le Gallois says, "If in a rabbit, which has been decapitated, but kept alive by pulmonary insufflation, the whole of the spinal marrow be destroyed by a stylet thrust through the whole vertebral canal, life will instantly and irrecoverably disappear, irritability alone remaining, which, we know, remains for some time after death. If, instead of decapitation, an opening be made in the vertebral canal near to the occiput, and by an instrument introduced through this opening the whole of the spinal marrow be destroyed, although the brain and its nervous communications with the trunk remain perfectly untouched, life will be instantly and irrecoverably destroyed in the trunk, the head alone remaining alive, as is apparent from its gapings."

It will be seen from the preceding extract, that the object of Le Gallois is to prove that the *spinal cord* is the seat of life, and not the *brain*; which organ, as Le Gallois truly observes, has been erroneously considered as the sole origin of the nervous power; and consequently the only *seat* of the principle of life. As regards the preceding experiments, they cannot, I think, be considered as favourable to the theory entertained. The possession of "*irritability*," by the decapitated and marrowless

dependency of the mental, or rather cerebral, impression on the use of the external senses, in opposition to the doctrine of innate ideas, to those of Mirabeau, Priestley, Blumenbach, Magendie, Lawrence, and others of what is called the modern school of materialism, on to that era when Gall's discoveries illuminated this dull atmosphere of ours. The first class of writers named may be said to hold a similar relation to the second as the latter does to Gall and his school. The Reids, Stewarts, and Mills of the present day are, it is to be hoped, at a discount. Let them only seek *fact*, and not mere *words*, and farewell to metaphysics with all its dogmas.

animal, assures me that the solar ganglion was still in the exercise of its peculiar function. What is the "*irritability*," of Haller, the "*motions without force*" which belong to the organism, but the property of the organic nervous system? The destruction of the spinal cord through its whole length in the second experiment, no doubt implicated the phrenic nerve; and if so, there is little wonder that the animal continued to gape, as the experimenter tells us it did.*

Le Gallois has, very evidently to me, considered the cessation of the excito-motory function as an indication of the complete suspension of all the vital phenomena.

To Bell and Hall we are not a little indebted for the more recent information we possess of the spinal cord and its functions; Mr. Grainger, too, must not be forgotten.

Bichât and Dr. Wilson Phillip have together contributed largely to our knowledge of the vital or organic functions. It is certainly strange that the arrangement of the functions into classes—viz., the animal and the vital or organic—should have been deferred to so late a period. Bichât, with all his originality and acute reasoning, strangely erred, *not* in saying "that hitherto anatomists have considered the nervous system as an uniform system,"—such had been unfortunately too true,—but in adding "that the different branches of this system ought to be viewed as constituting two," and not THREE, "general systems, essentially distinct; the one having for its principal centre the brain and its dependencies, meaning, no doubt, the spinal cord; and the other, the ganglions.

The translator of Blumenbach's *Physiology* describes, in a

* Since writing the above, I have performed the following experiment:—A free incision was made through the integuments immediately over the spine on the back of the animal; the extensor muscles were then dissected from their attachments, and the spinal column being thus exposed, its posterior parietes were removed to a sufficient extent to admit freely a moderate size blow-pipe. The instrument was introduced near the middle of the dorsal spine, and passed upwards within the spinal canal, through the substance of the brain, to the anterior and interior part of the cranium. The *immediate* effects were a complete paralysis of the four extremities, the *posterior* as well as the anterior, and an *apparent* extinction of life; the *heart alone remaining alive, as was apparent from the gapings of the animal*, to use the expression of Le Gallois. It is *now* nearly three hours since the above experiment was performed, and *although no attempt at an artificial respiration has been made*, the heart is to be seen at this moment through the thoracic parietes, contracting as vigorously as in any other animal. The *gapings* ceased after about ten minutes. In the absence of the cerebro-spinal functions, the animal before me is in a condition precisely similar to the amyencephalous monster of Hall, or to the mere polypus.

The assertion long since made by Le Gallois that every part of the body derives its principles of vitality and irritability from that portion of the spinal marrow from which it receives its nerves, is satisfactorily negated in the experiment recorded. In connection, too, with the above experiment, we cannot fail to remark the strong evidence in favour of that opinion which makes the *heart's* action independent of the *brain*.

note, the two classes of functions thus: "The *animal* functions prove us feeling, thinking, and willing beings: they are the actions of the senses which receive impressions; of the brain, which, perceives them, reflects upon them, and wills; of the voluntary muscles, which execute the will in regard to motion; and of the nerves, which are the agents of transmission. The brain is their central organ. The *vital* or *organic* functions are independent of mind, and give us simply the notion of life: they are, digestion, circulation, respiration, exhalation, absorption, secretion, nutrition, calorification. The heart," adds Dr. Elliotson, "is their central organ."

From what I can learn, it appears that Dr. Wilson Philip has explained himself very much more satisfactorily on the vital or organic functions, and the reciprocal relations between them and the brain and spinal cord, than Bichât has done, though neither the one nor the other has gone so far as he might have done. With the most extraordinary and convincing facts before them, I am not aware that physiologists have attempted to show much more than that the vital or organic functions are "certainly not dependent on the brain and spinal marrow *in the same degree* as the cerebro spinal functions."* If I mistake not, there have been no writers who have directed their attention more successfully to the ganglionic system of nerves, and their functions, than Fletcher, Copland and Breschet, excepting, of course, Dr. Stevens, whose paper published in the year 1842, gives much reason to expect yet better things. In them Bichat and Philip have found very able supporters. Dr. Copland's notes to his translation of Richerand's *Physiology* contain here and there very valuable remarks, physiological and pathological, on the vital or organic system of nerves. The same may be said of the articles *Fever* and *Hypochondriasis* in his very valuable and learned Medical Dictionary. But it is not enough to say that the ganglionic system of nerves, with the solar plexus for its central organ, presides over the functions necessary to life, as digestion, secretion, circulation, nutrition, &c., as if the brain and spinal cord, unlike the stomach, liver, heart, and alimentary canal, had an existence independently of the organic nerves. The functions of the brain and spinal cord must of necessity be regarded as *vital* functions. Life in man and the higher order of animals, consists, as Mr. Lawrence says, in the assemblage of *all* the functions; and if so, on what grounds dare we omit those of cerebration, and sensation, and motion? I shall hope to prove to the reader, that up to the present time physiologists have mistaken the cause for effect; they have looked to the brain, and spinal cord for the animating principle

* Vide Muller's *Physiology*, by Baly, p. 208.

of the ganglionic system of nerves, instead of looking to the latter for the integrity of the former.

In enumerating the opinions of physiologists, I must not omit to mention those of Hunter and Lawrence. As may be expected of those eminent and learned surgeons, the question of the nature of *life* was too important and interesting to be passed silently by. Mr. Hunter's opinions may be best expressed in the words of Abernethy, contained in a letter to Dr. Cooke, and published in his "*Nature and uses of the Nervous System.*" Abernethy writes thus: Mr. Hunter's illustration of his notions of life, by saying that it was superadded, as electricity and magnetism may be to substances in which they may inhere, was given in his lectures, and I have heard it from his own mouth. It made a strong impression upon my mind, because it did not affirm what electricity, magnetism, and life were, but only stated an analogy." Both Hunter and Abernethy strongly insisted on the importance of not confounding life with organization, as many did. Mere composition of matter, observes Hunter, does not give *life*, for the dead body has all the composition it ever had; nor do organization and life depend in the least on each other. Organization, he adds, may arise out of living parts, and produce action, but life can never rise out of, or depend upon, organization. An organ is a peculiar conformation of matter, let that matter be what it may, to answer some purpose, the operation of which is mechanical; but mere organization can do nothing even in mechanics; it must still have something corresponding to a living principle—namely, some power. This *living principle* Hunter recognized by the term "*materia vitæ diffusa*," and which Mr. Lawrence ridicules as a mere fancy, an idle speculation, and of no better repute than the "*impetum faciens*" of Hippocrates, the "*archæus*" of Van Helmont, or the "*anima*" of Stahl. In commenting on these definitions of a living principle, Mr. Lawrence observes: "Most of them have long lain in cold abstraction amongst the rubbish of past ages; and the more modern ones are hastening after their predecessors to the vault of all the Capulets." Mr. Lawrence's views of life I cannot think are more satisfactorily than those of Hunter. At the same time that it is admitted that to call life a property of organization would be unmeaning, would be nonsense, he claims for the "*primary or elementary animal substances*" those vital properties which enable the compounded organs to carry on their several functions, which, he adds, being united in the individual, constitute *life*. Mr. Lawrence, therefore, literally makes life the property of the organism—just as gravity, elasticity, &c., are the physical properties of inorganic bodies. Mr. Lawrence, it is seen, has

herein adopted the explanation offered by the German physiologist Reil,—in reply to whom Müller says: “It would follow, that if the elementary composition were alone the cause of the organic forces, it would be at the same time the formative principle itself. Now, since in organized bodies, immediately after death, the elementary composition of the organic matter does not appear to be different from that of bodies still living, Reil must admit the existence of other more subtile elements, not recognizable by chemical analysis, which are present in the living body, but are wanting after death.”

In reference to this matter, Dr. Elliotson has these words: “As the fluids which form the embryo must be endowed with life, organization cannot be the cause of life; but in truth organization is the effect of life, although, when produced it becomes an instrument of life. The erroneousness of the French doctrine, that “life is the result of organization,” has “been ably refuted. The error appears to have arisen, in some measure, from the want of definition, the word life being used, sometimes properly, for the power; sometimes improperly, for the result. Even if the result of life—the functions of a part—should be called its life, life could not be said to be the result of organization, but of a power to which organization is an instrument.” Now both the organization and its functions may be said to be demonstrable to the senses, but the same cannot be said of the “*power* to which organization is an instrument,” though neither its presence nor its seat can well be doubted. Granting, for the sake of argument, that the *solar ganglion* is the source or origin of this *power*, or *organizing principle*, or *creative force*,* which is exerted even on itself, and in every animal, strictly in accordance with what the nature of each requires, which exists already in the germ, and creates in it the essential parts of the future animal, and according to an eternal law, forms the different essential organs of the body, animates them, and modifies the already existing nervous system, as well as all the other organs in the laws of insects, during their transformation, causing the disappearance of several of the ganglia of the nervous cord, and the coalescence of others, and by its operation, during the transformation of the tadpole to the frog, causes the spinal marrow to shorten in proportion as the tail becomes atrophied, and the nerves of the extremities formed, &c.,—granting, I say, so much, it remains to show, if possible, the *ultimate cause* of this important part of the organism—from whence did it receive its being? It is not in the power of *man* to offer anything more than a very general reply to these queries.

* Physiologists say it has an existence independently of any special organ. See Müller's Physiology, p. 26, et seq.

Mr. Lawrences observe, in his lectures on life—"Having thus proceeded as far as we can in ascertaining the nature of life by the observation of its effects, we are naturally anxious to investigate its origin, to see how it is produced, and to inquire how it is communicated to the beings in which we find it. We endeavour, therefore, to observe living bodies in the moment of their formation, to watch the time when matter may be supposed to receive the stamp of life, and the inert mass to be quickened. Hitherto, however, physiologists have not been able to catch nature in the fact. Living bodies have never been observed otherwise than completely formed, enjoying already that vital force, and producing those internal movements, the first cause of which we are desirous of knowing. However minute and feeble the parts of an embryo may be, when we are first capable of perceiving them, they then enjoy a real life, and possess the germ of all the phenomena which that life may afterwards develop. These observations, extended to all the classes of living creatures, lead to this general fact, that there are none which have not heretofore formed part of others similar to themselves, from which they have been detached. All have participated in the existence of other living beings before they exercised the functions of life themselves."*

* This opinion, so confidently asserted by Mr. Lawrence, is more than likely to undergo very considerable modification, in consequence of the publication of the "*Vestiges of the Natural History of Creation*," wherein it is to my mind satisfactorily shown that *life* does not even now only proceed from *life*. The experiments of Messrs. Cross and Weekes are conclusive, and clearly negative the contrary assumption so much insisted on. The appearance of the "*Vestiges, &c.*," must be viewed as a great and glorious era in the world's progress. It marks the onward course of truth and of religion, untainted with that mean and despicable credulity with which the ignorant delusions of the fanatic, of whatever denomination, associate and degrade it. The character of the reviews which I have seen of this excellent and truly philosophical book bespeak too plainly so great a preponderance of the low, the animal propensities, that, it may be rightly inferred, the time is yet far distant when the constant and unflinching supremacy of the moral sentiments and the intellect shall find *man* in unison with that nature of which he is but a part, and so no longer deserve the censure of the poet, as conveyed in the few following lines:—

"Hath Nature's soul,
That formed this world so beautiful, that spread
Earth's lap with plenty, and life's smallest cord
Strung to unchanging unison, that gave
The happy birds their dwelling in the grove,
That yielded to the wanderers of the deep
The lovely silence of the unfathomed main,
And filled the meanest worm that crawls in dust
With spirit, thought, and love; on man alone,
Partial in causeless malice, wantonly
Heaped ruin, vice, and slavery; his soul
Blasted with withering curses; placed afar
The meteor happiness, that shuns his grasp,
But serving on the frightful gulph to glare,
Rent wide beneath his footsteps?"

SHELLEY.

“Thus we find that the motion proper to living bodies, or, in one word, life, has its origin in that of their parents. From these parents they have received the vital impulse; and hence it is evident, that, in the present state of things, life proceeds only from life, and there exists no other but that, which has been transmitted from one living body to another, by an uninterrupted succession.” If organism or the organized state be the result of the union of the organic creative powers and organic matter, it would seem plausible to infer that the first is supplied by the male animal, whether oviparous or viviparous, and the second by the female. The peculiar circumstances which, it may be presumed, attend this union, and the consequent development of the vital properties, will probably remain a mystery. It would, however, appear, that the first effort of the vital properties, whatever they may be, are directed towards the development of a central organ, the solar ganglion,* predestined to hold a precisely similar relation to the otherwise insensible and inert frame, to the dull and unmoving organism, as the vital fire to the animated statue of Prometheus.†—[*London Lancet*.

Report of cases of Tetanus cured by the division of the injured Nerve. By MOSES SWEAT, M. D., of North Parsonsfield, Maine.

CASE 1st. Peter Gerrish, a stout athletic mulatto man, aged 24 years, wounded the ball of his right thumb with the point of a scythe, on the 1st day of August, 1825. The wound healed kindly by the first intention; but on the ninth day unequivocal tetanic symptoms developed themselves, in trismus, pain in the jaws, opisthotonos, rigidity of the upper extremities, &c. The paroxysms so increased in violence and in rapidity of recurrence, that in 24 hours he became insensible to every thing around him; and it constantly required from four to six men to keep him on a mattress on the floor, so violent were the spasms. It was evident to all who saw him at this time, that he could not survive but a few hours longer. I proposed to the family in which he resided, to cut down and divide the injured nerve in the wrist, to which they gave their consent. An in-

*The reader is referred to the commencement of the first chapter, where he will find it stated that it is the opinion of Ackermann, Rolando, Blumenbach, and Gall, that the ganglia of the organic or ganglionic system of nerves are the first formed in the fœtus.

†See Lectures on the Study and Practice of Medicine, page 124, by John Conolly, M. D., late Professor of the Theory and Practice of Medicine at the London University, and Physician to the Middlesex County Lunatic Asylum, Hanwell, &c., &c.

cision of three inches was made accordingly, over the course of the median nerve, which, by a careful dissection, was soon found and divided. The spasms ceased *instantly*, not a muscle was seen to move (except those of respiration); he was perfectly still about an hour; he then aroused, and looking wildly around, inquired how he came there—what had happened, &c. He had no recurrence of tetanic symptoms afterwards.

CASE 2d. John Johnson (son of David Johnson of this town), aged 20 years, shot off one joint of his right thumb with a musket, on the 16th of January, 1826. It was dressed properly, and it healed kindly until the seventh day after the accident, when he was suddenly seized with tetanic symptoms,—his jaws soon became fixed, attended with severe pain—and the phenomena, in short, were the same as those in the preceding case. His parents and friends were unwilling to consent to the operation for the division of the nerve, until they were all satisfied that he was fast failing, when they consented. The operation was carefully performed, and the patient was instantaneously cured of all tetanic or spasmodic symptoms. The wounds healed in a short time, and he had no recurrence afterwards.

CASE 3d. Miss Almeda Kimball, of Hollis, aged about 20 years, of slender habit, had a branch of the ulnar nerve punctured at the right wrist, with the point of a needle, about the 1st of January, 1848. Severe pain followed immediately, which continued to extend until it affected the whole course of the nerve: inflammation soon followed about the wrist, which extended over the whole fore-arm, attended with great swelling and total inability to move the limb. I was called to visit her on the 25th of that month, found her under the care of Dr. Cheney, whose treatment was judicious. She was *then* affected with pain in the side of the neck and jaws, with some rigidity of the muscles about these parts, as well as severe pain and inflammation in the injured limb. I suggested the propriety of dividing the nerve, if tetanic symptoms should increase. On the 7th February, I visited her again; she had unequivocal symptoms of tetanus. We divided the ulnar nerve, and took out an inch, just above the groove in the inner condyle of the humerus. All symptoms of tetanus ceased and she soon recovered. In all cases where I have found it necessary to operate, I have removed a *small section* of the *injured nerve*.

I might mention several other cases, in which I have stopped spasmodic affections by dividing nerves, in wounds which I

have been called to operate on, for the security of arteries, in cases of aneurisms, hemorrhages, &c., improperly managed. One case I will relate, viz. :

CASE IV. On the 3d December, 1810, (while I was in practice with Dr. B.) I was called to visit J—— B—— of Limberick, who, in a fit of delirium tremens, on the 25th Nov. struck his fist through a square of glass and wounded the radial and ulnar arteries at the wrist. Dr. A. of N. was called, and dressed the wounds; stopped the bleeding (which was said to be rather profuse) by means of compresses and bandage, which prevented external hemorrhage while the compresses were closely applied, but, whenever they were loosened, the wound over the *ulnar* artery, it being rather large, would bleed. The wound in the integuments over the radial artery was but a mere puncture, and it had, in fact, healed by the first intention, while the wounded artery continued to bleed, and formed a large false consecutive aneurism. He had, that day, began to have some spasmodic contractions of the muscles of the arm. Dr. A. was present. It was agreed upon to operate and secure the arteries. The wound of the integuments over the ulnar artery, was extended to about two inches in length; the artery bound and secured by ligatures above and below the bleeding orifice made by the glass. We then made an incision through the integuments over the aneurismal tumor (which was large) to the extent of four inches; removed the *coagula*, found the artery, and secured it. In this operation, we found several spiculæ of glass, and on examination, found one small piece which had wounded the median nerve; to this injury of the nerve we ascribed the spasmodic symptoms which had just begun to develope themselves. We *divided it*, and all the spasmodic symptoms ceased at once, and he had no more afterwards.—[*N. Y. Journal*.

Cauliflower Excrescence of the Uterus. By W. H. CHURCH, M. D., of New York.

A. M. M., æt. 46, born in the state of New York, married, was admitted into the New York Hospital on the 17th of Dec., 1850. John Watson, M. D. attending surgeon.

The first notice the patient had of the above-named disease was one year since, when she was attacked with severe pain in the lumbar region, being aggravated during and after a passage from the bowels. She had suffered for several years previous to this time with hemorrhoids, consequently these troubles were attributed to them, and a course of treatment adopted with

reference to the hemorrhoids. Three months after the commencement of the pains she noticed a small tumor in the vagina, which has continued slowly to increase, until it has attained the size of a hen's egg. All treatment prior to her entrance into the hospital has been of a palliative character, the tumor not having been interfered with, except by the use of anodyne injections, *per vaginam*, to relieve the pain.

The patient has always lived upon a farm, and occupied in the regular habits of that class of people; has had several children, the youngest being nine years of age. She says that since the discovery of the tumor there has been a continual watery discharge from the vagina, which is now so profuse as to saturate ten or twelve napkins during the twenty-four hours. Hemorrhage has occurred several times, but in such slight quantity as to cause her no uneasiness. She is emaciated, and feels her strength beginning to fail, but during the whole time the menses have continued to flow pretty regular. To relieve the pain sufficiently to sleep, she has been obliged to take from one-eighth to three-eighths of a grain of morphine at bed-time for the last two months. The character of the pain is that of dragging in the lower region of the back. Upon examination *per vaginam*, the os and cervix uteri are felt largely distended, their walls being thinned and indurated; the cavity of the uterus is occupied by an irregular mass of a firm consistence, which can be traced up to its middle, and it is there found attached to the sides of that organ. Upon examining a piece of the tumor, which can easily be detached with the fingers, its surface is more florid than flesh color, with an irregular surface of a granular appearance, and from these points a white cheesy matter can be pressed by the fingers.

Dec. 18th. The speculum *vaginæ* being introduced, portions of the tumor were removed with the volsella and the raw surface freely rubbed with the *argentum nitratis*; pieces were thus twisted off, and the surface of the tumor coated over with nit. of silver until the inner surface of the os uteri was brought into view, when the operation was discontinued. Hemorrhage during the operation moderate in quantity.

Dec. 19th. Patient quite comfortable; there being slight hemorrhage, was ordered to use an injection of sulph. of alum 3ij. aqua ʒ viij.

Dec. 23d. No hemorrhage has occurred. There is now a discharge of thin purulent matter of an offensive character from the vagina.

Dec. 26th. Another examination was made, in which the operation was very much accelerated and a better view of the parts obtained by separating the walls of the vagina with three

spatulas in the hands of assistants. A large quantity of detritus matter was found about the os uteri, which was removed, and the same course pursued as at the previous examination, the whole tumor being nearly removed by the instrument when the remainder was thoroughly coated over with the solid nit. of silver (which was also used at the previous operation). The operation was a tedious one, requiring nearly an hour; the exhaustion and suffering of the patient was much less than could have been expected. The hemorrhage amounted to $\frac{3}{4}$ xij.

Dec. 28th. Has been comfortable, but complains of weakness, pulse small, and appetite very delicate. Ordered the bitter infusion, a wine glass full three times daily, nourishing diet and porter.

Jan. 2d. Patient is very anxious to leave the hospital and go to her friends, consequently she was to-day discharged, relieved, which certainly was the case, as the pain was almost entirely gone; could sleep better, appetite and strength had somewhat returned, and instead of saturating ten or twelve napkins daily, three or four were found sufficient.

Remarks.—Dr. C. M. Clark reports one case in which the disease had not returned in twelve years; that case was treated principally by astringents, and no operation was performed. The astringents had so contracted the vagina that it was almost impossible to introduce the finger. The case which we have just reported presents a good opportunity to test the possibility of a radical cure, as it is comparatively recent, having come under treatment in less than a year after the appearance of the first symptoms; the subject is not much past the middle age, with a naturally good constitution, upon which the disease has not yet made much impression.—[*Ibid.*]

External Applications in Dropsy.

To the Editor of the Boston Medical and Surgical Journal.

DEAR SIR,—The article in your Journal of December 25, "On the Treatment of Ascites by Diuretics applied externally," reminds me of a little of my own experience in the use of external remedies in dropsy.

Some six years since, I was called to see a child, 2 years old, with general anasarca and ascites. He had been under treatment some three or four weeks; but was steadily getting worse, and had been left by the attending physician as a hopeless case. His whole body was enormously distended; his features seeming hardly human. Appetite voracious; constant thirst;

bowels very loose; urine scanty; pulse feeble, quick and very frequent; patient restless, and constantly moaning. The usual treatment with diuretics and cathartics, would reduce the distension a little; but what was gained one day would be more than lost the next; for the patient was evidently losing strength, while the disease was steadily gaining ground. As a forlorn hope, I directed the following liniment, to be applied to one half the surface of the body, three times a day. R. Vol. liniment, 2 pts.; tr. cantharides, tr. digitalis, tr. colchicum, tr. iodine, *aa* 1 pt. The patient was well in a few days, without taking a particle of medicine internally.

Since then, I have used the liniment repeatedly with advantage; several mild cases yielding to this alone, without any other treatment. The liniment is peculiarly adapted to the treatment of dropsy in old persons, children, or delicate females; where the powers of life are feeble, and the stomach and bowels too irritable to bear medicine internally.

Since writing the above, I have treated a case similar to the first one mentioned:—the child having been more or less bloated for several months; legs and body as full “as the skin could hold.” All appearance of disease was removed after a few days’ use of the liniment.

That there is any especial virtue in the particular form or combination of remedies in the liniment, I do not believe; but that external remedies can be used with advantage in most cases of dropsy, I am sure.

Ausable Forks, N. Y. March 1851.

WM. W. FINCH.

Diagnosis of Typhoid Fever. By Dr. E. PARKES.

[Dr. Parkes concludes a clinical lecture on the above subject, by the following propositions:]

1. The diagnosis of typhoid fever is absolute when, on a febrile disease attended with looseness of the bowels, unequivocal rose-spots appear on the sixth or eighth day.

2. If there are no rose-spots, or if these are indistinct, the diagnosis is still nearly certain, if in a febrile disease, mild or severe, which has lasted eight or ten days, there is, or has been, epistaxis; if there is diarrhœa with alkaline stools, abdominal pains, bronchitic ronchi, with considerable muscular weakness, delirium, &c., provided that the *positive* symptoms of the disease above enumerated are absent. If sudamina appear on and after the twelfth day, the diagnosis is strengthened. Hemorrhage from the bowels, in such a case, would almost make the diagnosis absolute, without reference to other affections.

3. If in a disease presenting febrile symptoms similar to those seen in typhoid fever, it is impossible to obtain any of the signs usually furnished by the skin and mucous membranes, viz., rose-spot, diarrhœa, abdominal tenderness, epistaxis, bronchitic rhonchi, &c., the diagnosis of typhoid fever should never be given until inquiry has been made into the possibility of the case being one of those above enumerated. If, in such a case, the diagnosis of typhoid fever be ultimately given, this can be done only on the principle of exclusion, viz., by finding that the symptoms do not accord with the supposition that the disease is typhus, pyohæmia, latent pneumonia (i. e., pneumonia unmarked by the usual symptoms of cough and expectoration,) acute tuberculosis, acute glanders, &c. Now, in many of these diseases, we have special symptoms which are easily recognised; as in typhus, the mulberry rash, the dusky skin, the extreme stupor, &c.; in pyohæmia, the yellowish earthy tint of the surface, or the absolute jaundice, the severe shiverings, the intense headache, torpor, and delirium, which, to a practised eye is, I think, different from the delirium of either typhoid or typhus fever; in variola, malignant erysipelas, and in the gangrenous erysipelas from putrid infection, we have, in the vast majority of cases, diagnostic eruptions, or symptoms derived from the skin and subcutaneous cellular tissue, &c. In acute glanders, there is often the tuberiform cutaneous eruption, and the affection of the nasal mucous membrane; in pneumonia we discover the physical signs, unless the pneumonia be lobular and much scattered, when physical signs often fail; this case, however, is most commonly connected with pyohæmia. All these diseases are usually easily excluded; a very little care will enable us to be certain that they do not constitute the disease before us, and in many cases, even if one of them, such as pyohæmia or erysipelas supervenes on typhoid fever, the fact of there being two diseases present can be made out if the case has been watched.

It is, unfortunately, different with some other affections, especially acute tuberculosis, meningitis (tuberculous, purulent, or simple,) cerebral softening of some kind, and occasionally, delirium tremens. Any of these diseases may produce symptoms which closely simulate an ataxic form of typhoid fever. You will understand that, in many cases, the distinction of typhoid fever and these affections can be made easily by aid of the symptoms derived from the secondary effects on the skin or mucous membranes in typhoid fever, but we are speaking now of cases in which these utterly fail, in which we have decided that the case is not one of typhus, pyohæmia, variola, latent inflammation, &c., and consequently in which we have

reduced the problem to the determination of whether the case is ataxic typhoid fever, or acute tuberculosis, meningitis, delirium tremens, central cerebral softening, &c. I believe that error cannot always be avoided with the utmost care. Acute tuberculosis is most likely to be mistaken when it occurs in children. Often, however, there is a long initiatory period; the abdomen is comparatively unaffected, that is to say, there is little pain or diarrhœa, but there may be more sickness than in typhoid; the head symptoms have a different aspect, i. e., the headache and delirium do not occur in the regular order, but observe unusual alterations, and altogether the case does not exactly square with the symptoms of typhoid fever. The chest symptoms may be prominent, and afford a clue to the real nature of the case, although often all physical signs, except those indicating a general bronchitis, fail. Attention should also be directed to the absence of the positive signs of typhoid fever, viz. the rose-spots. In tuberculous meningitis we may have signs, from the presence of tubercle in the lungs or elsewhere, and sometimes assistance may be derived from considering, if it can be learned, the time when the headache and delirium came on, the contraction of the pupil, the degree of intolerance of light which is greater in meningitis than in typhoid fever; the vomiting, which is more marked in meningitis; the state of the tongue, which is cleaner in meningitis; the comparative mildness of the pyrexia, i. e. of the heat of the skin, quick pulse, &c., in this latter disease. In delirium tremens we are often guided by the kind of delirium, the history of the case, &c., and there is not so much difficulty here as in meningitis. Central cerebral softening is characterised by the predominance, very early in the disease, of the cerebral symptoms over the general febrile condition, whereas, in typhoid fever, although there may be intense headache and delirium in the first week, these do not assume so predominant a character as in central softening.

The diseases which resemble typhoid fever by simulating one or two of its symptoms, are some forms of entero-colitis in children. In primary cases in adults, the local symptoms are disproportionate to the general febrile state. There is very little heat of skin, rapidity of pulse, no headache, delirium, or other cerebral symptoms, the stools are often slimy, with gelatinous flakes, mixed with more or less greenish fluid, but there is not the yellow or dark red fluid, and granular curdy substratum of typhoid fever. The difficulty, however, is greater in children than in adults, as in them the febrile symptoms run higher. But the skin is seldom so hot as in typhoid, nor is the prostration so extreme; the appearance of rose-spots, or sudamina, will decide the point.

[The author mentions another symptom which has been much insisted upon as a diagnostic mark, viz. enlargement of the spleen in typhoid fever. He observes that, in fact, the spleen is seldom so enlarged as to be appreciable by percussion, and he therefore does not regard it as worthy of confidence. He continues:

Finally in fixing the diagnosis of typhoid fever in adults, it should never be forgotten that it is most common under forty; and seldom seen above that age. In old people there are many febrile conditions consequent on latent and subacute inflammation, or some urinary derangement which may, at first sight, bear some resemblance to typhoid fever, but the age of the patient will be an argument against the existence of the disease.]

Medical Times. Ranking's Abstract.

On the Symptomatological Value of Palpitation. By Dr. O. B. BELLINGHAM.

Dr. O. B. Bellingham thus contrasts palpitation arising from organic disease of the heart, and that independent of it.

Palpitation depending upon Organic Disease of the Heart.

1. More common in the male than the female.
2. Palpitation usually comes on slowly and gradually.
3. Palpitation constant, though more marked at one period than at another.
4. Impulse usually stronger than natural; sometimes remarkably increased, heaving, and prolonged; at others irregular and unequal.
5. Percussion elicits a dull sound over an increased surface, and the degree of dullness is greater than natural.
6. Palpitation often accompanied by the auscultatory signs of diseased valves.

Palpitation independent of Organic Disease of the Heart.

1. More common in the female than the male.
2. Palpitation usually sets in suddenly.
3. Palpitation not constant, having perfect intermissions.
4. Impulse neither heaving nor prolonged; often abrupt, knocking, and circumscribed, and accompanied by a fluttering sensation in the præcordial region or epigastrium.
5. The extent of surface in the region of the heart, which yields naturally a dull sound on percussion, is not increased.
6. Auscultatory signs of diseased valves absent; bruit de soufflet often present in the large arteries, and a continuous murmur in the veins.

7. Action of the heart regular, irregular, or intermittent; not necessarily quickened.

8. Palpitation often not much complained of by the patient; occasionally attended by severe pain, extending to the left shoulder and arm.

9. Lips and cheeks often livid; countenance congested; anasarca of lower extremities common.

10. Palpitation increased by exercise, by stimulants and tonics, &c.; relieved by rest, and frequently also by local or general bleeding, and an antiphlogistic regimen.

7. Rhythm of heart usually regular; sometimes intermittent; its action generally more rapid than natural.

8. Palpitation often much complained of by the patient; more readily induced by mental emotion, and frequently accompanied by pain in the left side.

9. Lips and cheeks never livid; countenance often chlorotic; anasarca absent, except in extreme cases.

10. Palpitation increased by sedentary occupations; by local and general bleeding, &c.; relieved by moderate exercise, and by stimulants or tonics, particularly the preparations of iron.

[*Medical Gazette. Ibid.*]

Differential Diagnosis of Gastralgia and other more Serious Affections of the Stomach. By M. VALLEIX.

The diagnosis of gastralgia is often difficult; the slighter forms may be mistaken for the slight gastric disturbance, called by the French, "*Embarras Gastrique*," or for acute gastritis; while the ordinary chronic forms may be mistaken for chronic gastritis, simple ulcer of the stomach, cancer of the stomach, or intercostal neuralgia.

The most important distinctive signs are thus given by Valleix.

GASTRALGIA—ACUTE FORM.

Acute pain in the epigastrium.

Appetite preserved.

No headache.

Nausea only after meals, or in the morning.

GASTRIC DISTURBANCE.

Discomfort rather than pain.

Anorexia.

Headache frequent.

Nausea frequent at all periods of the Day.

GASTRALGIA—ACUTE FORM.

Appetite good.

No pain on pressure.

Vomiting rare, mucus or of food.

No fever.

ACUTE GASTRITIS.

Appetite lost.

Acute pain on pressure.

Bilious vomiting frequent.

Distant fever.

CHRONIC GASTRALGIA.

Usually uncomplicated.

Vomiting rare, mucus or food.

CHRONIC GASTRITIS.

Usually complicated with another affection.

CHRONIC GASTRALGIA.

Spontaneous pains often *very acute*.
Usually no pain on pressure.
 Progress of disease *irregular*.
Absence of fever.

CHRONIC GASTRALGIA.

Appetite *more or less preserved*.
 Vomiting *a considerable period* after food.
No vomiting of pure blood or dark matter.
 Progress *slow*.

CHRONIC GASTRALGIA.

Vomiting as before.
 Destroys *slowly*.
No signs of cancerous cachexy.
 Progress *irregular*.

CHRONIC GASTRALGIA.

Pain *not increased by pressure*
 Well-marked gastric disturbance.

CHRONIC GASTRITIS.

Bilious vomiting frequent.
 Spontaneous pains *less acute*.
Pain on pressure acute.
 Progress *less irregular*.
Fever generally present.

SIMPLE ULCER OF THE STOMACH.

Appetite *lost*.
 Vomiting *immediately* after food.
Sometimes vomiting of blood or black matter.
 Progress *rapid*.

CANCER OF THE STOMACH.

Vomiting *at long periods after food*.
 Destroys *rapidly*.
Signs of cancerous cachexy.
 Progress *regular*.

INTERCOSTAL NEURALGIA.

Pains *on pressure*.
 No gastric disturbance.

[*Brit. and For. Med. Chir. Review. Ibid.*]

Diagnostic Value of Epigastric Pains.

[According to Wunderlich, ('Handbuch der Pathologie, und Therapie,') the following points are worthy of attention, in reference to pains in the epigastric region:]

Gastric pains in persons whose digestion and appetite are unaffected, and which are not exacerbated by hunger or food, most probably depend upon the spinal cord, or on some organ adjacent to the stomach. Pains which are diminished by strong pressure may be set down to neuralgia. Pains which are distinctly increased when the hand is placed gently on the stomach, but which are not proportionally aggravated by firm or abrupt pressure, are either imaginary or sympathetic. Pains continue for days or weeks, or recurring at definite periods without any obvious cause; also pains which come on suddenly with great severity and disappear as rapidly, are probably, due to gastric neuralgia; they may depend on gaseous distension. Pains arising in the scrobiculus cordis and radiating in various directions, may arise from cardialgia, rheumatism of the abdominal walls, or peritonitis. Gastric pains, which are suspended by food, depend on neuralgic affection, or the presence of parasites. Pains which exist, both while ordinary

food is taken and when fasting, but which disappear when stimulating food or drink is taken, depend on anæmia of the stomach. Pains which are increased by the smallest quantity of food, indicate probable gastritis or degeneration. If pains supervene an hour or two after meal-time, we fear disease of the pylorus. [This assertion will needlessly alarm a large proportion of dyspeptics.—Ed.] Lancinatings are sometimes remarked in cancer.—[*Brit. and For. Med. Chir. Rev. Ibid.*]

Inverted Toe Nail—A new remedy. By BENJ. P. DRAKE, M.D.
of Lexington, Ky.

CASE. Miss A. E. T., æt. about 16, of irritable constitution, has been for the last three years the subject of inverted toe nail in the great toes of both feet. Almost every remedy that has been suggested for its relief had been resorted to, but without success. The nail had been scraped very thin, caustic had been extensively and frequently applied, the roller had been worn for months, and lastly, the diseased sides of the nails had been split down to the glands, and then, with the forceps, torn out by the roots. The relief following these severe measures was but partial and temporary, and in the course of a few weeks or months the disease was as distressing and painful as ever.

In this state of the case it occurred to me to make trial of the common tannic acid, which I applied in the following manner: I placed a portion of the tannin in its dry state on the nail, and then raising the exuberant granulations, with the tortoise-shell handle of a lancet, I gently but carefully pressed it down to the embedded edge of the nail, and filled the cavity with it, after which I covered the whole of the proud flesh with the same application, and enveloped the toe with a roller bandage. As no pain followed, the dressing was permitted to remain until the fourth day. On its removal, the improvement was so manifest that I had no hesitation in using the remedy again, which I did precisely as at first. To be brief, six or eight applications of the tannin, at intervals of three or four days, were made, and the cure appears to be complete and perfect.—[*Western Journal of Medicine and Surgery.*]

Ingrowing Toe-nail treated by Collodion.

We find in the Bulletin Général de Thérapeutique (28th Feb., 1851) a notice of the use of collodion in the treatment of ingrowing toe-nail, by Dr. Meynier, of Ornaus. The flesh be-

ing pressed aside, a little collodion is poured between it and the nail, which soon dries and keeps the parts asunder until cicatrization is effected. M. Larrey states that he has tried this plan with success in four out of six cases.

Miscellany.

AUGUSTA, April 15th, 1851.

Dr. DUGAS: Dear Sir,—With your permission, I will record the following remarkable anomaly, which your avowed penchant for monstrosities will render interesting, at least to the Editor, if not to the generality of the readers of your Journal.

Very respectfully,

HENRY F. CAMPBELL. •

Congenital Deficiency of both Patellæ.—The subject of this most unusual defect is a negro boy about twenty years of age, employed as a hand upon one of the steamboats on the Savannah river. In stature he is rather poorly developed though not much below the ordinary size. On examination of the lower extremities, we found the knee presenting a very remarkable appearance. At the joint there is a curve outwards, rendering him slightly bow-legged. In the extended position the contour of the articulation does not differ very materially from that ordinarily seen; the defect is only perceptible in this position by a manual examination, but on flexion it is at once fully apparent. There is then present on each side of the joint a large prominence caused by the projection of the internal and external condyles of the femur, which appear much larger than usual—at these points, the integument is indurated from coming in contact with the ground in kneeling; normally, this induration is central, over the lower portion of the patella and its ligament. Between the condyles, instead of the elevation ordinarily found, there is a depression in which, on the contraction of the extensor muscles, the common tendon may be felt to move under the integument. On complete flexion of the limb, the front surface of the joint becomes very much flattened, and the groove and prominences above-mentioned are still more evident. The tubercle of the tibia is somewhat larger than ordinary, and to it the ligamentum patellæ, or rather the continuation of the common extensor tendon, is attached as usual. This tendon does not differ much from the ordinary size and is accommodated by and plays in the trochlea between the condyles, which, normally, is the situation of the pa-

tella. The articulation is apparently quite as firm as usual. The boy is active and performs all the movements of the joint necessary in walking and running with the utmost facility. On the most careful comparison, we could not detect the least difference in the conformation of the two extremities.

The above case is, so far as we know, without a precedent. The mechanical relations of the patella, to the articulation, are such that in its various movements, it has been always considered of the greatest importance. Upon its integrity depends the transmission of the whole action of the powerful extensors of the leg—its fracture completely annihilates extension and progression is completely hindered. On account of this importance, it has for a great length of time been the subject of particular interest both to the anatomist and the surgeon. By the former, it has been traced through the whole vertebrated scale, and its presence or absence in various animals fully noted,* and by the surgeon its abnormal conditions have been most thoroughly scrutinized: yet by no one of the many writers on this subject can we find a case reported parallel to the above, or at all approaching it in character.

Proceedings of the Medical Association of the State of Alabama.—
Mobile, December, 1850. pp. 156.

We sincerely congratulate our friends of Alabama upon the publication of so creditable a production as that above named. These "proceedings" evince not only much zeal in the cause of science, but attainments of a high order on the part of the officers and reporters. The volume contains, the annual Address, by A. Lopez, M. D., President, and the Valedictory Address of Chas. E. Lavender, M. D., elected President for the ensuing year; also Reports, by Dr. C. F. Percivall, on the Diseases of Lowndes county; by Dr. D. P. Smythe, on the Medical Botany of Sumter county; by Dr. W. A. Welch, on the Medical Botany of Talladega; by Dr. W. H. Anderson, on the Diseases of Mobile; by Dr. H. V. Wooten, on the Diseases of Lowndesboro'; by Dr. L. H. Anderson, on the Diseases of Sumter county; by Dr. T. W. Mason, on the Diseases of Wetumpka; by Dr. W. P. Reese, on the Diseases of Lowndes county, and Papers by Dr. W. P. Reese on Marasmus, Typhoid Fever and Pertussis; by Dr. W. H.

* On this subject, the following summary is not without interest.

The patella exists in all placental animals, but is absent in many marsupials. In birds it is usually present; there are sometimes two, one placed above the other, as in the ostrich. No patella has ever been found in any reptile.

(*Todd's Cyclopedia Anat. & Phys.* Art. *Sesamoid Bones*.)

Anderson, on Cod-Liver Oil ; and by Dr. C. E. Lavender, on a wound of the Heart. During the session, a number of interesting facts were related, some of which we will reproduce.

—

Effect of the Mother upon the Fœtus in utero.

Dr. R. LEE FEARN related the following very remarkable particulars of a case, where the impressions received by a mother, during pregnancy, affected the child in utero. A gentleman, whilst gunning, shot through the metacarpal bone of the index finger. The wound was a bad one, and piece after piece of the bone came away. A few months after the accident he married, and in due season his wife bore him a child, perfectly formed in all respects. When about four months advanced in her second pregnancy, an operation was deemed necessary to remove the last remaining portion of bone in her husband's finger. She witnessed the operation, and was much shocked and sickened at the sight. When the child was born, it was found to be deficient in this very bone, though in all other particulars it was a well formed child. The Doctor thought this was by no means the result of chance, but a very conclusive instance of cause and effect.

Dr. DOSSEY remarked that the relation of this case called to his mind a similar instance :

Dr. G—— was thrown from his horse, and broke his leg, midway between the ankle and knee. His wife was about five months advanced in pregnancy. When the child of which she was pregnant was born, it had on the leg corresponding with the injured limb of the father, and at precisely the same spot, the appearance of a fracture of the limb, and there was also a very decided shortening of the leg.

—

Wound of the Heart ; penetrating the right ventricle ; from which the patient recovered. Read before the Association, by CHARLES E. LAVENDER, M. D.

James H——, student, aged 19 years, of good health and sound constitution, was stabbed, on the 9th of April, 1850, in the left breast, by a fellow student, with a pocket knife, the blade of which was about three inches long and three-fourths of an inch wide in the middle, and very narrow at the point.

When I saw him, at 4 o'clock, P. M., about five minutes after the wound was inflicted, he was laid on a long table, on his right side, with his head slightly raised. He was vomiting, with jaws rather rigid ; cold sweat on his face ; eyes drawn back, pupils much dilated ; countenance pale and deadly ; respiration irregular, interrupted and terminating in deep sighs ; action of the heart entirely suspended ; clothes dripping with blood. On tearing away the clothes from his chest, a wound presented itself in the left side, between the sternum and the nipple, about two inches anterior to, and three-fourths of an inch below the left nipple, between the fourth and fifth ribs, at the cartilaginous extremity, the greater extent of wound being between

the cartilages. The wound, from which venous blood was flowing in a full, continuous stream, was about one inch in extent, in a direction across the body; the edges of the knife having struck the lower side of the cartilage and the upper side of the rib. The cut edges of the intercostal muscles were distinctly seen, through which a dark opening, about the size of a man's fore-finger, allowed the blood to flow. One gallon and a half of blood was supposed to be lost; it could not have been less than one gallon. The right ventricle of the heart was evidently opened, and I supposed he could not live fifteen minutes.

I turned him hastily upon his back, raised his right arm, which was pendulous, and placed it by his side, dashed a large towel, just dipped in a bucket of cold water, on his chest; sprinkled cold water and spirits of camphor in his face, and secured free ventilation. The bleeding stopped instantly, but the breathing continued oppressed, interrupted, and somewhat stertorous. About five minutes after the bleeding ceased, a slight flutter was felt in the heart, and was distinctly appreciable under the palm of my hand, at irregular intervals, for a minute or more, when pulsation became perceptible, and in a few minutes more there was pulsation at the wrist. He now swallowed water, and spoke incoherently; breath during this time cold. A mattress was drawn under and blankets thrown over him, and he was kept on his back, with his shoulders slightly elevated. About 5 o'clock, he recognized persons, spoke hurriedly, called for persons, and supposed he was dying; but he afterwards remembered nothing that occurred before 6 o'clock, at which time he became exceedingly restless, complained of pain in his breast and head, with some thirst. Pulse feeble, interrupted, and over one hundred.

When the external bleeding ceased, I apprehended internal hemorrhage; but no evidence of this presented itself at that time or subsequently. About 9 o'clock, he began to grow warm. At 10, he became exceedingly restless, and complained of intense suffering, but of no acute pain. Pulse about 120, intermitting; respiration interrupted, and at times as frequent as 60 to the minute. From 12 till 3 A. M., but little hope was entertained of his living till daylight, when his nervous system yielded to the quieting influences of morphine, about two grains of which had been given, at intervals. Towards morning he enjoyed some refreshing sleep.

Fearful of a return of the hemorrhage, or of disturbance to the nervous centres, I did not allow him to be removed from the academy, where I first found him, till 3 p. m. on the following day. He was then removed to his boarding house, with such care as to cause no disturbance. He suffered somewhat from restlessness and thirst. The first was remedied by small doses of morphine, the latter, by cool sub-acid drinks. At night, he suffered from distension of the bladder; not being allowed to change position, he had not been able to empty it. Catheter was used.

11th. Passed a restless night; interrupted slumber; frequent starting; hot head; some delirium. Considerable febrile excitement through the day; skin hot and dry, but pale; countenance shrunk,

and indicative of much distress ; tongue red and dry ; pulse thready and irregular, about 120 ; complete prostration of muscular power. Lies on his back ; if turned to the right side, evinces but little pain, but soon turns back, with a sigh and heavy breathing ; if turned on the left, suffers pain in direction of the wound, is much distressed, and rolls back immediately. Bowels inactive, gave enemata. Bladder so torpid as not to expel the urine, when the catheter is introduced, without external pressure, Cooling drinks, laxatives, occasionally, small doses of morphine.

12th. Rested rather better last night. But little alteration in symptoms ; rather more thirst. Skin and pulse somewhat softened by small doses of antim. and morph. Bowels and bladder as before.

13th & 14th. Rests some better. Pulse ranging about 100, rather light ; still some starting in sleep ; respiration not so quick, but still heavy ; some light delirium ; tongue coated with whitish fur ; loathing of food ; no voluntary evacuations. Use catheter every 12 hours, and enemata occasionally.

15th & 16th. Slowly improving ; rests better. No change in condition of bladder or bowels. Use spirits turpentine, with mild mercurials, to act on secretions.

17th & 18th. Not doing so well. Constant fever ; pulse rather full, about 100 ; veins full. Can lie on neither side ; occasional pains, more or less acute, from the external wound through the chest to the spine. Some action on bowels ; bladder totally inactive, air passing in through the instrument when pressure is removed, after emptying that viscus. Gave a few grains of quinine, and small doses of morph. and ipecac.

19th. Rested pretty well last night. Fever subsided ; skin cool and soft ; moderate action on bowels. Drew off a pint of urine ; yet notwithstanding this distension of the bladder, some air rushed in when the catheter was first introduced. Tongue becoming clean, no thirst. Uses strawberries, which have constituted his only subsistence. Looks more lively ; breathes well.

20th. Improving. Wound healed ; no pain ; can lie comfortably on his right side. Some appetite ; takes tea and toast, and this day ate a young pigeon broiled. Pulse 84.

21st. Rested well, without anodynes. This day passed urine without help, for the first time. Bowels in a healthy condition ; appetite good. Sat up in a chair for some minutes, but with much fatigue. Pulse soft, 82 ; breathing good.

May 1st. Has continued to improve slowly. Sets up for hours, and walks about the house.

2d. Rode out, without fatigue.

4th. Left for home, on steamer *Isabella*.

There was a distinct bellows sound in the heart, for about two weeks, whose swells were not synchronous with arterial pulsation. This sound grew less distinct, till it was entirely lost.

I have seen Mr. H. frequently during the summer. He has been well, and is now enjoying fine health. December, 1850.

Wounds penetrating the cavity of the heart are considered, by most professional men, as necessarily fatal. In the N. Y. Journ. Med. is reported a case of wound of the heart—the patient living ten days—external wound near the sternal end of the fourth rib. On the 9th day, the patient “fell on the floor of the ward, while crossing it.” The pericardium was found perforated within the mediastinal space. The heart itself was perforated half an inch to the right of the septum; perforation passing entirely through the right ventricle, through the septum, into the left ventricle. The orifices were lined with coagulated lymph. The learned editor of the Am. Journ. of Med. Science, in commenting on this case, which he re-publishes, says: “Wounds of the heart, when penetrating its cavities, are always fatal, though the patient often lives for a considerable period after the accident.” He then alluded to large collections of cases, to establish this negative proposition—that penetrating wounds of the heart *can not be cured*. Had the case just alluded to been well managed, it might possibly have been cured; in which case, our profession never would have known it. But “he fell on the floor of the ward, while crossing it,” on the ninth day, died on the tenth, and the knife revealed the surprising fact, that both ventricles of the heart had been penetrated.

In the Journal of Medical Science, for July, 1850, there is an interesting case of wounds of the left ventricle of the heart, which, survived five days; reported by Dr. Frugien of Portsmouth, Va. A young negro man was found lying on the floor, in a state of the most profound collapse. “A wound was discovered, equi-distant from the nipple and the left edge of the sternum, and just over the left costosternal cartilage of the fourth rib. There was no hemorrhage from the wound.” “The Doctor’s first impression was that the heart had been wounded, and that the case would terminate fatally. “The arrest of the probe by the cartilage,” he says, “and its deflection to the right, caused me to come to an opposite conclusion.” The collapse was then attributed to the presence of crude, indigestible food in the stomach. The wound was received on Monday night, and the patient continued to improve till Saturday, when in disobedience of orders, “he went out, and used other improper exertions.” At 8 o’clock he died. He had been setting up a few minutes previously, and conversing cheerfully, when he sunk down from his chair and expired. Autopsy showed a wound passing through the wall of the right ventricle, without penetrating its cavity, thence through the septum into the cavity of the left ventricle. Through the opening thus made, the blood had escaped into the pericardium, until it put a stop to the movements of the heart. The wound through the pericardium had completely cicatrized, as well also as that of the heart for two thirds of its extent. Had this patient been confined on his back, and restricted to water gruel for twenty days he possibly might have lived.

It is the recorded opinion of Dorsey, Dupuytren, and others, that wounds of the heart are not necessarily fatal. But Taylor, in his Medical Jurisprudence, says, “until some clear instances of recovery from penetrating wounds of the cavities are reported, the majority of practi-

tioners will continue to look upon them as necessarily, although not immediately fatal." As *one* instance of such recovery, I offer, with some diffidence, the above case. It may not be improper to state, that the youth who suffered was, at the time, a member of my own household. I was by his side constantly, night and day, for two weeks. The facts were noted down as they occurred, with all the exactness of which I was capable. The case is deeply interesting, in many points of view, especially so in a practical one: showing, what the two cases alluded to unfortunately showed before, that, in wounds of the heart, the horizontal position should be strictly maintained, and the utmost quiet and relaxation enjoined, for at least two or three weeks after the infliction of such injuries.

Southern Medical Literature.—The following resolutions were adopted unanimously by the Association:

Dr. DOSSEY of Mobile, offered the following resolutions, and urged their adoption by many very appropriate remarks, disclaiming, at the same time, all prejudiced political or sectional feeling, averring that he was actuated solely by the hope of elevating the standard of our profession in our midst:

Whereas, the Alabama State Medical Convention, feeling deeply impressed with the importance of encouraging the growth of medical literature among us, therefore—

Resolved, That, as a body, we will sustain by patronage, and as far as we can, by articles, contribute towards building up and sustaining the Medical Periodicals of the South and South-West.

Resolved, That we deem it the duty of this Association to lend its influence and support, as far as practicable, to the Medical Institutions of our own section, believing that the facilities for acquiring a thorough medical education are equal to those found elsewhere.

Dr. PERCIVAL spoke warmly in favor of these resolutions. He thought we much needed a Home Medical Literature, and that our patients were often the sufferers, by our too closely adhering to the precepts taught in schools and books by those who know nothing, *practically*, of the diseases of the Southern country.

Dr. HICKLIN, also, spoke in favor of their adoption.

Dr. KETCHUM took this opportunity to call the attention of the Association to a work, that has directly in view the encouragement of Southern medical literature,—“Fenner’s Southern Medical Reports.” He stated the difficulties which the editor had to encounter in bringing out such a work, and the aid which it was necessary for the profession to extend towards it, to secure its success. At the close of Dr. Ketchum’s remarks, Dr. Anderson of Mobile, offered this resolution:

Resolved, That the Alabama State Medical Association highly appreciate the motives which induced the able editor of the “Southern Medical Reports” to undertake his task; and being well satis-

fied of the practical utility of the work, and the ability with which it is edited, they cordially recommend it to the faculty of the State, as a standard volume for any medical library.

Anæsthetics in Richmond, Virginia.—The committee appointed by the Medical Society of Virginia, to “enquire into the experience of the Medical profession of the city of Richmond, in regard to the utility and safety of anæsthetic agents,” have recently made a very elaborate Report, which is inserted in the “Stethoscope,” a valuable medical journal, now published in Richmond.

We are informed that anæsthetics have been administered since their introduction in Richmond to 1384 persons, viz: by Physicians, Chloroform to 291 persons, Ether (sulphuric, we presume. Ed.) to 99, Ether and Chloroform to 10. By Dentists, Chloroform to 423, Ether to 556.

“Three cases have been reported, in which fatal or permanently injurious consequences have been *suspected*; but in all of these, there were other palpable causes quite sufficient of themselves to have produced the results. That the anæsthetic agent used, even *co-operated* with these causes, has not been satisfactorily shewn; and without this evidence we cannot admit a conclusion opposed by all the other evidence which we possess.”

From the facts presented, the committee deduce the following propositions:

“1st. Of nearly fourteen hundred instances reported, in which anæsthesia has been produced, not one has occurred in which either a fatal or permanently injurious consequence has been proved to have resulted.

“2d. That on every occasion on which it is desirable to use anæsthetic agents, we may do so with confidence, observing proper precautions.

“3d. That chloroform is preferable to ether, and is equally safe. We would compare its advantages to those of the alkaloids, quinine, morphine, &c., over the bulky and often nauseous substances from which they are derived.

“4th. That in surgical operations the patient is not only saved the cruel agony which has hitherto been inseparable from many of them, but is in a more-favorable condition for their successful performance.

“5th. That the process of natural labor is facilitated by anæsthesia.

“6th. That in some cases of a purely medical character, these agents furnish a most valuable resource to the physician.

“7th. Finally, when we consider the extensive application of these agents, the diminution of suffering and the preservation of life which they have effected, and the relief from embarrassment to the operating

surgeon which they afford, together with their safety, they deservedly rank among the most valuable resources of the healing art, and their discovery marks an important era in the history of medical science."

Intemperate Use of Chloroform.—We find in the body of the Report a case of such novelty that we transcribe it in full :

"J., about sixty years of age, blacksmith, of fine athletic form, had enjoyed such uninterrupted good health, that he had not been known to lose a day's work during twenty years. It was his habit to work every day, Sundays included, until a late hour of the night. Every Saturday night he stopped work earlier than usual, and indulged himself in a frolic. About three years since, his mind became suddenly disordered while he was at work in his shop. He was totally incapable of applying himself to his usual employment, and imagined that he had been poisoned. He returned home and went to bed, from which he has never risen since, except for a short period.

"While under medical treatment for this hypochondriacal condition, it was suggested to him to use ether, which had then been just introduced, for the purpose of producing anæsthesia. A few trials were not satisfactory to him, and when chloroform was introduced, he soon substituted it for ether. Since then, he has continued to use it to an enormous extent. He has often inhaled a pint in twenty-four hours. On one occasion, his son left in his room a pound, which he had just purchased. On returning home six hours after, he found the bottle empty. On enquiry, his father assured him that he had inhaled the whole of it, and entreated him for more. Fearing that it would prove fatal, he refused to procure a further supply, until after an interval of about twelve hours, when his father's entreaties became so importunate that he yielded, and during the remaining six hours, the old man inhaled ten ounces more, amounting in all to 26 oz. in 24 hours. Probably, however, much of this wasted.

"One apothecary of the highest character testifies, that he has supplied him with more than two hundred pounds, and that he has not sold him any for a considerable time. His son declares, that his father's use of chloroform has consumed the greater part of the earnings of himself and brother, in one of the largest and most profitable shops in the city. He supposes they have expended at least twenty-five hundred dollars in this way during the last three years. Lately, they have succeeded in reducing the amount used, to four ounces in three days.

"It is remarkable, that during the period that he has been using chloroform, he has entirely abandoned the use of ardent spirits.

"About the middle of last February, two members of the committee visited the patient, in company with his son. He was found occupying an attic room, lying upon a pallet on the floor, in compliance with his own wishes. His appearance was that of a hearty, fleshy man, of about sixty years of age. His pulse, respiration, in fact all his functions, we ascertained were perfectly healthy. His appetite

and digestion were remarkably good. During this inordinate use of chloroform, he has fattened probably thirty pounds, his weight being now about one hundred and eighty pounds.

"He has never been unpleasantly affected in any way, either during anæsthesia or afterwards, except once, when, having become insensible, his head fell upon the inhaler. Then, a more profound state than usual, marked by stertorous respiration, was produced, but it was of short duration.

"We remained in the room about have an hour, conversing with him most of the time, and were several times interrupted by his urgent entreaties for more chloroform, although he had just emerged from the anæsthetic state. His remarks were chiefly on the subject of his having been poisoned, which is evidently his principal illusion. He imagines himself unable to walk, and refuses even to be dressed.

"At length his desires were indulged; and an ounce vial, half full of chloroform, was brought to him. He eagerly grasped it; and having drawn the bed-clothes over his face, sufficiently to cover his mouth and nose, he placed the vial to his lips, and took strong, deep inhalations for ten or fifteen minutes. A slight quivering passed over his frame, he rolled upon one side, and lay in a state of profound sleep. We then left him to his strange infatuation.

"This case proves conclusively, that the intemperate use of chloroform is attended with far less danger than is the same use of alcohol or opium. It is a remarkable fact, that in this case it has not been necessary to increase the dose, which would have been required, had any known stimulant or narcotic been used, instead of chloroform."

History of the Southern Medical and Surgical Journal.—Judging from the many changes in the Editorial management of the Southern Medical and Surgical Journal, its career would seem to have been one full of vicissitudes; yet its career has been eminently successful. This Journal owes its origin to the indomitable energy of the late Professor Milton Antony, the illustrious founder of the Medical College of Georgia, who, after having established this school upon a permanent foundation, determined to encourage Medical Literature in the South by furnishing a suitable vehicle for the record of facts and observations. Overcoming all the obstacles to such an undertaking, he associated with himself, in the Editorial chair, Dr. Joseph A. Eve, (the present able Professor of Obstetrics,) and published the first No. of this Journal on the 1st of October, 1836. It was issued, as it is now, in monthly Nos. of 64 pages each, making an annual volume of nearly 800 pages. The monthly form was very properly deemed the most convenient for the active practitioner of medicine. Prof. Joseph A. Eve withdrew from his editorial connection at the termination of the 1st volume, and the 2d and 3d vols. were edited by

Prof. Antony alone, whose lamented death occasioned a suspension of the publication until the 1st of January, 1845, when it was resumed by Professors Paul F. Eve and I. P. Garvin. These gentlemen conducted it jointly during three years; with the last No. for 1847 Prof. Garvin retired, left it in the hands of Prof. P. F. Eve two years, and then resumed its supervision alone in 1850. This Journal has therefore been edited during the issue of the nine volumes, (old and new series,) preceding the present one, alternately by Professors M. Antony, Joseph A. Eve, Paul F. Eve, and I. P. Garvin. (That these frequent changes were the result neither of fickleness nor of ill success is abundantly established by the progressive increase in the number of subscribers, and the high estimation in which the work has been held throughout our country. The true cause is to be found in the vast amount of labor required for its creditable management, and in the extensive professional engagements of the parties. No one who has not tried it can justly appreciate the task of editing such a periodical, and of attending at the same time to the harassing duties of a large practice.)

The multiplicity in our land of medical periodicals is regarded by many as a decided evil. This, however, is a great error. Every new medical journal increases the number of readers as well as of writers. When the Southern Medical and Surgical Journal was first issued, it was rare that the voice of a Georgian was heard upon medical topics. By a reference to the original communications it contains, we find that they number 412, and that they were written by 146 different physicians, the large majority of whom are Georgians, and the remainder from the adjacent States. With such facts before us, we feel that the career of our Journal must continue to be one of progressive prosperity and usefulness. As a native of Georgia, we feel proud of her rapid advance in refinement and science; as the Editor of this Journal, we desire to honor those who have by their contributions made it what it is. We therefore beg leave to subjoin a list of their names. If any be omitted, we hope to be corrected.

Antony, M.	Bailey, D. F.	Branch, Franklin
Antony, E. L.	Baldwin, A. C.	Bignon, H. A.
Anthony, J. M.	Barratt, J. P.	Brandon, D. S.
Arnold, R. D.	Burt, W. M.	Cotting, J. R.
Arnold, A. B.	Baker, E. L.	Cunningham, A.
Barrett, C. B.	Barr, F. W.	Chase, H.
Bowen, J.	Barton, E. H.	Cunningham, S. B.
Bean, A.	Beal, L. B.	Cohen, L. L.
Bacon, J.	Blackburn, J. C. C.	Carpenter, W. M.

Campbell, H. F.	Hook, E. B.	Nicoll, A. Y.
Cumming, W. H.	Hamilton, T.	Nott, J. C.
Carter, T. W.	Hammond, J. F.	Norwood, W. C.
Colley, F. S.	Hard, D. B. W.	Nisbet, R. D.
Campbell, R.	Hitchcock, C. M.	Oakman, E. H.
Connell, A.	Hammond, A. L.	Oliver, J. H.
Cooper, G. F.	Harris, J. C.	O'Keeffe, D. C.
Cullender, G. D.	Harriss, J.	Pendleton, E. M.
Casey, H. R.	Harper, P. W.	Pearson, B. H.
Dugas, L. A.	Johnson, W. J.	Parker, W. P.
Delony, E.	Johnson, N. B.	Quintard, C. T.
De Saussure, H. W.	Jones, W. L.	Robertson, F. M.
Davis, Jno.	Jeter, H. M.	Robert, W. H.
Eve, J. A.	Kollock, P. M.	Richardson, C. P.
Eve, P. F.	Kirkpatrick, A. R.	Robertson, J. J.
Eve, E. A.	Keckely, E. C.	Riordon, J.
Erskine & Sheffey,	King, W. N.	Rossignol, H.
Ford, L. D.	Lee, W. M.	Ramsay, H. A.
Fort, Tomlinson	Lee, J. F.	Strobel, B. B.
Garvin, I. P.	Lake, J.	Simmons, T. Y.
Grant, G. R.	Levert, H. S.	Shephard, C. W.
Gorman, J. B.	Le Conte, Jno.	Smith, G. G.
Geddings, E.	Little, R. E.	Stevens, J. P.
Gamble, S. D.	Leak, W. W.	Sheffey, L. B.
Groce, B. W.	Long, J. A.	Summer, W. J.
Gordon, J. M.	Long, C. W.	Tufts, J. B.
Gardner, J. M.	Le Conte, Jos.	Tutt, G. M.
Greene, A. B.	Longstreet, A. P.	Twiggs, J. D.
Gaither, H.	Mayes, J. A.	Wilde, R. H.
Girardey, E.	Martin & Smith,	Whitridge, J. B.
Green, J. M.	Melchoir, R.	Wildman, P. H.
Gautier, W. J.	Meals, H. H.	Wooten, H. V.
Holloway, G. K.	Moore, R. D.	West, C.
Heustis, J. W.	Miller, H. V. M.	Wooten, G. H.
Hook, D.	Macon, E. H.	Westmoreland, J. G.
Haynes, C. A.	Meek, S. M.	Williamson, W.
Harden, J. M. B.	Means, A.	Warren, R. L.
Hammond, D. W.	Mastin, C. H.	Wilson, J. S.
Holman, J. C.	McJunkin, D. W.	Word, R. C.
Holliday, J. S.		

Occlusion of the Vagina.—Dr. P. C. Spencer, of Petersburg, Va., reports in the April No. of the "Stethoscope" an interesting case of occlusion of the vagina consequent upon sloughing after parturition, in which he successfully restored the canal by excision of the cicatrix. The female has since borne a child without farther accident.

Dr. A. G. Mabry related to the Medical Association of Alabama,

a similar case in which "the Doctor commenced the treatment by making several incisions into this body" (an unyielding cicatrix) "and introducing bougies, but owing to some bad management the incisions healed up, and it was necessary to repeat the operation again and again. After several operations, a bougie was introduced and kept in its proper place by a T. bandage, which eventually made a perfect cure. (Proceedings of the Med. Association of the State of Alabama, December, 1850.)

Dr. Debrou reports in the *Gazette Médicale de Paris*, 18th Jan., 1851, a very remarkable case of congenital occlusion of the vagina and os uteri, in a female 19 years of age, which was successfully relieved by incisions and tents. The female subsequently married and bore a child, but died of puerperal metritis on the 10th day after delivery.

Chloroform in Infantile Convulsions.—The New-Hampshire Journal of Medicine reports a case of obstinate convulsions occurring in a child affected with an enormous hydrocephalus. After failure of the usual means, chloroform was administered by inhalation, promptly arrested the convulsions, and saved the patient's life. The case was treated by Dr. N. Martin, of Dover, N. H.

Action of Ergot in producing Retention of the Placenta.—Dr. Charles Hasbrouck, of Rockland county, New York, relates in the New Jersey Medical Reporter, several cases tending to illustrate his belief that the administration of ergot during parturition may occasion a retention of the placenta. The subject is new, we believe, and certainly well worthy of further investigation. We have not room for the entire article, but will simply add the author's preliminary remarks:

"Spasmodic contraction," or "premature contraction" of the cervix uteri, is referred to by obstetrical writers as an occasional cause of retention of the placenta; and Dewees, in his system of midwifery, mentions the fact, that in some instances of this character, the body of the uterus is also found to be "hard and well contracted." But so far as my information extends, the frequent relation between this condition of the uterus and the administration of ergot during labor has never been pointed out. The following cases, it seems to me, are calculated to show that such relation does occasionally exist; that the placenta may, perhaps, not unfrequently, be retained by the permanent and uniform contraction of every part of the uterus, excited by the specific action of ergot upon that organ.

Extraordinary Tapping.—Dr. T. D. Lee, of New York, reports a case of ascites, in which he performed paracentesis abdominis 39 times and drew off 141 gallons of water! The case terminated fatally. We recollect removing 10 gallons at one tapping, in a case of encysted dropsy, and repeatedly afterwards 6 gallons, by which the life of the patient was prolonged about 18 months.

North-Western Medical and Surgical Journal.—Dr. Edwin G. Meek has retired from the co-editorship of the North-Western Medical and Surgical Journal, and is about to migrate to California. He will carry with him the best wishes of the fraternity. The Journal will continue under the able management of Prof. Evans.

The Ohio Medical & Surgical Journal.—This valuable bi-monthly, heretofore edited by Dr. S. H. Smith, is now conducted by R. L. Howard, M. D., Prof. of Surgery in Starling Medical College. We feel assured that the professor will sustain himself ably in his new position, and cordially welcome him into the editorial corps.

Dr. Jenner.—The British government is about to erect a monument in honor of Jenner. Better late than never.

Medical men going to Enrope.—We learn that the following distinguished physicians are to visit Europe this season: Professors Silliman, father and son; Professors J. B. S. Jackson and H. J. Bigelow, of Boston; Prof. R. L. Howard, of Starling Medical College, Ohio; Prof. White, of Buffalo.

Inoculation in Rubeola. By JOHN E. MCGIRE, A. M., M. D., L. L. D., Professor of Chemistry, Physiology, &c., in the University of St. Mary's, Physician to the Catholic Male and Female Orphan Asylums, Chicago.

Inoculation in Rubeola is no new experiment. As to the advantage of the process, diversity of opinion exists. Drs. Home, in Edinburg, Dewees, and Chapman, at the Dispensary in Philadelphia in 1801, practiced inoculation without any satisfactory results, while the experiments of Prof. Speranza of Mantua, and others, were varied, decisive and successful. Having no opinion of my own to confirm, wishing only to arrive at the truth, if possible, I determined when the very favorable opportunity presented, by the breaking out of Rubeola in these Asylums, to test this point. The Asylums are situated, (the female in north, and the male in south Chicago,) without the thickly settled portion of the city, having the advantage of healthy locations. The houses are large, well ventilated, and are under the charge of the Sisters of Mercy; thus the best nursing could be secured, and the best opportunity which might ever again occur to me of watching every

stage of the progress of the disease. Early in December the first case of measles was brought into the female asylum. I proceeded to inoculate from this case, when the eruption was at its height. Blood was drawn from a vivid exanthematous patch on the diseased child's arm, and inserted into the arms of the three children first mentioned in the list below. On the fourth, sixth and seventh day, after the inoculation, the measles appeared, pursuing a regular and mild course. The result of these cases determined me to carry the experiment farther, and that the trial might be a fair one, I selected for comparison those whose physical conformation and constitutional idiosyncrasy, seemed most nearly alike, giving the disadvantage of age to the inoculation. The following table contains the names, ages, and results of all the cases whether inoculated or not:

NOT INOCULATED.		INOCULATED.	
Died	Age.	Recovered.	Age.
Ellen Brown,	3 yrs.	Ellen Kehoe,	11 yrs.
Katy Russell,	2	Ellen Grant,	4
Philomena Kehoe,	3	Mary M'Carty,	8
Elizabeth Patton,	2	Rose Mack,	5
Ellen Crowley,	5	Mary Grant,	9
Recovered.		Eliza Hurley,	4
Mary Carroll,	9	Ann Cahill,	8
Ann Brennan,	6	Ella Welsh,	5
Mary Patton,	7	Ann Mulhall,	9
Johanna Cahill,	5	Ann Hagan,	3
Emeline Hurley,	4	Mary Mulhall,	4
Mary Nugent,	5	Ellen McCarty,	10
Mary Brain,	10	Anna O'Brien,	13
Elvira Gilmartin,	5	Cath. Power,	9
Fanny Mooney,	12		
Mary Ann Tell,	10		

This table gives us 29 names, 24 recoveries and 5 deaths, all occurring among those not inoculated. The cases of all those inoculated, commencing from the fourth to the ninth day after inoculation, proceeded regularly, with the ordinary symptoms of simple measles, to convalescence, which was speedy and complete, with one exception viz., the first case. This child entered the asylum about a year ago, suffering with violent ophthalmia. She had been cured. On the disappearance of the measles, the ophthalmia returned, and though the sight was much endangered, yet there now only remains a little weakness which is disappearing. All these cases occurred consecutively from the first week of December to the second week of January.

Four children who were known to have had measles in the spring of 1850, were inoculated; nothing else was observed than the inflammation which would follow any ordinary lancet puncture.

Of those not inoculated with four exceptions, the antecedent symptoms were very severe. The fever was violent; distressing vomiting occurred in three cases. The catarrhal symptoms were violent;

throat sore, hoarseness, rigors, cough almost continuous, dry, the whole chest sore, difficult respiration, delirium at night in some of the cases.

Four had the "congestive modification," the eruption appeared slowly and imperfectly; one of these died. Two others presented the Typhoid variety; one died of diarrhœa, the other recovered, but afterwards four dangerous ulcerations appeared on the limbs, and gangrenous stomatitis, in the left lower jaw. All of the teeth of that part of the jaw, fell out, the left side of the tongue and the cheek were involved in the disease. This case ultimately recovered. Bronchitis supervened in six cases. Three had partial aphonia, one complete; this one died.

When these last mentioned cases attempted to swallow any liquid, it was thrown back through the mouth and nose with violent expulsive effort.

In the male Asylum, there were 23 cases and 6 deaths. None were inoculated, but 3 of the whole number had the disease mildly, and these were the three first attacked. The others had violent antecedent symptoms, and tedious convalescence. Five of those who died had the aphonia and difficult deglutition before spoken of, the other died of Phthisis.

In Review of these facts much might be said. I have chosen, however, to give them as they occurred, without comments, leaving to the readers of the Journal, to estimate them at what they are worth; merely adding, *that if there is no advantage in inoculation, the result which the second column furnishes, would be a strange anomaly.*—[North Western Medical and Surgical Journal.

Anatomical Dissections in the State of New York.—The following Act was lately introduced into the New York legislature by Dr. Tuthill, of Suffolk, and will probably become a law of the State. It is copied, nearly verbatim, from the act, in our own State laws, relating to the practice of physic and surgery.

"Section 1. Any physician or surgeon duly qualified according to the law of this State, or any medical student, under the authority of any such physician, may have in his possession, human bodies, or parts thereof, for the purposes of anatomical inquiry or instruction.

"§ 2. Either of the following board of officers, to wit: the overseers of the poor of any town of this state, and the commissioners of health, the city inspector, or the mayor and aldermen of any city of this state, may surrender the dead bodies of such persons, as are required to be buried at the public expense, to any regular physician, duly qualified according to law, to be by him used for the advancement of anatomical science; preference being always given to the medical schools, by law established in this state, for their use in the instruction of medical students.

"§ 3. No such dead body shall in any case be surrendered, if the deceased person, during his last sickness, requested to be buried, or if, within twenty-four hours after his death, any person claiming to be

of kindred or a friend to the deceased, and satisfying the proper board thereof, shall require to have the body buried, or if such deceased person was a stranger, or traveller, who suddenly died before making himself known; but the dead body shall in all such cases be buried.

“§ 4. Every physician shall, before receiving such dead body, give to the board of officers surrendering the same to him, a sufficient bond that each body, so by him received, shall be used only for the promotion of anatomical science or instruction, and that it shall be used for such purpose within this state only, and so as in no event to outrage the public feeling.”—[*Boston Med. and Surg. Jour.*

Alleged Uncertainty in Medicine.—Dr. Thompson, President of the New York State Medical Society, in an address at the last annual meeting of the Society, thus compares the medical with the other professions in regard to certainty in its results.

“When compared with the other professions, its character is vindicated, and it rises in our estimation, for it may be questioned whether the results of its practice are any more doubtful than those of the law, which also has been stigmatized for its ‘glorious uncertainties.’ In a subject like theology, which treats of man’s eternal destiny, concurrence of opinion might be expected. Its doctrines, however, find no more agreement among theologians and polemical writers than do the accredited principles of medical science among well-informed and cultivated medical men. In the science of politics, in the laws which regulate the commerce and mutual intercourse of nations, in the systems and processes of agriculture, in the arrangement of society, and its government by laws whose principles and modes of action shall prove most successful in directing its interests and regulating its business affairs, men differ widely in their judgments and the greatest dissimilarity of opinions prevails. Why, then, amid this discrepancy of judgments, and difference of opinions, should medicine be held up above all, conspicuous for its uncertainties, or for its lack of settled principles?”—[*Ibid.*

On the Extraction of Needles.

To the Editor of the Boston Medical and Surgical Journal.

SIR,—Much has lately been said on extraction of needles in the flesh, and I am going to throw in my mite while the subject is up. I have seen much mischief in cutting for a needle, or a portion of one; more, in my opinion, than by leaving it to itself; for it has never been my misfortune to meet with a case where the foreign body did not find its way to the surface, and then it is easily extracted. The inside of the hands and soles of the feet are the parts most liable to be injured; and if the surgeon begins his search in these situations in the expectation of finding what he is searching after, he will in most instances be disappointed. The tendons, fascia, muscles and nerves of the hands and feet cannot fail of being much injured, and the surgeon will have the credit of causing the injury. I have had many

patients present themselves to me with such accidents—some of them very much frightened, and insisting on having a search made until the foreign body was found. Or they had already undergone an operation, with extensive incisions which exposed the tendons and ligaments, making a trivial thing one of a serious character.

One invariable rule should guide the surgeon in such cases. When the foreign body can be seen or felt, no difficulty will be experienced in its extraction. A pair of closely-fitting forceps, after cutting down on to where you have fixed it with our fingers, will generally do it without trouble. But in all cases where it is *out of sight*, and cannot be *distinctly felt*, let it alone, quiet your patient's fears by assuring him that no harm will result, and you will not have cause to regret the course. I never used the scalpel in a case where the needle was not in sight nor could be felt, but what I regretted it; and, what is of more consequence, the patient did the same. On the other hand, I never left one to nature, but both patient and myself were glad in the end.

Syracuse N. Y. March 12, 1851.

A. B. SHIPMAN.

Swallowing a Handkerchief.—In the Provincial Surgical and Medical Journal, is the report of a case of a boy, who actually *swallowed a silk handkerchief* nearly a foot square. On the third day it was dejected from the bowels, perfect in every respect, except a slight discoloration. Probably this is the first instance in which a human subject has been thoroughly *wiped out* with a silk handkerchief. It is said that the boy was subject to epileptic fits, and imbecile in his intellect; and although not cured, was not rendered any worse by this extraordinary operation.—[*Boston Med. Jour.*]

Pension to the Widow of the late Mr. Liston.—The Government, as no doubt most of our readers have observed, has granted to the Widow of the late Robert Liston a pension of £100 per annum. While it is a subject of regret that Mrs. Liston should require such an addition to her income, it is gratifying to know that the just claims to such aid by the widow of one of the greatest of modern surgeons have been thus acknowledged by the Government.

Though almost a solitary instance of the kind, it must be satisfactory to the great body of the surgeons of this kingdom to find, even in a single instance, that the labors of one of their brethren have been, even to the extent mentioned, recognized as a national benefit.—[*London Lancet.*]

Insanity caused by Tape-worm, and cured by Kousso.—Dr. Wm. Wood gives an interesting case, in the London Lancet, of a man, confined in the Bethlem Hospital on account of insanity, who became completely restored to his health and reason, after a tape-worm had been discharged by the administration of that celebrated anthelmintic kousso.—[*Boston Med. and Sur. Journal.*]

SOUTHERN MEDICAL AND SURGICAL JOURNAL.

Vol. 7.]

NEW SERIES.—JUNE, 1851.

[No. 6.]

PART FIRST.

Original Communications.

ARTICLE XVIII.

Remarks on Typhoid Fever. By J. A. LONG, M. D., of
McMinn County, Tennessee.

I do not propose writing a treatise on Typhoid fever, but simply to record my own observations and experience in this disease. Having no theory to support, or controverted points to settle, I shall simply state facts as they have occurred to me in practice from time to time. Since the spring of 1844, (at which time I entered the practice of medicine,) typhoid fever has become more prevalent, and periodical fevers less so. In proportion as the former has increased, the latter have diminished in frequency, until the past year, which was a productive one of typhoid fever. Periodical fevers (intermittents and remittents) were almost unknown in this region of country during that year. Typhoid fever prevails in this section in an endemic form, raging in certain localities, or on some particular water course, whilst the adjacent country is entirely free from its ravages. It is a disease that prevails at all seasons of the year, but is most common here in autumn and winter, the gravest cases occurring in cold and damp seasons. It attacks families, and even whole settlements, without any known or appreciable cause. In some households, one is taken down after another in succession, until every form and stage of the disease can be seen at the same time, and in this way is sometimes prolonged in the family from three to four and even six months at a time.

The question whether typhoid and other forms of fever belong to one and the same great family of diseases, or are distinct forms of disease, is an important one, and I think not yet satisfactorily settled. It is one that I do not intend on the present occasion to discuss, but simply to state facts in relation to this fever. There has been much said in this country about the name of this disease—and some of our oldest and most experienced physicians still cling to the names of *nervous*, *winter* and *typhus* fever, whilst the community in general believe it to be (from its name, typhoid fever) a new disease that has appeared among us, as it is so much more common than in former years. As I intend to write nothing except what has fallen under my own observations, I shall pass by all those discussions of the French and other writers in regard to the different appellations that have been given to this disease by different authors. I will only mention (for the benefit of those who have not had an opportunity of consulting Dr. Bartlett's work on Typhoid and Typhus fevers) that it has been called *entero-mesenteric*; by Petit and Serres, *dothinenterite*; by Bretonneau, *follicular enteritis*; and *abdominal typhus* by others. All these appellations going to point out the peculiar lesion or affection of the alimentary tube in this disease, and principally that of the small intestines, which I will notice more fully hereafter. But the term Typhoid fever, in this section, is coming into general use; all others, as *nervous*, *typhus*, *winter*, *continued* and *slow* fevers, going into disuse. I would here remark, that I am not entirely settled in my own opinion whether that group of symptoms which makes up the disease in question is really and strictly a fever, or whether it should not be classed, as above, with the diseases of the alimentary canal. The rise and progress of the disease; the peculiar character of the pulse, as is also noticed in other affections of the bowels; the local symptoms so early noticed in nearly every case referable to the *ileac regions*, and especially to the *right ileac fossa*; the great danger of relapses from taking solid food and other articles of diet after convalescence—all point to the latter organ as the seat of the disease. Why, then, not as well call diseased action in any other part of the body giving rise to fever, a *fever*, (as well as the one under consideration,) and not an inflammation, with its appropriate

appellation according to its locality? I am fully persuaded that the treatment in this disease would have been doubly successful if it had been known under some title pointing out more fully the disease of the *small intestines*: then the disease would have been prescribed for, and not the name, as I fear is too often the case in the present day.

The result, in this section of country, of a treatment with calomel and quinine is almost universally fatal. Typhoid fever generally comes on slowly and gradually. So uniform is it in this particular mode of access, that it is one of the principal features in its diagnosis. I cannot at present call to mind a single well marked case of typhoid fever in which the onset was sudden or violent, though occasional attacks of this kind may, and probably do, occur from modifying causes, or from unusual predisposing states of the system. I have almost uniformly been told on my first visit to patients that they had felt unwell for several days, or perhaps weeks, and indisposed both to bodily and mental exercise of any kind. They were, however, unable to tell in what their disease consisted, more than weakness, or a general sense of languor, disturbed sleep, &c. A dull headache accompanies or succeeds the above premonitory symptoms, this being preceded by a chill or a sense of coldness, with fever, dryness of the mouth and fauces, with the tongue more or less furred, dry, and occasionally cracked; surface dry and warm, high colored urine, &c. The febrile symptoms in typhoid fever are rather of a low grade or type, with slight exacerbations and remissions at some period during the twenty-four hours. Generally I have found the pulse somewhat accelerated at night, accompanied with restlessness and want of sleep, even in mild cases—watchfulness, jactitation and delirium, in grave ones. One of the most constant and characteristic symptoms of typhoid fever is *diarrhæa*, and this symptom is to be found as well in mild as grave cases. It is, however, occasionally absent in both the former and the latter, being uniformly present in cases of medium severity. This symptom, I believe, is always accompanied with more or less abdominal soreness, particularly if pressure be made upon the right *ileac* region. Most generally the tenderness is accompanied with a gurgling noise in this part of the abdomen. The latter symptom I have

sometimes detected even before diarrhœa had set in. My patients have generally complained of fulness and a dull aching in the abdomen, sometimes, but rarely amounting to choleric pains. Some writers, as Wooten of Alabama, have divided this disease into two general classes, *nervous* and *mucous*, according as one or the other of those great systems suffered most, or as the mucous or nervous symptoms predominated in the disease. He also gives us a set of mixed cases, where both the mucous and nervous systems suffer equally; and here it is, he says, we are most apt to meet with those grave cases which, unfortunately for humanity, are too often to be seen in this as well as other regions of the globe. It is true, that the *cerebro-spinal* system, as well as the *mucous*, suffers greatly in this disease; but in this country those two classes of symptoms are so uniformly blended in almost every case, that a division of this kind would be of but little practical importance. It is equally true, by hair-splitting distinctions, that one or the other set of symptoms (mucous or nervous) will be found to predominate in nearly every individual case. This I attribute more to differences in constitution, age, sex, habit, temperament, &c., than to any particular variety of the disease. Typhoid fever is truly a *nervous* disease, as is manifested by general weakness, headache, delirium, loss of vision, deafness, ringing in the ears, somnolence, vigilance, jactitation, muscular prostration, &c.

Epistaxis is a pretty constant symptom in this fever, as well as occasional hemorrhages from other parts of the body. The delirium is of the low muttering kind, attended with watchfulness, jactitation, and picking of the bed-clothes, though occasionally it is wild and furious; the patient rising from his bed, striking at his attendants, or pulling at, scolding, or menacing some imaginary object about his bed. I have seen them even leap from their beds, and traverse the room in which they lay, giving considerable resistance to those who attempted to oppose them. Patients can in general be easily aroused when distinctly spoken to, and will then answer questions in relation to their situation or feelings perfectly rationally, but as soon as left alone will sink back into their former state of stupor, saying nothing except what is forced from them by repeated questions.

They generally reply that they feel better; nothing is the matter—they are well, and so on. The pulse in this disease is peculiar and characteristic—it is small, quick and frequent, having a kind of double beat, or, as Dr. Bartlett calls it, the *besfereus pulse*. So constantly have I found this pulse in Typhoid fever, and so seldom in other diseases, that in a locality where this disease is prevailing, I can almost make out my diagnosis from this symptom alone. I generally find the pulse to range from 90 to 110 in men, and from 100 to 120 in women, in common cases. When uncomplicated with other diseases, the prognosis is for the most part favorable; but in grave cases, or where some local disease is present, the pulse ranges higher, from 110 to 120 in males, and 120 to 140 or 150 in females. In many cases of typhoid fever, from the extreme nervousness or twitching of the tendons, (*subsultus tendinum*.) the double beat of the pulse, or apparent reaction after every pulsation, and its almost unparalled frequency, renders it impossible to count the pulse with certainty in this disease. I have seldom seen nausea or vomiting in this disease, or even much complaint of feeling pain or other disagreeable sensations in the epigastric region. Much has been said, and many contradictory statements have been made, about the state and condition of the tongue, some asserting that this organ differed but little from its natural and healthy state in every stage of the disease, whilst others contend that the tongue exhibits various unhealthy appearances. I have always found the tongue more or less coated with a white, yellow, or brown fur—moist, dry, cracked, or swollen, according to the severity of the disease. The appearance of the tongue indicates to the experienced practitioner the extent of disease in the alimentary tube. When there is much soreness of the abdomen on pressure, accompanied with obstinate diarrhœa, the tongue is found swollen, with a dark brown or black coat, with its tip and edges more or less red. I am ready to admit, however, that in mild cases the tongue shows only slight deviations from its natural appearance. Some patients are so slightly attacked that they never take their bed, whilst others are rapidly hurried to a fatal issue. At other times (and this is more commonly the case) the disease is slow, tedious and lingering in its nature,

and sometimes many days, and perhaps weeks, elapse and pass off, leaving the friends still in a state of restless suspense as to whether life or death will finally gain the ascendancy.

Typhoid fever is confined to the young and middle aged, or to those between the ages of 10 and 30 years, rarely attacking those younger than 10 or over 30. This disease is evidently contagious. This is strikingly true in the grave forms of the disease, where large families are crowded together in small apartments, illy ventilated, and where it is impossible to observe cleanliness as it should be done in a sick chamber, by removing all unnecessary furniture, clothing, and the discharges of the patient, requisites so essential to the promotion of health and avoidance of disease, not only in typhoid fever, but all other diseases incident to the human family. I have usually found that the first cases that occurred in a family or neighborhood were generally the severest. It is unlike periodical fevers in this respect, as the latter generally come on in their mildest form, gradually growing severer with the advancement of the season. I would further remark, in proof of the contagious nature of this disease, that I have never seen the same individual suffer more than once from typhoid fever, notwithstanding I have seen the disease in the same family at different times. Such as had suffered from previous attacks ran no more risk than those who had suffered from measles, scarlatina, or variola, and were afterwards exposed to them. There is so much to be learned in typhoid fever, from the physiognomy that I am always anxious on my arrival at each visit to catch a glimpse of my patient's countenance, which never fails to make a decided impression on my mind, before any further examinations are made or questions are asked. A rose-colored *eruption* is spoken of by almost every writer on this disease, but it is seldom seen unless looked for at a proper stage of the disease. This eruption is most apt to be found upon the anterior portions of the chest and abdomen, but occasionally it is thinly scattered all over the body. In a few of my grave cases (I find by consulting my case-book) I have encountered some very obstinate *eschars* or *bed sores*, mostly on the hips and sacrum. I have also seen deep ulcerations, or obstinate sores, produced from blistering in typhoid fever. There is generally slight cough,

with little or no expectoration, though occasionally it becomes troublesome, with free expectoration streaked with blood, or of a rust color. This expectoration is accompanied with dulness on percussion of the chest and other symptoms, denoting more or less congestion of the lungs or pneumonitis. The *diagnosis* in this disease is attended with some difficulty in sporadic cases, but in seasons remarkable for the prevalence of typhoid fever, its mode of access, its attacks on whole families and settlements, raging mostly in an endemic form, its slow and tedious nature, attended with a diarrhœa, render the diagnosis comparatively easy. The discharges are generally watery, of a green or dark green color, without any traces of bile in them, and mostly without smell.* When the above symptoms are accompanied with fever, a quick, frequent and small pulse, with a double or reacting beat, ranging from 90 to 140 beats in a minute, soreness on pressure over the right *ileac region*, with a gurgling noise, occasional tympanitis, with an unusual degree of weakness, are found in the same patient, they are sufficient to establish a case of typhoid fever. The *prognosis* in this disease is attended with some difficulty, as some grave and prolonged cases terminate in recovery, whilst we are told that occasionally mild ones terminate rapidly in death, by peritonitis, from perforation of the intestine. But, as a general rule, where the disease is not complicated with acute local affections, or chronic disorders, or in broken down constitutions from previous disease, and where the pulse does not range higher than 100 or 110 in men, 110 to 120 in women, the cases will terminate favorably, if the treatment be appropriate and not *aggravating*, as is too often the case. According to my own experience, the prognosis in typhoid fever is for the most part favorable, the disease having a strong tendency to terminate in recovery in its uncomplicated form, even when left to run its course without treatment. I have frequently seen grave and prolonged cases terminate favorably, but have not seen mild ones termin-

* I am aware that this is a controverted point, but I write only what I know to be facts, and I have almost uniformly found the stools of typhoid fever, when diarrhœa was present, to be without smell, and I have often predicted a favorable change while the patient was at stool, by perceiving distinctly a strong bilious smell to the discharges.

ate unexpectedly from peritonitis or otherwise. If it were not for the daily reports of physicians, medical journals and newspapers, of a large proportion of deaths taking place from this disease in different portions of the country, I would venture to speak in more positive and favorable terms in relation to the prognosis. In low and grave cases, the patient lies on his back in a state of stupor, and slides down in the bed from muscular weakness. One of the first favorable symptoms to be noticed in such cases, is a tendency to turn on the side himself, or even ask to be turned. When this position cannot be maintained but for a few moments at a time, but is daily repeated, it indicates returning muscular strength, consequently a favorable change in the disease.

Treatment.—There are as many modes of treatment of this disease, in this section of country, as there are physicians. Its cure has generally been attempted by *calomel* and *quinine*, especially by the older class of physicians. They have attempted to cut short the disease in its onset, by bringing the system under the specific influence of this potent medicine. In this they have not only failed, but almost universally lost the patient, especially where the disease was of a grave form and went to run a tedious course. *Typhoid fever is a disease that cannot be cured, but can be safely conducted through its different stages by a judicious course of treatment.* He who attempts to cut short the disease by strong medicines will lose over one half of his patients. The treatment of typhoid fever must be strictly *eclectic* to be successful, for no exclusive or specific mode of treatment can be laid down that would be applicable even in a small proportion of cases. We must meet symptoms as they arise in each and every individual case, and endeavor to connect the morbid functions of the different organs, and keep them in as healthy a condition as is compatible with the nature of the case, suffering the disease to run its course as other *specific* disorders.

With these remarks, I proceed to give my own mode of treatment in this disease, which has been entirely successful up to the present time. My course of treatment is plain, simple and mild; in this respect, not unlike the prescription given

to Naaman of old, by the prophet, Elisha. I attack the disease, not with a view to cure, but to safely conduct the patient through its different stages. So uniformly is this my course of practice, and so thoroughly am I convinced of the utility of such a course, that I speak of conducting my patients safely through the disease, and not of breaking the fever, as is the common term, especially among the vulgar. A great man, and practitioner of medicine, once, on being asked what he thought of a certain treatise on fever, replied, "he did not like fever curers. A fever," said he, "can be conducted safely through its different stages—it cannot be cured." These remarks on fever, by Pitcairn, are applicable in every particular to the treatment of typhoid fever. When the patient is stout and robust, I take some blood, not being in the least governed by the quantity or quality of the fluid drawn, but its effects upon the heart and arteries. But typhoid fever is a disease so insidious in a great majority of cases that the practitioner is not called in until the bleeding stage has passed by, (if it ever existed,) which it does not in a great majority of cases. The functions of the skin and liver are generally suspended throughout the entire course of the disease; the former being dry, warm, and harsh to the touch, and generally of an unusual yellowish hue, whilst there is no trace of bile in the alvine discharges. After sufficient blood is taken, where blood-letting is deemed proper, I am governed by the circumstances of the case. If diarrhœa is present, (which is generally the case,) and no traces of bile in the alvine evacuations, I give blue pill and Dover's powders every four or six hours, according to circumstances. I watch narrowly the effects of the Dover's powders in every instance, as that medicine is not borne well by some patients; but in a majority of cases it is not only tolerated, but produces the happiest effects when given in union with blue mass in the proportion of from 3 to 5 grs. of the former with 3 or four of the latter every four or six hours, and followed by castor oil, if it does not act on the bowels once or twice in the twenty-four hours. But if this prescription does not check the diarrhœa, I add a small proportion of the sugar of lead to each dose, until the diarrhœa is effectually checked. The bowels may be allowed to remain inactive, without risk, for twenty-

four or even forty-eight hours, when they should be opened by castor oil, or, what is sometimes better, a mild injection: gum arabic or elm water should be used freely from the beginning of the disease. Nothing should be allowed but the mildest liquid diet, and that in small quantities. This course of dieting should be observed throughout convalescence, very gradually increasing the quantity and quality of the food, to prevent relapses, which are very common and dangerous in this disease. Purgatives should be avoided, or entirely withheld, as they do no good, and generally set up obstinate and ungovernable diarrhœa. I have more than once seen the system under the influence of mercury in typhoid fever, in *mild* cases, without checking the disease in the least degree; but in the general making a more tedious case. But to salivate a patient in a grave case of typhoid fever, I think (I had like to have said impossible) almost impossible, unless the constitution be able to withstand the disease and the medicine until nature begins to ameliorate the symptoms; then, and not until then, can ptyalism be induced. If the salivation does not sink the patient it will add to the tediousness of the case, and render him more liable to relapse. I could give numerous cases of this kind, but time and space forbid at present. One of the most common and dangerous effects of the administration of calomel in this complaint is hæmorrhage from the bowels. Blisters are excellent remedies in this disease, when indicated and well timed. They should be applied to the lower portion of the abdomen, where there is great soreness on pressure, with or without tympanitis. When this latter symptom is present, they sometimes produce the happiest effects. They may be applied to the nape of the neck, when the delirium in grave cases is wild and furious—to the side, or other portions of the body where pain is seated, or even where pain has been complained of, in the onset of the disease. Under such circumstances they seldom or never fail to produce the most satisfactory results. The patient will be greatly benefited in all cases by having his hair cut short, and applying cold to the head, where there is much heat of this region. The body should be sponged daily with warm water, or water and vinegar, and the patient's body well dried. This never fails to add much to his comfort. Daily

sponging is too much neglected in the treatment of all fevers, and especially that of typhoid fever. Strict attention should be had to changing the patient's clothing and bed clothing, as this not only contributes greatly to the comfort of the patient, but is one of the principal means to prevent the spread of the disease.

ARTICLE XIX.

The Morbific Influence of Intestinal Worms. By ROBERT CAMPBELL, M. D., Assistant Demonstrator of Anatomy in the Medical College of Georgia.

Perhaps there is no agency in the whole etiological catalogue, which has been dignified with more importance than that under consideration. There are few diseases, or symptoms of disease, from the first knowledge of intestinal worms until the present day, which have not found a satisfactory solution in this prolific source of mischief. And while now, many over-rate its import, there are others who have rebounded beyond the opposite extreme ascribing sanative properties to these, to say the least of them, useless and troublesome parasites.

The proposition of some theorists who maintain that worms are natural to all animals, therefore innocuous, that they are serviceable as the natural scavengers of the body, feeding upon the excrementitious residuum from which the chyle has been abstracted, is a vagary which needs but little disputation. Although Nature has generally an eye to the welfare of her creatures, there are instances which would seem to form decided exceptions to her general character of beneficence. Naturalists have furnished innumerable instances of the fatal effects of parasites upon different animals. The vermin which infest young poultry are equally natural, still under favorable circumstances, how frequently do we observe fowl-yards strewed with the evidences of their disastrous consequences! Besides, it would seem inconsistent with natural economy, that a family of smaller animals should be engendered to devour the excrement of the larger, while in the body, when very soon, if they did not impede its progress, it would be gotten rid of by

a process *apparently* instituted by nature herself. If the foregoing hypothesis were true, then wormy children should be fairer samples of health, than those who are not favored by their *benign* influence. Observation and reason prove the fallacy of this interpretation of the function of these entozoa.

It would occupy too much space and time to enumerate the thousand and one phenomena which have from time to time, and in different parts of the world, been attributed to the presence of worms in the *primæ viæ*. The whole paraphernalia of symptoms constituting various epidemical diseases, have by some writers, been charged upon these pernicious accumulations.

Although the symptoms of worms, as enumerated by authors, are almost innumerable—yet, it is unfortunate, that none of them can be relied upon as pathognomonic of their presence in any case, except that of *their actual appearance*. Many of their concomitants might legitimately have been attributed to their agency, were it not for the fact, that their testimony is greatly invalidated by the same phenomena presenting themselves in other diseases, imparting to them decided verminous characters, when upon post-mortem examination, this cause is sought for in vain. But it is not our province here, to examine into the respective value of their symptoms particularly, but to ascertain their position as a *cause* of disease, and to form, if possible, some rationale of their pernicious action upon the economy, through principles based upon its constitution and functional endowments.

The question—“Why is it so difficult to detect the presence of worms, by their pathological manifestations?”—finds its solution in this: that inasmuch as the irritation produced by them is only *mechanical*, there are various other mechanical causes operating upon the same sentient surfaces, capable of producing similar pathological manifestations. Hence, they can be claimed by their advocates as only one of the many sources of *mechanical irritation* and not as imparting any peculiar poisonous principle to the system; though Vogel asserts, that they prove injurious also, “by exerting a specific action, possibly by fluids which they secrete or in some other unknown way.” And this brings us to calculate the amount of

irritation, which this excitant is capable of effecting ; although it is a matter, somewhat involved in obscurity, there being no way of approaching this result by observation and experiment ; yet reasoning from analogy, we are furnished with satisfactory grounds, upon which to predicate our inquiry. We must consider worms in the light of a *foreign body*, superadding the aggravating circumstances of their vital properties. The presence of a body of adventitious character in the stomach or intestines, whose weight, size and solidity are equal to those of fifty lumbricoides or a *tænia solium*, for instance, must embarrass, in some measure, its normal operations. Add to this the power of motion, (which is known to exist in an eminent degree,) the operation by which the occupancy of the organ is maintained—whether by adhesion to its coats or by the exercise of activity, together with the draft made upon the system directly or from its pabulum, for the support of this foreign object—and we can form some idea of the degree of disparaging force which worms are capable of exerting upon the organism.

The extent of the irritation produced by worms must be proportioned to their size and number ; and there are also other circumstances to be taken into consideration which modify the development of its effects ; among which are—the difference in the character of the worms, their varying location, and the unequal amount of sensibility enjoyed by their several localities. For instance, the *tænia* and lumbricoid more frequently occupy the small intestine, which is possessed of a more limited degree of innervation than the rectum, the site of the *ascaris vermicularis*, or the stomach which is often entered by the tape and lumbricoid worms, and which is endowed with a still more exalted degree of nervous susceptibility.

There are also constitutional conditions which favor the morbid action of stimuli generally. It is known that those subjects termed “nervous” are most susceptible to the effects of irritants ; and again, this excitability may be induced by a pathological state, as that of general excitement of the nervous system, under the influence of fever, &c.

It would appear from the foregoing, that intestinal worms are not accessory to the perfect exercise of the organs of the

body; but, on the contrary, that through their agency, health may be more or less deranged, principally in consequence of an irritation which they are capable of exciting; that their morbid influence varies extremely with their number, size and species, and the sensibility of the parts they occupy, together with the constitutional or co-incidental excitability of the general system. We know that the effects of irritation are in greater or less degree two-fold—*topical*, or those pertaining to the part upon which the impression is exerted, and *sympathetic*, or their reaction upon the general system. The disease, then, resulting from *verminous irritation*, is manifested in local and sympathetic, or general morbid phenomena.

Among the *local effects* have been enumerated—rending or colic pains in the bowels, from direct irritation of their muscular coat; diarrhœa or dysentery, from increased secretion or excited peristaltic action; painful distension of the bowel; mechanical obstruction by a mass of worms; or by intussusception, tenesmus, hemorrhoids, etc., etc. The two last effects cited, pertain to the vermicular species, from their peculiar location. The post-mortem appearances, which have been observed, of the tissues in contact with a mass of worms, are redness and an inordinate quantity of mucus.

In determining the validity of the foregoing morbid effects assigned to the immediate influence of worms, it is but necessary to make reference to the many cases reported by respectable authors, in which the size, number and character of the worms present were amply sufficient, legitimately, to account for such results. Cases occurring under our own observation, also, might furnish additional support to this reasonable probability. Their power to injure the coats of the intestines, or entirely to perforate them, has been admitted by some and denied by other very respectable authority. Vogel states, in speaking of the round worm, that “it appears to be capable of perforating the intestine, by thrusting asunder with its head the fibres of the intestinal coats; it thus passes into the cavity of the abdomen, where it gives rise to inflammation, suppuration and abscess. Sometimes it even escapes externally through the abdominal walls.” Condie, on the other hand, although he confesses that he has seen a lumbricoid worm penetrating the

intestine, being half way through and held in the opening, yet denies their power ever to perforate.

Among the sympathetic effects ascribed to this source of intestinal irritation by many respectable authors, and which are said to disappear upon the expulsion of worms, are hacking cough, symptoms of croup, pneumonic symptoms, asthma, rapidity and irregularity of pulse, convulsions, chorea, tetanic symptoms, hydrocephalus, epilepsy, paralysis, delirium, mania, emaciation, dropsy, and various anomalous disorders.

We now enter upon the more difficult part of our inquiry—the investigation of these more obscure and mysterious effects of intestinal worms—effects though, which are attributed with as much confidence to this cause, by medical philosophers, as any of more direct sequence. And here, we are to consider not only the local manifestations of their influence, with their echo in some other, perhaps distant part of the organism; but to attempt the pursuance of their hidden connections, through some physical and tangible medium of relationship. To accomplish which, we must necessarily discover some chain of communion, binding all the organs of the body, however dissimilar their construction and destination, and even the minutest parts of those organs, into one reciprocating system, each perceiving the impressions directed to each other, and responding to them by as decided manifestations, though differing in character and proportion, only in such measure as the peculiar constitution and function of those organs may be compelled to modify them. This desideratum will be revealed to us, in a glance at the well-known anatomy of the nervous system, with its familiar physiological phenomena.

The anatomical relationships of the parts in which we are now interested, comprehend an extensive scope, although of so general a character as to enable us to rehearse them in but few words:—We know that the alimentary canal is innervated by the ganglionic system of nerves—the stomach and rectum receiving in addition supplies from the cerebro-spinal—the former by the eighth pair, the latter from the hypogastric and sacral plexuses. By means of the branches of the eighth pair, the stomach is connected to the œsophagus, the lungs, the pharynx, the larynx and the heart. The sympathetic branches supply-

ing the stomach are derived from the solar plexus. By means of this plexus, the stomach is also brought into relation with the other abdominal viscera. The sympathetic or principal nerve supplying these organs, is intimately connected with the cerebro-spinal system by union with all its nerves. Familiar instances of this conjunction with the cerebral nerves, are the ophthalmic ganglion with the third, the sphenopalatine and otic ganglia with the fifth, the superior cervical with the sixth nerve, &c. All the spinal nerves are known also, to have a close connexion with this nerve, by a branch passing from each root of every spinal nerve, to a neighboring part of the sympathetic; hence the involuntary and voluntary nervous systems are known to be intimately associated in their anatomy.

It is established, that the intimacy subsisting by anastomosis, between nerves presiding over different purposes, does not interfere with the discharge of their respective offices; the nerves being but the passive channels through which the operative force is transmitted, and unassisted, having no more to do with generating or diverting the influence they convey, than the telegraphic wires have in charging themselves with, or disposing of, the electric current. Since, then, the nerves are but passive conductors, they can oppose no obstacle to the passage of missions, of whatever origin, *reversely*—from their sentient extremities to the nervous centres; nor even to their being *reflected* from those centres, enhanced in force (for it is known that, here, power may be engendered or renewed),* to some distant organ or organs—the reflected image there manifest, not necessarily bearing resemblance in character or degree, to the original impression, but being transformed into that peculiar phenomenon, which it is the nature of those organs respectively to produce. It is well known that it is upon the existence of such a quality in the nervous system, that many of the physiological processes depend for their consummation—e. g. the process of deglutition.

It is obvious, that the same qualities in the nervous system, which are employed in a physiological state, to produce phenomena subservient to life and health, may be appropriated by

* See Todd's Cyclopædia of Anatomy and Physiology.—Art. "Nervous Centres."

morbific influences to effect their destruction! As in the case of deglutition, any object coming in contact with the mucous membrane of the fauces, by indirect stimulation, excites the action of the pharyngeal muscles—so may baneful influences operating upon the nervous extremities of a certain portion of the organism, through the excito-motory or reflex action of the nervous system, with its all-pervading relationships, discharge their venom upon some distant, and, apparently, unconnected organ. And thus may intestinal irritation, and that arising from the presence of *worms*, (since this is a source of intestinal irritation,) manifest itself by morbid phenomena, in different and distant organs of the body.

In taking a retrospective glance at our subject in its different bearings, we feel authorized in maintaining, that we have in its history all the necessary elements for the production of such phenomena as are ascribed to the *influence of intestinal worms*! For those which involve disorder in the voluntary system of muscles—as convulsions, chorea, tetanus, &c.—we have, applied to the sentient extremities of excitor nerves, an irritant, sufficient, under certain circumstances, to excite in the nervous centre, a “polarity” capable of effecting these phenomena in the muscular system, through the medium of the motory nerves. For those which result in impairment of the mental faculties, as delirium, mania, &c., *they* may be interpreted by referring them to reflected irritation. The brain, being an organ of peculiar constitution, whose functions are the intellectual faculties &c., this intestinal irritation is reflected through the medium of nervous communication which we have already seen to exist, and manifests itself upon that organ, in excited or perverted function, and we have mania or delirium. We know that operations of the mind are capable of influencing the functions of the digestive organs—and *vice versa*, along the same chain of communication, the brain may be influenced by causes having their origin in the digestive apparatus.

These few examples may suffice, but in like manner, perhaps *all* the diseases which have ever been attributed to intestinal worms as a cause, and the many symptoms popularly referred to them, might find a philosophical basis in the anatomy of the human frame, and the known laws governing its physi-

ology and pathology—were it not for the fact, that there can be made no distinction between intestinal irritation from other sources, and that from worms, unless it be substantiated by their actual appearance. And it is fortunate that the course of medication required for their expulsion is such, that it may be pursued in all suspicious cases without endangering materially the welfare of the patient.

Finally, we deem it just to deduce from our investigations the following conclusions, viz: that intestinal worms are not conducive to health, but, under favorable circumstances, are capable of exerting an irritation which involves the health and life of their subject. That the circumstances favorable for its development, are their existence in great numbers, or of large size—particularly in the stomach or rectum; and *the co-existence of other diseases, which they principally serve to complicate and render dangerous*. That there is no pathognomonic symptom of their presence, except their actual appearance; and that, where we have reasonable grounds for suspecting their existence the proper anthelmintics should be exhibited.

ARTICLE XX.

Nitrate of Silver. By JNO. S. WILSON, M. D., of Muscogee County, Georgia.

So much has been written of late years on this potent and valuable article of the *Materia Medica*, that I cannot expect to add anything new. My object in this communication is, to make some practical remarks on its therapeutic application, with the hope of extending its use, and removing the prejudices which may still linger in the minds of many physicians—prejudices originating in the unfortunate designation, “Lunar Caustic,” and its classification among the eschorotics. Those prejudices no longer exist among physicians who daily witness its delightful sanative effects in wounds, inflammations and ulcers, and they are fast receding before the light of experience and observation.

Dr. John Higginbottom, F. R. C. S., says, after twenty years further experience in the application of this remedy, “There is

no form of acute superficial inflammation, arising from either constitutional or mechanical causes, where the nitrate of silver may not be applied with great safety and advantage. The nitrate of silver is not a caustic in any sense of the word. It subdues inflammation, and induces resolution and the healing process; it preserves, and does not destroy, the part to which it is applied.”* Dr. Higginbottom recommends the nitrate highly in the following disorders: In recent bruises, in small and large ulcers, old ulcers of the legs, punctured wounds, bites and stings, dissection wounds, wounds from rabid animals, lacerated wounds, hemorrhage from leech bites, incised wounds, erysipelas, cuticular and phlegmonous; ulcers with varicose veins, (with graduated compression); superficial burns and scalds, and senile gangrene. This long list, which might be extended by the addition of other diseases, external and internal, medical and surgical, such as, herpetic and various specific cutaneous diseases, aphthæ of the mouth and throat, ulceration of the gums, various forms of ophthalmia, corns, diarrhœa, dysentery, gonorrhœa, leucorrhœa, &c., serves to show the varied application of this agent, and its title to our regard, as one of the greatest and most useful articles of the *Materia Medica*. So far as my limited experience goes, with “the opprobrium of surgery”—*old ulcers of the legs*—I feel justified in saying that all *curable* forms, whether simple, indolent, irritable or varicose, can be cured by the use of the nitrate of silver, together with *cold water* and Bynton’s method—adhesive strips and graduated compression. I will even go further, and say, that I believe nine-tenths of such ulcers can be cured by the persevering application of these remedies, assisted by proper constitutional treatment. By these local remedies alone, without the aid of general treatment, I have succeeded in curing an ulcer in an aged subject, of five years standing, involving the whole of the dorsum of the foot, and complicated with caries of the metatarsal bones. My faith in specifics is limited, and I am very far from believing in the universal applicability of any single remedy; but the happy effects of the nitrate of silver on ulcers, in conditions apparently diametrically opposite, seems to form a wonderful exception to the action

* Vide this Journal, May, 1850, p. 293.

of other remedies, and completely confounds all our theoretical reasonings. When applied to a simple ulcer, an artificial cuticle is formed which protects it from irritation, while the formative process goes on quietly beneath. When applied to an indolent ulcer, a constringing and tonic influence is exerted on the weak and flabby granulations, imparting to them new life and a healthy recuperative activity. When applied to an irritable ulcer, it allays the morbid irritability, by substituting its own peculiar irritation, which naturally tends to resolution and restoration of the affected part. In conclusion, on this point, I would say, that I consider the nitrate, with graduated compression, superior to all other modes of treatment in ulcers of every kind and grade, and that they leave us but little to wish for in this difficult department of surgery. In *herpetic* and various forms of localized cutaneous diseases, I esteem it one of our most certain and effectual remedies. In *apthous stomatitis*—*anginose affections*, simple, scrofulous, or exanthematous, it stands unrivalled. I have seen it used in a case of chronic apthous stomatitis, of several years duration, with the happiest effect, in a female of feeble and cachectic habit, after her disease had resisted all other remedies. The only auxiliary was the mixt. ferri comp. In *angina*, where the swelling was sufficiently great to threaten immediate suffocation, I have given almost instant relief by mopping the throat with a concentrated solution of the nitrate of silver. In that distressing form of angina accompanying *scarlatina*, I believe it is now almost universally admitted to be the best of all local applications, and I think it fully entitled to the high position it occupies. In *opacity* of the *cornea* and *chronic ophthalmia*, I consider it far superior to any or all the salts of lead, zinc, or mercury. In *chronic diarrhæa*, of an intractable kind, I have used it in solution, in doses of $\frac{1}{4}$ gr. every three hours, with good effect. In *dysenteric tenesmus* it acts like a charm, when administered by enema—10 grs. to the oz. of water.

I would particularly invite the attention of the profession to the valuable properties of the nitrate of silver in this distressing disease, for I fear that its great virtues are not properly appreciated, and that its use is not at all commensurate with its distinguished merits. I find that, even as late as the year 1848, it

was considered an entirely *new* remedy, in this disease, by a physician of Tennessee, who was filled with "*astonishment and pleasure*," when he witnessed its delightful effects, for the first time, "because he thought he had made a grand and important discovery!" In dysentery, it is well known that the inflammation or ulceration is often confined to the lower portion of the rectum; in this case, we can bring this potent agent immediately in contact with the diseased surface, and thus *cure* it, without the assistance of any other remedy. But I would not thus restrict its use; for, even admitting that the lesion exists in the colon, or the upper portion of the rectum, the sympathetic excitement is seated in the sphincter ani muscle and the lower part of the rectum. By the use of the nitrate, we substitute a peculiar remedial excitement, of transient duration, for a morbid, persistent irritation, much more painful, and of indefinite duration. Of course the cases now under consideration will require appropriate general remedies, while the nitrate of silver still retains its place as an invaluable auxiliary—as an *anodyne*, while the other remedies are producing their constitutional impression.

I have recently prescribed for a woman who exhibited the following symptoms: cold skin, pulse almost imperceptible, bloody mucous discharges, with excruciating tenesmus every five or ten minutes, as she lay in bed. Ordered, cal. grs. v.; pulv. Dov. x. But little relief. After the use of two injections of the nitrate of silver (10 grs. to the oz.) the tenesmus was completely relieved. The calomel and Dover's powders were continued, and in four days she was discharged cured.

In conclusion, I would remark, that I think it best to use the nitrate in large doses, and uncombined. Should the irritation be excessive, an enema of starch and laudanum can be used. In the case referred to, no complaint whatever was made of the remedy.

In a case of *chronic metritis*, combined with leucorrhœa, dysmenorrhœa, uterine hypertrophy and induration, with their consequences, tenesmus and micturition, I have derived great advantage from the application of the solid nitrate to the os uteri, by means of the speculum; and, what is somewhat remarkable, although I considered the tenesmus and micturition

mostly mechanical, the application of the nitrate of silver gave more relief to the first mentioned symptom than any remedy which I could apply to the rectum itself.*

I have extended this article to greater length than I designed, but not greater, I think, than the importance of the subject demands. If any thing I have written will have any influence in extending the use of the nitrate of silver, and if others derive the same happy effects from its use which I have had the good fortune to realize, they will be amply compensated, while the writer will be highly gratified in being able to contribute something to the usefulness of his profession, and to the relief of suffering humanity.

PART II.

Eclectic Department.

On the Physiology and Pathology of the Ganglionic Nervous System. By JAMES GEORGE DAVEY, Licentiate of the Royal College of Physicians, London, &c. (Continued from p. 293.)

"There are yet great truths to tell, if we had either the courage to announce, or the temper to receive them."—DISRAELI.

CHAPTER II.

Before proceeding to consider in detail the physiology of the solar ganglion and its dependencies, it is perhaps desirable to offer a few observations on the anatomical relations and dependencies of the three nervous centres. These will, it is hoped, facilitate the right comprehension of those physiological remarks which follow.

The brain, the spinal cord, and the solar ganglion, regarded respectively as the centres of the several nervous systems of man and the higher animals, though executing distinct functions in the economy of life, are, by means of the innumerable ramifications of their subordinate parts, and their union each with the other brought into so close a connexion, that their physical separation is a matter of impossibility. Though not *mutually dependent*, they nevertheless exercise such a reciprocity of action, that the health and well-being of man is to be regarded as inseparable from their union. The solar

* I think I have seen a similar statement made in some of the journals, but cannot now refer to it.

ganglion, brain, and spinal cord, may be together, not inaptly, compared to an ordinary tree, the root, trunk, and extremities of which may be said to severally represent them; for the first has been the source, and continues to be the life of the other two; it has existed without them, and might again do so, though during their integrity it fails not to be the recipient of their toil, so to speak. The *root* will sometimes live on without the trunk and its extremities, and so will the paralyzed idiot. By referring to an engraving of the principal part of the organic nervous system, or sympathetic nerve, the anatomical connexions of the ganglionic with the spinal nerves, throughout the entire length of the cord, will be distinctly made out. It will be seen that each of the several ganglia, whether cervical, dorsal, or lumbar, gives off communicating branches with the individual spinal nerves, thus uniting the two systems. The solar plexus, the centre of the organic nervous system, is therein demonstrated as formed by the union of the two semilunar ganglia. It is described by Mr. Erasmus Wilson as "*a gangliform circle*," from which branches pass off in all directions, like rays from a centre. It gives off distinct filaments, which accompany, under the name of plexuses, all the branches given off by the abdominal aorta. Thus we have derived from the solar plexus the—

Phrenic plexuses.	Supra-renal plexuses
Gastric plexus.	Renal plexuses.
Hepatic plexus.	Superior mesenteric plexus.
Splenic plexus.	Spermatic plexus.
Inferior mesenteric plexus.	

However, the principal nerve of the solar plexus is the great splanchnic. It arises from the upper and back part of the organ, and proceeding upwards, pierces the diaphragm immediately to the outer side of each crus, ascends in front of the vertebral column, within the posterior mediastinum, and terminates by dividing into five branches, which severally proceed to the sixth, seventh, eighth, ninth, and tenth dorsal ganglia. The pelvic viscera and the lower extremities are supplied with their organic nerves from the hypogastric plexus and sacral ganglia. The union of the ganglionic and spinal nerves would, in itself, be sufficient to establish a connexion between the former and the cerebrum and its nerves. They have, however, a more direct communication, by means of the ascending or carotid branch of the superior cervical ganglion, which, having entered the carotid canal with their internal carotid artery, divides into two branches, which form several loops of communication with each other around the artery, which constitutes the *carotid plexus*. Now the carotid plexus is the centre of commu-

nication between all the cranial ganglia; and, being derived from the superior cervical ganglion, between the cranial ganglia and those of the trunk; it also communicates with the greater part of the cerebral nerves, and distributes filaments with each of the branches of the internal carotid, which accompany them in all their ramifications.* As each of the cranial ganglia, moreover, communicates freely with the cerebral nerves distributed about the head and face, exteriorly and interiorly, it follows that nothing more is required to establish the anatomical position which has been taken. I may add, nevertheless, that the pneumogastric nerve or vagus, by being distributed to the respiratory and digestive apparatus and heart, where it forms communications with the organic or vital nerves, as well as with the solar ganglion itself, by means of a small branch, which is represented in Plate 19 of Mr. Wilson's work before quoted, as being given off by the principal trunk, where it turns forward to be lost on the anterior surface of the stomach, yet more effectually secures the relationship between the cerebrum and the solar ganglion, the centres of their respective systems.† The phrenic nerve, also, in being formed by the union of filaments from both the cervical and sympathetic nerves, and in being further distributed to the diaphragm, provides for the same end.

The anatomical relations of the several nervous centres, as above explained, Mayo strangely regards as a reason why the "sympathetic nerves in human beings can only be regarded as branches of the cerebral and spinal nerves, destined for particular uses."

I have before treated of the "*nisus formativus*," of its nature and uses, as well as of its general independence of the brain and spinal cord, and I have therefore, to some extent, anticipated the observations I had to offer "*on the physiology of the solar ganglion*." The reader will, I hope, excuse me if I recapitulate in a general way the line of argument there found.

In the first place, as regards the acephalous and amyencephalous monstrosities of Lawrence and Hall, we have the most conclusive evidence of the independence of the organic or ganglionic system of nerves on the cerebro-spinal system. We have a convincing and irrefragable assurance that the function of the solar ganglion and its dependencies is in itself as completely performed without the brain and spinal cord as with

* Vide Wilson's Practical and Surgical Anatomy.

† I am perfectly aware that Dr. Stevens maintains that both the vagus and spinal accessory nerves derive their origins, not from the medulla oblongata, but from the upper part of the semilunar ganglion. I cannot think so. In the first place, the direct connexion of the par vagum with the *semilunar ganglion* is by means only of a small nervous branch or filaments. As to the spinal accessory nerve, is it not distributed to the sternomastoid and trapezius muscles?

them. Who in looking to the acephalous, the brainless monster reported by Mr. Lawrence, would venture to declare that the influence of a *brain* was necessary to *life*; that it was anything more than an instrument of life. And who, in looking to the amyencephalous, to the brainless and marrowless monster described by Hall, could dare affirm that either the brain or the spinal cord were in themselves indispensable to existence; that is, to *life*? In both instances we are shown facts, which demonstrate to us as plainly and conclusively as can be demonstrated, the perfect and normal operation of the vital principle. We see the body nourished the viscera perfected, and the bony fabric matured; and we are assured, therefore, that circulation, absorption, and nutrition, have been in full operation; in perfect action, without either a brain or spinal cord. It is truly absurd for Müller or any other physiologist to declare that "the *anatomy* of these monsters is *not* at present known with sufficient accuracy for any '*conclusion*' to be drawn from it."* The same cause which enables the abdominal and thoracic viscera of brainless and marrowless monstrosities to continue their functions, the identical source from which the heart and its vessels receive their power to circulate the blood; the liver, to secrete its bile; the stomach, to digest its contents; the intestinal absorbents, to take up the chyle; and the kidneys, to secrete their peculiar fluid, also empowers the organism of the decapitated animal in which the spinal cord has been also destroyed, to continue its specific action; the "*ultimate cause*" of the several phenomena of *life* is in both instances the same. "Clift saw the heart of a carp continued to beat eleven hours after the destruction of the spinal cord;" and Müller tells us that "in fishes the contractions of the heart continue for the space of half a day after the destruction of the brain and spinal marrow." The same author obstinately adds, "that the brain and spinal marrow must nevertheless be regarded as the principle source of the nervous influence; the cardiac nerves, under such circumstances, still retain a portion of the motor influence." In the instance of the frog experimented on by Redi, which lived "six complete months" after the "removal of the whole brain," as well as that of the animal operated on by Sir B. Brodie, in which the posterior part of the spinal marrow was removed without affecting in any degree the strictly *vital* actions natural to those parts which receive their nerves from it, would Müller venture to assert that, in either case, the phenomena observed were referable to the circumstance of the retention of a certain power or principle of action, originally derived from either the brain or the lost por-

* See Müller's *Physiology*, vol. i. p. 208, second edition.

tion of the spinal cord? Impossible! Nothing is more certain than that the destruction or *removal* of the solar ganglion and its dependencies is immediately fatal to the functions of the brain and spinal cord, no less than to those of the other corporeal organs, as the liver, stomach, &c.; and it is equally true that the destruction or removal of either the brain or the spinal cord, or even both of them, *if effected with care*, may be accomplished without impairing the energy of the ganglionic system. If you destroy the brain and spinal marrow of a frog, you will not *kill* the animal: the heart will still continue to beat, and the digestive process be carried on. A slight blow on the epigastrium, *over the solar ganglion*, will sometimes kill a man, but extensive injuries of the brain and spinal marrow are not unfrequently borne with comparative impunity. Pressure on the solar ganglion of a dog or cat or rabbit, will quickly destroy life; and if it be carefully made over the upper part of the *lumbar region*, where the experimenter will avoid the solar ganglion, the lower extremities will become completely paralyzed; the animal will merely draw them after him, as if they were but artificial appendages. Remit the pressure and in a few minutes the spinal functions are restored and the animal is as lively as ever. This, in itself, is a beautiful illustration of the dependence of the vitality of the lumbar spinal cord on the contiguous ganglia of the sympathetic. In the lowest classes of animals, we see the existence of the same nervous organism without the addition of parts which characterize those higher in the scale of being; in them there is observed a ganglionic system only, and therefore it is that they execute functions purely preservative. We may ask, if a ganglionic system be deemed sufficient for the vital actions of one class of beings, why should it not for another? Digestion, circulation, secretion, absorption, &c., are no more perfectly performed in man than in the oyster or medusa. *Life*, then, is identical throughout animate nature, and if a particular nervous power be held sufficient in one instance for the effects observed, why should it not in another, or even in all? There is this difference between the lower and more perfect animals; that whilst in the latter the vital principle is, it would appear, generated in a central organ, and from it diffused through the organic nervous system, in the former it is, I apprehend, produced equally by all the ganglia of the sympathetic; and therefore it is that "the division of these animals into separate fragments does not destroy the organism, but, on the contrary, gives rise to the production of several distinct beings." It would seem that the addition of a spinal system to a ganglionic, as in the annelidæ, constitutes the limit within which Nature allows the

divisibility of the vital principle. "The articulatae, although, like the annelidæ, they have a ganglionated nervous cord, do not continue to live when thus divided." The first possesses, in addition to ganglionic and spinal nerves, a medulla oblongata, from which nervous branches arise which furnish the animal with the use of its external senses; and these establish so indissoluble a connexion, and so direct a dependence on the external world, that the facilities which the animal enjoys more than compensate for the loss of the other property. A step is, moreover, made towards that more perfect organism wherein the increase in number and complexity of its parts tend, in an eminent degree, both to establish the responsibility of man, and to explain the nature of those physical laws to which *man* must submit, if he would avoid the many miseries which now press on him from all sides.

We may, with much propriety and truth, regard the permanent condition of the medusa, for instance, as characteristic of the temporary, or accidental, or acquired condition of *man*. In the very early period of foetal life, we observe nothing more than a gelatinous, shapeless mass, in which the microscope can nevertheless discover a nervous ganglion or ganglia. *Life*, in both, is complete. This is a man's temporary condition; the same is permanent in the polype. In the amyencephalous monster—that is, brainless and marrowless foetus—we see the ganglionic life of the medusa accidentally occurring; and similarly so in the animal which has been deprived of its brain and spinal marrow; as well as in the individual under the influence of animal magnetism; when, as oftentimes happens the cerebral and spinal functions are completely obliterated, and the patient, of course lost to all consciousness and feeling, realizes the precise condition of one in a *profound* sleep.

From the observations already made, both in this and the preceding chapter, it is seen that the nutrition, growth, and reproduction, no less than the decay, of the organism, must be attributed to the operations of the organic nervous system. That the nutrition of all parts of the body, in conformity with the original type, presupposes the persistence of that power which originally produced all the distinct parts, all the organs, as "members of one whole," as parts necessary to our idea of the being; and which is present in the germ before any distinct organs are formed, while the animal exists merely "potentially."

Nutrition, then, is the continued reproduction, as it were, of all parts of the animal by its internal power; but in the adult, the reproduction can be effected only by the process of assimilation—that is by the union of new matter with the assimilating

parts; while in the embryo, in which no organized "ground-work" as yet exists, the parts are formed, their "ground-work" is, in fact, created, by the *formative power*, which is still undivided. "For although, until the whole body perishes all the organs are directed by *one formative principle*, which produces the concurrent action of all assimilating tissues, and the operation of which we admire, as the *vis medicatrix naturæ*, in the correction of the subtle material changes which are induced by disease; yet organized parts of the body, once formed, cannot, in most cases, if wholly destroyed, be again restored by this power, this vital organizing principle," (Müller)—which emanates from the solar ganglion, the central organ of the ganglionic nervous system.

The observations made by Müller in treatment of the influence of the nerves on nutrition, are singularly contradictory. His argument is to the effect, that although the nutrition of monsters "is by no means defective," but their development up to the period of birth, even *perfect*, yet "is there no proof that the sympathetic nerve has a more especial influence on *nutrition* than the cerebro-spinal nerves, except, perhaps the fact that the nutrition of a part does *not* cease when the nerves which it receives from the brain or spinal cord are divided"—a fact, which I must ever regard as a sure indication that the organic nerves can be none other than the nutrient nerves. Such is the inevitable conclusion. Why, it may be asked, does an injury or disease of the superior cervical ganglion arrest the nutritive processes in the eye. The branches of the third, fourth, fifth, sixth, pairs of nerves, usually distributed to the contents of the orbit, are untouched, and nevertheless, the eye loses its vivacity, and becomes *disorganized*. We have, then, in these instances, as in many more, both *positive* and *negative* evidence of the truth of my position—viz., that the solar ganglion is the SEAT OF LIFE; and that by means of its dependencies the vital principle is diffused through the organism. Further, in order to show that the organic nerves do not influence nutrition, he, Müller, says, "Where any organ is wanting, there is always a corresponding absence of the *nerves*," meaning their cerebro-spinal nerves, "absence of the eyes is attended with absence of their nerves." This is a mere assumption, and to prove it so, I beg to offer for the consideration of the reader the following *extracts* from a report of my own to be found at page 293 of vol. ii. of THE LANCET for 1835 and 1836, concerning the post-mortem examination of a child born without eyes: "On raising the anterior lobes of the cerebrum, but *one* olfactory nerve was seen, and that was on the left side; the optic commissure was placed more posteriorly than natural, the sella turcica being

much anterior to it. From its forepart, and in the median line, the rudiment of a nerve was continued, it was of a loose texture, and of a scarlet hue, as if from injection of its vessels. It proceeded into the substance of the pituitary body, in which it was lost. Several small blood-vessels passed through the foramen opticum; the fissura lacera transmitted both into and out of the orbit the ordinary nerves and the ophthalmic vein." "The orbital roofs were then removed, and disclosed the presence only of condensed cellular tissue, which when cautiously dissected away, exposed the several small muscles which naturally belong to the organ of vision, supplied with their respective nerves."

Sir Benjamin Brodie truly says that the processes of nutrition, growth, &c., must be referred to the same class of functions as that of secretion generally. The following experiments prove, not only so much, but also the relative properties of the cerebro-spinal and ganglionic nerves. In a young cat, the termination of the "nerves of the eighth pair on the cardia of the stomach were carefully divided; the animal was perfectly well afterwards; was lively, ate its food as usual, and the respiration was not affected. At the end of a week, and three hours after having been fed with meat, the cat was killed. On dissection, digestion was found to be going on as usual; the food in the stomach was in a great measure dissolved; and the thoracic duct and the lacteals were distended with chyle, having the ordinary appearance. The nerves were carefully traced, and it was ascertained that not the smallest filament had been left undivided. This experiment was repeated with exactly the same results.* It was long since asserted, "that the nerves of the par vagum are by no means necessary to digestion," and more recent experimenters, including Magendie and Dr. J Reid, have arrived at a similar conclusion, provided their division is made in such a manner as not to effect respiration and the free oxygenation of the blood.†

A portion of the anterior and posterior crural and sciatic nerves were removed in a dog in the upper part of the thigh; the limb, of course, became immediately benumbed and paralyzed. A wound was then made in the leg, and the claws of

* See Dr. Cooke on Nervous Diseases, vol. i. p 130.

† It is by no means intended to affirm that the vital organs, as the stomach, lungs, &c, are in every respect out of the reach of cerebral injury or irritation. Such would be nonsense. The urgent dyspepsia which attends on studio-sedentary habits proves the sympathy existing between the brain and the stomach, equally with the effects of violent mental emotion on either the stomach or heart, &c., and conversely the great digestive powers of the North American savage, to whom letters are unknown. I have, too, cured a great number of insane people, whose cerebral disease has been exclusively dependent on disordered states of the abdominal viscera. See the works of Combe and Caldwell.

all the feet were cut so as to make them of the same length. The wounds suppurated and healed as rapidly as if the nerves had been entire. At the end of seven weeks I measured the claws of the four feet, and found that those on the paralyzed limb had grown equally with those of the others.

The experiment was repeated by Sir B. Brodie on guinea-pigs, and with the same results. In one of them the tibia was broken, and the bone readily united.

The posterior part of the spinal marrow of a frog was removed so as to destroy the origin of the nerves of the lower extremities. In this case the bones of one leg were broken, and when the animal was killed the bones were found united, partly by callus and partly bony substance. Experiments, very similar in their nature and effects, have been performed by Dr. Sharpey; and Müller says, by Schroeder. I have also practically verified the position here taken—viz: that nutrition, growth, and reproduction, are equally the functions of the organic nervous system, both by experiments on living animals and by observing the recovery of the demented and paralyzed among the insane, from various kinds of bodily injuries (including fractures) and disease.

Whilst, however, Müller contends for the reciprocal dependence of the organs, and of the cerebro-spinal nerves, on each other for their existence, he cautions his readers from concluding that the production of the former depends on the pre-existence of the latter. He truly says, “both organs and nerves are produced by the same power, the *nisus formativus*, which resides undivided in the germ.”—*London Lancet*.

The Pancreatic Juice in Digestion.

Mr. Editor: I send you an article upon the importance of the pancreatic juice in digestion. It was suggested upon looking over notes which I took whilst attending a course of lectures upon physiology, delivered by C. Bernard of Paris, during the year 1848. I am not aware that the views here given have been hitherto published in this country. Carpenter, in the last edition of his work, alludes to its property of digesting fatty matter, but does not take into consideration its other properties. I take some interest in this subject, as I had the pleasure of witnessing the experiments of M. Bernard, which satisfied me of the truth of his views.

Yours, &c.,

M. P. SCOTT.

Bernard was the first to discover the true functions of the pancreas, which, if his conclusions are sound, is one of the most

important organs concerned in the process of digestion. By his patient investigations and philosophical deductions, he has cleared up one of the mysteries of physiology, concerning which all has been heretofore mere speculation. It is true that his conclusions have been deduced from experiments made upon the lower orders of animals; but by shewing the identity of the different fluids concerned in digestion in all of the lower orders, he was justified in coming to the conclusion, by analogical reasoning, that the same phenomena exhibited by them were also performed in the human system. He has proved thus the importance of searching for truths in medical science, by experimenting upon animals, and upon the facts thus gained, establishing the physiological laws which regulate the human system. Upon a just and accurate knowledge of these laws, it is only possible to establish a sound system of practice, without which all must be uncertainty or empiricism.

The old practice, of first concocting a theory and afterwards searching for facts to sustain the preconceived opinion, has been discarded—the order is reversed. The inductive system of reasoning obtains now in this science as in all others, and all theoretical notions that are not founded upon facts are cast aside—we obtain our facts, and upon them establish our theories. This may be a slow method; but it is the only sure one. It must be confessed, however unpalatable it may be, that we are far behind our brethren on the other side of the Atlantic in our researches in physiology and pathology. We are satisfied with folding our arms and quietly waiting for all improvements in our science which may be sent to us. These we receive and swallow with avidity, or speculate vaguely upon, or reject if they do not agree with the theories taught when we attended lectures. None think the experiments upon which they have been founded worthy to be repeated, or of making experiments to test the truth of those theories created in the imaginations of the speculative class of physiologists. But we have an offset, and flatter ourselves that if the French excel us in the science of physiology and are the best pathologists, we are the best practitioners. A vain illusion—for what kind of practice must that be that is not founded upon facts drawn from physiology and pathology, upon a proper knowledge of the functions of the different systems and organs of the human being, and the changes which are produced by disease. In the same vein and in the same manner the swarm of quacks and charlatans who overspread the land boast that they excel us. They point to the *results* of their systems—laugh at physiological facts, and care not to know whether a human being has a liver, so that he is possessed of a stomach or sac to receive their

drugs and nostrums. They go ahead. The French are the most cautious practitioners, for the same reason which induced Frère Jacques to give up the operation for stone in the bladder after the anatomy of the parts was taught him. He never knew before the imminent risk his patients ran of losing their lives if a blunder was committed. We are more heroic, bolder practitioners—not better.

The process of digestion has always been one of the most interesting subjects in physiology—no part of the science has perhaps so much engaged the attention of the profession in all ages, and there is none about which there has been so much speculation, nor indeed at the present time has it ceased to engage the attention of scientific men—the field for speculation and experiment is still open. Certain facts have, however, been established, by which ancient as well as more modern theories have been overthrown.

The opinions of some of the older writers, that the stomach was the only organ for digestion, and that the liver and pancreas, like the kidneys,* were mere secernants, destined to separate certain noxious principles from the blood, has been long since exploded. More modern authors have adopted the theory of chemical solution put forth by Spallanzani. These suppose that all kinds of food are reduced by the action of the gastric juice to one uniform, homogeneous semifluid; that all kinds of aliment produce the same nutrient principles which, submitted to the action of the gastric juice, becomes chyme, and that this chyme is changed by the action of the bile and pancreatic juice into chyle, which is now ready for absorption by the lacteals. This last is the opinion of Beaumont, deduced from his experiments upon a patient having a fistulous opening in the stomach. This opinion has been proved by modern experimenters to be erroneous, as also the opinion that chyle was formed by the action of the bile and pancreatic juice upon chyme—thus overthrowing the assertion formerly made that the chyle contains the essence of all kinds of nutriment.

Almande was enabled to establish that all alimentary matters were not digested in the stomach—his observations were made upon a patient having an intestinal fistula, through which he was enabled to see certain kinds of food pass, and to extract those which had not been affected during their passage through the stomach. Subsequent experimenters have established this. Thus the glucose class of aliment is not acted upon by the gastric juice; this fluid does indeed act upon cane sugar, changing it into grape sugar, but here its action ceases, but upon amidon it has no effect. How this class of substances was ul-

*This was the opinion of Blondlow.

timately disposed of has been a subject of much speculation and experiment. Some have supposed that the saliva was the digestive agent, that by its action the nutritive materials were separated; others, that the stomach was the organ for this purpose, and that ultimately they were changed into oleaginous compounds and formed part of the constituents of chyle.

It would appear now that the stomach digests the azotized class of food, or rather that the first stage of the digestion of that class is performed in that organ, and that their digestion is completed in the duodenum by the operation of the bile and pancreatic fluid. The agency of the pancreatic juice in digestion has been until recently confounded with the bile, and many of its effects attributed to that secretion. Thus, when it was ascertained that fats and oils were not digested by the agency of the gastric juice, it was supposed by Müller and Brodie that the digestion of these matters was performed by the bile; it was not until the experiments of Bernard that its true functions were discovered. Physiologists for the most part have held the opinion that the pancreas was merely a salivary gland—some few thought that its office was to supply albumen to the system when there was a deficiency of this principle in the blood. They supposed that the pancreatic juice contained albumen, as it was seen to coagulate when heat was applied, or a strong acid added. Both of these opinions have been proven to be erroneous, but that the office of this secretion is to digest certain alimentary substances which are not acted upon by any other digestive fluid; and also by its union with the bile to complete the digestion of the azotized class which have been first submitted to the gastric juice.

The pancreatic juice is a colorless, slightly viscid fluid, constantly alkaline in its reaction; it is coagulated by heat. Its tendency to putrefaction is very great, and, for this reason, should be always experimented with very soon after being taken from the duct. After being kept for a short time, it ceases to act upon alimentary substances submitted; its appearance alters, it loses its consistency, and will not coagulate when heated. The quantity of the secretion is influenced by the condition of the organ, as is also its quality; thus, when the pancreas is irritated or slightly inflamed, there is a much larger flow; but it will not act as that secreted during a normal condition of the gland; if, however, the inflammatory action is great, the secretion will be entirely arrested; it is similar in this respect to the stomach.

Those who considered the pancreas as a salivary gland, founded their opinion upon the similarity of the pancreatic juice to saliva. The action of each upon amidon was supposed to

be the same, and referred to as evidence of the identity of the two secretions. Had they been more careful in making their observations, they would have seen that there was some slight difference in the action of the two secretions upon this substance. For example, saliva changes amidon first into dextrine, and subsequently into sugar of second class or grape sugar, and, finally, into lactic acid, whereas the pancreatic juice only changes it into grape sugar. This difference of itself should have raised some doubt as to the identity of the two. Magendie, in speaking of the action of saliva, says: "If by an opening into the parotid duct, we remove the fluid of the parotid gland before it reaches the mouth and place it in contact with starch, no conversion into sugar takes place. It is the same also with the saliva drawn from the maxillary ducts in the dog. There is, then, something peculiar in the buccal saliva dependent upon all its constituent principles." Here, then, is a wide difference between the salivary glands and the pancreas; for the pancreatic fluid of itself effects the transformation.

The first class of substances which are acted upon by the pancreatic fluid to which I shall refer, is the amylaceous, or that class, which is capable of undergoing transformation into grape sugar. That the saliva does not, under ordinary circumstances, effect this change can be easily established; indeed, that the action of saliva is mechanical and not chemical, and that its place could be supplied with pure water, is now, I believe, the received opinion. These substances undergo no change in the stomach, as may be seen by giving amidon to a dog, and afterwards extracting the contents of the stomach through a fistulous opening, and testing them. This will also be seen to be the case, if it is digested out of the body with pure gastric juice. If this substance be followed, it will be seen that it disappears in the duodenum. Now, the only secretions which are poured into the duodenum, are the bile and the pancreatic juice, and to one of these must the disappearance of the starch be due. That it is not owing to any action of the bile, may be proved by digesting it with pure bile when no transformation takes place; and, we might now reason, that it must be the pancreatic fluid which digests it, that being the only remaining digestive agent; but, a more conclusive proof is, that if the duct leading from the pancreas be tied, and the contents of the duodenum of an animal previously fed upon starch be examined, it will be seen that no change has been made upon it; and, lastly, if starch is digested with pure pancreatic juice, a transformation into grape sugar will result. That it is now fit for absorption is most probable; as if a solution containing grape sugar be injected into a vein, no trace of it, can be disco-

vered, either in the blood or in any one of the secretions, thus shewing that it has been assimilated, so that it is reasonable to infer that this class of substances do not undergo any further change, as now they are ready for assimilation.

Of its agency in the digestion of fatty matter. It was, for a long time, the received opinion, that the bile was the agent designed for the digestion of fatty matter. This opinion was founded upon experiments made by Müller, and it was not until his experiments were repeated by Bernard, that it was found to be fallacious; for, if pure bile be added to oil free from acidity, no emulsion will be formed; and, if the mixture be allowed to remain a short time, there will be a complete separation, the oil floating as pure and unchanged as it was before it was mixed with the bile. If, now, a small quantity of pancreatic juice be added, an emulsion will be formed immediately, similar in appearance to chyle. From this it might be inferred that the bile, experimented upon by Müller was not pure, or that the oil was rancid; for bile has a peculiar action upon rancid oils forming with them a kind of soap. Here may have been a source of error; most probably, however, that distinguished physiologist obtained the bile from the biliary duct after it had been joined by a pancreatic duct. In the dog there are two ducts leading from the pancreas, one joining the ductus cholecocus, the other entering the duodenum an inch lower; so that the error was natural enough if the bile was taken from the biliary duct after being joined by the pancreatic duct, giving as the result all the phenomena justly ascribable to the pancreatic juice. In order that the experiment should succeed, it is necessary that the pancreatic juice should be perfectly fresh, and the oil free from acidity. When the experiment is performed out of the body, the emulsion will have an acid reaction. This is not the case in the intestine, for it does not lose its alkalinity. This difference is probably due to the oxygen of the air, there being a decomposition of the oil into glycerine and oleic acid, the latter being free gives to the emulsion an acid reaction.

The gastric juice has no effect whatever upon the oils as any one may convince himself by obtaining pure gastric juice from the stomach of a dog, and subjecting the oil to an artificial digestion at the common temperature of the body. It will be seen that the oil will remain unchanged.

Thus it has been shown that neither the gastric juice nor the bile, when experimented with out of the body, has any power in digesting the oils, but that the pancreatic fluid forms with them an emulsion similar in appearance to chyle; but, it may be argued that, although such are the results of the experiments

as performed, that in the intestine the action of these fluids may be different, or that the secretions from the intestine may alter in some way their operation. The following experiment is, I think, conclusive :

If a rabbit be fed upon oil, and its abdomen opened during full digestion, no trace of the oil will be found, but the lacteals will be seen loaded with chyle.

Now, take another rabbit, ligate the pancreatic duct, and feed it in like manner upon oil; after a sufficient time has elapsed, open the abdomen as before, when the oil will be found unaltered in the intestine, nor will there be seen any appearance of chyle in the lacteals.

From these facts two conclusions may be justly drawn ; first, that the oils and fatty matters are solely digested by the pancreatic juice ; and secondly, that chyle is not the essence of all kinds of alimentary matter, but is formed by the action of this fluid upon fatty matter ; for we have seen that none of the other secretions act upon oil ; that that of the pancreas forms an emulsion with it similar in appearance to chyle ; and lastly, that no chyle is formed if the duct of the pancreas has been previously tied.

The pancreas then is the chylopoietic gland.

The pancreatic fluid changes sugar of milk into sugar of grape ; there being this difference between the two, that the former will not undergo fermentation when yeast is added to a solution containing it.

The active principle of the pancreatic juice, to which the changes induced upon the various substances submitted to its action is due, is an organic matter which can be isolated ; for a solution containing this organic matter will effect the same changes as the pure fluid itself ; it has been named *chylopoiene*, from its property of forming chyle ; it may be precipitated by alcohol, and afterwards dissolved by water. There is also another peculiar principle, which has never been isolated, which will be alluded to further on.

Pancreatic juice aids also in completing the digestion of the azotized class of alimentary substances which have been subjected to the action of the gastric juice and bile. This class of alimentary matter, after being introduced into the stomach, is dissolved by the gastric juice, and forms a new compound, the *lactate* of fibrine. When this compound is introduced into the duodenum, a further change takes place when it is mixed with the bile. According to *Platner*, there is a general play of chemical affinities, double decomposition takes place, and two new compounds are formed—lactate of soda and a choleate of fibrine ; the latter is insoluble, and must undergo a further

change before it is fit for absorption and assimilation. This is effected by the intestinal juice, which is the result of the union of the pancreatic and hepatic secretions; there being two peculiar principles in the former, one chylopoiene, which can be isolated, the other, mingling with the bile, forms a third liquid, (intestinal juice,) which completes the digestion of the azotized substances.

All kinds of food are reduced under the action of the different fluids concerned in digestion to three classes, viz :

1st Class, Albuminose.

2d Class, Glucose.

3d Class, Fatty matter.

The Albuminose class is absorbed by the veins and are poured into the general circulation and assimilated. The glucose is absorbed by the veins which empty into the vena porta, and traverse the tissue of the liver. The third class, or fatty matters, having been dissolved by the pancreatic fluid, are absorbed by the lacteals, and constitute the chyle, which probably undergoes a further change in traversing the mesenteric ganglions; it is constantly alkaline. Besides the element already mentioned, chyle contains lymph held in suspension, which has been erroneously supposed by some to be vegetable chyle. It is owing to the presence of lymph that chyle coagulates. After passing through the mesenteric glands, it assumes a rosy hue, which is owing, according to some physiologists, to the admixture of blood. Tiedeman attributed this color to the lymphatics of the spleen.

These discoveries are, I think, calculated to shed light upon certain forms of indigestion, whose seat has heretofore been placed in the stomach; and we may be induced to search elsewhere for those derangements of the functions of digestion placed under the *omnium gatherum* head of dyspepsia. A better plan of treatment may be adopted—a more appropriate diet, one which may leave the suffering organ in a greater state of repose; thus, there have been cases of dyspepsia reported, which have been cured by confining the patient to a diet of fat bacon, which was supposed by most persons to be peculiarly indigestible; here was a mystery which was explained by saying that, what was one man's food was another's poison. Would it not have been more philosophical to say that such a diet did not tax the stomach; that organ being left in a state of repose, nature was enabled to repair the injury induced by the imprudence of the patient; the pancreas in the mean time furnishing the digestive agent? Again: when the stomach is the seat of any chronic disease, do we not recommend a farinaceous diet, because experience has taught us that these articles

of food are of easier digestion? I think that now a better reason can be given, when we know that the stomach is not called upon to aid in digesting them, but that this process is carried on in the duodenum.

We find also in certain forms of dyspepsia, that solid food is the most appropriate diet; now, bearing in mind the functions of the stomach, whose office it is to digest the azotized class of aliments, should we not be led to ascribe the dyspepsia to a derangement of some other organ concerned in digestion; for assuredly, if the stomach was the seat of disease, it would be somewhat paradoxical if it digested well that class of food for which all agree it is principally designed, and that class in the digestion of which it is only remotely or not at all concerned should be rejected. Would it not be more reasonable to refer the seat of disease to those organs whose especial office it is to digest the amylaceous class of substances, and that therefore, this kind of food was rejected, and that the stomach being free from disease, digested as usual the nitrogenized matter, for which it is peculiarly fitted?—*Stethoscope.*

An Inquiry into the character of Green and Melænal Discharges from the Bowels. By SAM'L. G. ARMOR, M. D., Prof. of Physiology, Pathology, and Clinical Medicine, in the Medical Department of the Iowa University.

Diseases of the abdominal viscera, from the frequency of their occurrence either as primitive or consecutive affections, are entitled to much of our consideration; and from the fact of the obscurity, or the want of reliable data from which to draw our conclusions, we should duly consider every thought or fact calculated to throw further light on diseases of the liver, the intestines, and all the other digestive organs.

Of the various secretions poured into the digestive apparatus, none have been regarded with more interest than that of the liver; yet the secretions of no gland of the body have, at the same time, been less accurately studied. A comparatively unexplored field of inquiry here presents itself to the chemical pathologist: and we are persuaded that chemical analysis of bilious stools and other stercoral matters, will one day furnish signs of great value in the diagnosis of diseases of the liver and other abdominal viscera.

By common consent, especially in the Western States, the liver is thought to be a frequently offending organ; it is made the common "scape goat" for offences of which, in many instances, it is not guilty; and in accordance with a popular idea, that

it is the principal outlet through which impurities of the blood are poured, mecurial and other cholagogue remedies have attained, we think, an undeserved popularity.

That sluggish or perverted action of the liver is soon followed by general morbid action, we would readily infer from the importance of the office it performs in the animal economy. The blood would be morbidly impressed if its hydro-carbon were not removed by the liver; and the digestive functions would also suffer if healthy bilious matter were not duly elaborated. It may be said, then, to have both a *forward* and *backward* action,—forward on the surface on which the secretion is poured, and on the contents of the duodenum, and backward on the gland itself and on the blood from which it is secreted.

The investigations of pathologists, however, into the usual changes noticed in the biliary secretion and excretion, have had reference rather to *quantity* than *quality*,—to its increase in bilious flux, or its suppression in cases of jaundice arising from occlusion of the excretory ducts. But if we push our investigations beyond this, by enquiring into the different modifications which health and disease may effect in the composition of the bile, we will find ourselves at once groping in the dark. Physicians, it is true, have described a multitude of changes in the bile, by the aid of which they have explained the nature of many diseases; yet we may search the records of practical medicine in vain, from the times of Galen to the present, for well authenticated facts, or positive proof, in support of these views.

The subject has recently occupied the attention of Dr. Golding Bird, whose researches in chemical pathology, are always of the most accurate and interesting character. He has directed his attention chiefly to the *green alvine evacuations of children*, “with the view of testing the accuracy of the popular opinion of their being chiefly composed of bile.” This view has long been maintained, or rather *assented* to, by the medical world. The evacuations, usually termed malæna, have been also regarded as depending on vitiated bile; but more recent investigations favor the view that this character of discharge is occasioned by a morbid exudation from the intestinal mucous membrane. It is true that it is frequently,—perhaps always, connected with either functional or structural disease of the liver; and the association being so uniformly present, Langstaff, in the absence of more positive evidence, was lead to attribute it to a morbid sympathy between the liver and intestines; while others, accommodating themselves to a simple and more commonly received opinion, attributed it to an increased activity of the liver in throwing off crude bilious matter; whereas the true

cause is referable, according to Dr. Bird, not to sympathy, nor to excessive action of the liver, but to congestion of the portal venous system.

It has been demonstrated by frequent experiment, that the composition of human bile is essentially similar to that of the ox. That we may have an accurate idea, then, of the normal constitution of this fluid, I present the following analysis of the bile of an ox, as given by Berzelius :

Water,	-	-	-	-	-	904.4
Bilin, (with fat and coloring principles)	-	-	-	-	-	80.0
Mucus, chiefly from gall bladder,	-	-	-	-	-	03.0
Salts,	-	-	-	-	-	12.6
						1000.0

That bile is often present, and even in large quantities in fæcal dejections, cannot admit of a doubt; but that it is *necessarily* present in the green discharges, so common in early infancy, is the point in dispute.

A specimen of a green evacuation, passed by a hydrocephalic infant whilst under the influence of mercury, was carefully analyzed by Dr. Bird. It was a dirty green, turbid fluid, and by repose, separated into three distinct portions. 1. A supernatant oily fluid, presenting a brilliant emerald color. 2. A dense stratum of mucus, coagulated albumen and epithelial debris. 3. A deposit of large crystals of triple phosphate of magnesia and ammonia in prisms of an apple green color. The supernatant green fluid was decanted for examination, and its chemical elements indicated by the following

Analysis.

1. 1000 grains of the green fluid left, by careful evaporation, a deep olive green, highly deliquescent extract, weighing 100 grains.

2. This extract (1) being immersed in alcohol of 0.837, formed a mass like bird lime, which could not be mixed with the spirit. Even after long boiling, it appeared hardly to diminish in bulk. The clear tincture being decanted, left, however, an extract weighing 30 grains. This residue possessed the yellowish green color of faded leaves, an odor of fresh broth, and a sweet, sub-astringent taste, with a very slight admixture of bitterness.

3. The alcoholic extract being carefully incinerated, left 5.5 grains of ashes, consisting chiefly of chloride of sodium mixed with mere traces of tribasic phosphate of soda (3 Na, O, P2 O5.) It was alkaline, but did not effervesce with acids.

4. The portion left undissolved by boiling alcohol yielded to water 13 grains of nearly tasteless matter, which by incineration, left a powerfully alkaline ash, weighing 1·75 grains, not effervescing with ashes, and consisting nearly exclusively of alkaline tribasic phosphate of soda.

5. The residue, insoluble both in water and alcohol, weighed 57 grains, and consisted almost entirely of coagulated albumen, dry mucus, and modified blood. It left by incineration 1 grain only of ashes, consisting almost wholly of brick-red sesquioxide of iron.

The following is a view of the results of the examination:—

Alcoholic extract,	} Organic,	34·50
	} Inorganic,	5·50
Aqueous extract,	} Organic,	11·25
	} Inorganic,	1·75
Insoluble matter,	} Organic,	56·00
	} Inorganic	1·00
Water and volatile matter,		900·
		<hr/>
		1000·

The organic portion of the alcoholic extracts consisted chiefly of fatty matter, cholesterine, and a green substance supposed to be identical with the so called *biliverdin*, with but a mere trace of bile, barely sufficient to communicate a bitter taste to the extract, but not sufficient to leave any carbonate of soda in the residue of incineration. The aqueous extract consisted chiefly of ptyalin, with other extractive matters.

The composition of the fluid part of the green evacuation is thus expressed:

<i>Biliverdin</i> , alcoholic, extractive, fat, cholesterine, with traces of bile,	- - - - -	24·5
Ptyalin, aqueous extractive colored by biliverdin,	- - - - -	15·21
Mucus, coagulated albumen, and hæmotosine,	- - - - -	56·0
Chloride of sodium, with traces of tribasic phosphate of soda,	- - - - -	5 5
Tribasic phosphate of soda,	- - - - -	1·75
Sesquioxide of iron,	- - - - -	1 0
Water,	- - - - -	900·
		<hr/>
		1000·

An analysis of a green calomel evacuation has been recorded by Simon; he has not given the proportion of solids and water present, but merely detailed the composition of the dry extract, which consisted of

Soluble in alcohol	{	Bile, bilifelic acid, biliverdin,	21.4	}	42.4
		Fat, containing cholesterine,	10.0		
		Alcoholic extractive, - - -	11.0		
Ptyalin, aqueous extractive, - - - - -					24.30
Albumen, mucus, epithelial cells, - - - - -					17.10
Saline matter, - - - - -					12.90
					<hr/>
					96.7
Loss - - - - -					3.3
					<hr/>
					100.

Professor Kerstein, of Freiberg, has published some late researches on the nature of green stools said to be of frequent occurrence in patients who are under a course of certain mineral waters. He altogether denies that any quantity of bile is present in the green evacuations of invalids and others who partook of the Carlsbad waters; but attributes the tint to the green sulphuret of iron, generated in the stomach and intestines by the reduction of the sulphate of soda of the water, to a sulphuret, and its subsequent action on the iron held in solution in the water. He states in confirmation of his view, that hydrochloric acid destroys the green color of the stools, evolving at the same time sulphuretted hydrogen. Dr. Frankl, however, of Marienbad, attributes the color of these evacuations to the 'same source' as the green mucous discharges from the vagina in leucorrhœa, the urethra in gonorrhœa, and the nasal secretions in some forms of coryza.

The following extract will give the reader a general idea of Dr. Bird's views:

"I have assumed that the green color of the matter examined was owing to *Biliverdin*, a conventional term for a substance very imperfectly understood, and very likely applied to substances essentially distinct in their nature. Berzelius has compared biliverdin to the chlorophylle, or green coloring matter of leaves, although this must be regarded as partaking rather of a wax-like nature than as a mere colored extractive. It must, however, be borne in mind that green coloring matter may be possibly generated in the animal economy from the action of certain matters on the hæmatosine, or coloring matter of blood. Thus it is well known that when blood is exposed to the influence of sulphuretted hydrogen gas, it acquires a deep olive-green color when viewed by reflected; and a dingy red, by transmitted light,—phenomena identical with those presented by the coloring matter of bile. Attention has been drawn to this remarkable fact by Professor Leopold Gmelin. It is now ten years ago since a series of researches on the action of oxydating agents upon blood were published in the pages of this journal by Dr. Brett and myself. In that paper

we described two products of the action of nitric acid upon clot of blood,—an olive-green sweetish astringent substance, and an intensely bitter yellow one; we applied the conventional term of chloro-hæmatin to the former, and xanthe-hæmatin to the latter.

Since then, the coloring matter of blood is fully capable of being converted into green pigments under the influence of different agents, it must, I think, be admitted, that we are not to assume the green color of an animal excretion as of necessity depending upon the presence of an excess of bile. And when chemical analysis fails to indicate the presence of any quantity of this secretion in a bright green evacuation, it is but legitimate to seek for some other cause of this tint. The proportions of the so called biliverdin very closely approach to those of the xanthe-hæmatin before alluded to, and I confess that I am induced to regard the green color of the emerald and ‘chopped spinach’ stools of children as depending upon the presence of modified blood, rather than on an excess of bile.

Believing that the green stools alluded to are but a form of malæna, I have often closely questioned the nurses of children voiding them, regarding the appearance of the evacuations before and after the development of the green color, and have almost constantly been told that streaks, or even clots of blood had been observed.

I regard, then, the presence of green stools as indicative not of a copious secretion of bile, but of a congested state of the portal system, in which blood is exuded very slowly, and in small quantities, so as to allow of the color being affected by the gasses and secretions present in the intestines; a state of things capable of readily ending in melæna, in which the effusion of blood is so copious and sudden as not to give time for the occurrence of the changes alluded to.

There is, moreover, a peculiarity in the green dejections of children and others whose portal circulation is congested, which, so far as I know, is quite distinct from any property presented by mere bile under similar circumstances—I allude to the effect of exposure to the oxygenating influence of the air upon them. When first voided, the ‘chopped spinach’ stools, are in the majority of cases of a bright orange color, and they assume their characteristic grass green hue only after exposure to the air. The time required for this change varies remarkably. I have seen an orange colored stool become green in a few minutes; and in the same patient, only a day or two afterwards, many hours may have been required to effect the same change.”

The fact that these discharges most generally take place in the warm summer months, favors the view of Dr. Bird. The debilitating effects of heat, when combined with moisture, very readily develop diseases of the gastro-intestinal mucous membrane. In the first place there is a direct relation, based on the general law of identity of structure, between the skin and mucous membrane; a morbid impression produced on the one, is frequently communicated to the other. In addition to this, sudden changes from the extreme heat of mid-day, to the damp

chill of evening, produce intropulsions of blood. The abdominal viscera suffer from this congestion; their vital properties are lowered, and their functions illy performed. The liver, over stimulated by blood largely charged with hydro-carbon, while it is at the same time deprived of its normal amount of serosity, does not secrete its usual amount of bile. It is overtasked; and the result of its engorgement, occasioned as well by a defect of its natural secretion as by intropulsive congestion, is a damming up of the venous circulation of the intestinal canal; for it will be borne in mind that the abdominal circulation, in passing to the lungs, first passes through the liver. Whatever, therefore, arrests a free hepatic circulation must also effect the abdominal circulation; and this condition, we could very readily believe, might, under peculiar circumstances, be followed by percolation of more or less of the elements of the blood through the mucous coat of the intestinal canal; and this very depletion would tend to relieve the congestion of the liver, and thereby enable it to resume its function. Is this a satisfactory explanation of the change for the better which generally follows those dark discharges? However this may be, in the absence of more correct knowledge of the changes that bile undergoes in disease, we should carefully analyze every thought and fact calculated to throw further light on a subject of acknowledged obscurity. If Dr. Bird's views be correct, (and they are certainly plausible,) they will very much modify the popular notion of the pathology of abdominal affections, characterized by these green or melænal discharges.—*Western Medico-Chirurgical Journal*.

A Family poisoned by eating a Gar. By Dr. W. BROOKS, of Circleville, Illinois.

Amid the many and varied forms of diseased action the destructive ravages of which, the Physician is called upon to aid in counteracting, it is rare to meet with such a case as I am about to relate.

April 27, 1850, I was called at 1½ o'clock A. M., to attend upon a family residing in this village, said by the messenger to have been poisoned. I immediately hastened to their assistance, and on entering their apartments the scene that presented was one well calculated to elicit the sympathies of the most obdurate heart. There were no less than five persons suffering in all the agony that pain could inflict. I inquired into the cause and got the following history: A party had been on a fishing excursion the day before, and had been so unsuccessful as to return with nothing but what is vulgarly called a Gar, (a

species of fish quite abundant in many of the western rivers, which has a long pointed horny mouth like the shark, and a hard scaly skin.) The family being anxious for fresh fish, the Gar was skinned, dressed and par-boiled, it together with *the* eggs were then fried and served up for the table. The whole family partook heartily of the rare delicacy and admired the fish. At 11 o'clock in the evening they felt a burning sensation in the pit of the stomach, which was soon succeeded by vomiting, purging, and cramping, in their most aggravated forms. The pulse was small and frequent. Tongue somewhat swollen, with a red serrated margin, and a dark yellow fur along its centre. The matter ejected from the stomach was very green, and very offensive. Stools not dissimilar in appearance from those of a case of dysentery. Cold sweats, alternating with flushed and suffused countenance.

Treatment.—I ordered a sinapism applied to the epigastrium, and administered the following: \mathcal{R} camph. 3 grs., Flos. sulph. 3 grs. opii $\frac{1}{2}$ gr. The first second and third doses were rejected as soon as they came in contact with the irritated stomach, the fourth was retained longer. I found that they were effecting some good, and persisted in their use, giving another as soon as the previous one had been ejected from the stomach. Within an hour from the time I gave the first dose of the above mixture, all of the alarming symptoms had, excepting one case, disappeared. I left orders for the powders to be given every two hours until I should see them again.

At 10 o'clock the same day, all better but the one that seemed most obstinate to manage in the morning. This patient vomits and purges still, occasionally, with some fever, furred tongue, and offensive breath, complains of pain in the stomach and bowels. Prescribed cal. camph., and sulph. morphia in small doses every two hours.

28th. Found patient this morning in a comatose state, but had rested easy all night. Pulse hardly perceptible at the wrist, free from all pain, the surface of the body, hands and feet very dry. Tongue slightly furred and a foul breath.

Prescribed cal. camp. and morphia, in order to procure an action from bowels, as there had been no evacuation from 4 o'clock on the previous evening. This to be followed by enemata should it be deemed necessary.

29th. Patient this morning suffering only from general prostration, put her on Dover's powders and quinine, and dismissed her as cured. I had neglected to say that my other patients were similar in most respects in symptoms and treatment to the one already described. The foregoing, peculiar and novel as it is, nevertheless is a true "fish story."—*North-Western Med. and Sur. Journal.*

Local Anæsthetic Agents.—M. Aran has recently made a report to the Académie des Sciences upon local anæsthetic agents, a notice of which we take from the Journal des Connaissances, Jan. 1851. The experiments he has made with chloroform to produce local anæsthesia have been very unsatisfactory. Chloroform does not always induce insensibility, and its application is always attended with a very disagreeable burning sensation, which sometimes annoys the patient more than the disease for which it has been applied. M. Aran has found a large number of agents that possess a local anæsthetic power, and particularly the classes that are hydro-carbonated and chloro-hydro-carbonated. The agent he most prefers is a substance discovered by M. Regnault, and called *chlorated hydrochlorine ether*. It is easily applied, is more certain in its action, and is less irritating than the others. The sesqui-chloride of carbon may also be used, but its action is very slow, as it takes about two hours for insensibility to be produced. Fifteen or thirty drops of the chlorated hydro-chlorine ether, poured upon a painful part, or upon a compress, and this applied over the seat of pain, will soon calm the suffering and produce anæsthesia in a very few minutes. An ointment may be made of it and applied:—

Chlorated hydro-chlorine ether, 4 grammes,
Cerate, - - - - - 20 “

Or an ointment of sesqui-chloride of carbon may be used:—

Sesqui-chloride carbon, 4 grammes,
Cerate, - - - - - 30 “

Anæsthesia produced by these agents extends to the deep-seated parts. The insensibility produced by them lasts in the physiological state from half an hour to an hour, but longer when applied to relieve pain. M. Aran recommends their use very highly in muscular rheumatism or neuralgia. He says they are very useful in articular diseases, as they annul the pain and thereby permit surgical manipulations and applications to be made without torturing the patient. He has used them with considerable success in painful diseases of the viscera, in nervous and lead colics, in uterine diseases and in pleurisy and pericarditis. In these diseases he either obtained a complete abolition or a great amelioration of the pain.

Aneurisms.—We find in the Archives Générales an interesting communication upon Aneurisms, by M. Chassignac, an epitome of which we will give. He first speaks of the clots or coagulation of blood that are sometimes formed in an Aneurismal Tumor. It frequently happens that though these clots are formed, they do not entirely close the arterial canal, but there exists a passage for the blood through the substance of the coagulum. When this is the case the canal does not pass through the centre of the clot, but invariably to one side, the canal existing nearer the side that is least dilated. These canals are frequently lined with a delicate but resisting membrane.

The author next attempts to show that varicosed aneurism can be readily distinguished in all cases, from all other forms of aneurisms, by the character of the sounds heard on auscultation. The sound heard in a varicosed aneurism is harsh, tumultuous, continuous and trembling, and very different from the fine blowing sound heard in other aneurismal tumors. A peculiarity in the sound produced in a varicosed aneurism is that it is propagated above and below the tumor in the venous branches that are connected with the punctured vein. Thus in a varicosed aneurism of the crural artery and vein, the characteristic sound will be propagated to and be distinctly heard in the posterior tibial vein, behind the internal malleola. In an ordinary aneurism there is a blowing or rasping sound, purely intermittent that is not propagated above or below the tumor.

M. Chassignac thinks that perhaps the propagation of the sound in varicosed aneurisms may be explained by supposing that there is a chemical change that takes place by the constant mixture of the arterial and venous blood. He gives this explanation as an hypothesis.

Arteries, says the author, are endowed with considerable sensibility. In tying arteries, however well isolated they may be, there is always pain and it is sometimes very acute. This pain cannot be caused by nervous filaments being included in the ligature, for it is seated at the point of the ligature, whereas if nervous filaments had been included the pain would have been seated along the course and at the extremity of the nerve.

Gastralgia.—The Journal des Connaissances extracts from the Revue Thérap. du Midi, a notice of the prescription that M. Caizergnes has been in the habit of making in Gastralgia. The late Professor considered the combination of sub. nitrate bismuth and the extract of belladonna as a veritable specific in this complaint:

℞. Sub. nitrate bismuth - - 10 grammes,
 Ext. belladonna - - 1 “

To be made into 40 pills and are administered night and morning. These pills are believed to relieve, not only the nervous symptoms, but the general derangement of the system which sometimes accompany them, as chlorosis.

Itch Ointment.—M. Bazin recommends, in the Journal des Connaissances, very highly, an ointment made of chamomile flowers as a remedy for the itch. He says, that it removes immediately the itching, and does not produce any eruption when applied, as do many other ointments.

℞. Powder of Chamomile flowers, }
 Oil, } aa 500 grammes.
 Cerate, }

M. Bazin proposes that, in ordinary cases, the ointment of Bayard should be used, which is the following:

℞. Sulphur sull, }
 Gunpowder } aa 8 grammes.
 Yolk of an egg,
 Olive oil, 40 grammes.

Treatment of Priapism.—M. Listach recommends, in a recent report to the Académie de Médecine, a compression of the prepuce by a ligature or with the fingers, to relieve obstinate priapism. If the object is to prevent nocturnal erections, the prepuce should be drawn in front of the gland, and a ligature passed around it sufficiently tight to retain it in that position. If it is desired to relieve an existing erection, the prepuce should be drawn in front of the gland and held in that position for a short time.—*Journ. des Connaissances.*

Miscellany.

AMERICAN MEDICAL ASSOCIATION.

The annual meeting of this Association was held in Charleston, S. C., on Tuesday, the 6th of May. As the proceedings of this body are of general interest to the Medical profession, we have given them as they were reported for the Charleston Courier. The Association adjourned on Friday afternoon to meet in Richmond, Va., on the first Tuesday in May, 1852. From the abstract which follows, it will be perceived that a great and highly beneficial change has been made by the substitution of committees on special subjects for the standing committees, whose voluminous reports abstracted from the journals, were the subject of such general and deserved disapprobation.

This Association met at St. Andrew's Hall, Broad-street, their President, Dr. Mussey, in the Chair, and Dr. H. W. Desaussure, Secretary.

The Association having been organized, Dr. Thos. Y. Simons, the Chairman of the Committee of the South Carolina Medical Association, in a warm and hearty address, welcomed the Delegates present from the other States, to the city and State, on behalf of his associates, which was responded to in a becoming manner by the President.

Dr. Frost, on behalf of the Committee of Arrangements, read the list of Delegates present.

The President of the Association read a letter from Dr. Stilé, resigning his office, in consequence of the impaired state of his health.

On motion of Dr. Arnold, of Savannah, Ga., it was proposed that the letter of Dr. Stilé be placed on record, in compliment to him, for the interest he has manifested in the Association.

Dr. Arnold offered the following resolution, which was adopted :

Resolved, That a committee of one from each State represented in the Association, to be chosen by their respective Delegates, be appointed to nominate suitable officers to be elected for the ensuing year.

On motion of Dr. Frost, the Association took a recess of ten minutes to enable the Delegation to appoint one of their number a member to constitute the nominating Committee, in compliance with the above resolution.

The President of the Association, at this stage of the proceedings, read an address of some length on matters connected with the profession, and the advancement of Medical science, which circumstances prevented us from hearing, but which we learn was well received, and elicited the commendation of those present.

On the re-assembling of the Convention, the President reported the following gentlemen, as having been selected by said committee from the different State Delegates, viz : Dr. Geo. Mendenhall, of Ohio ; B. R. Wellford, of Virginia ; Joseph Fithian, of New Jersey ; R. D. Arnold, of Georgia ; G. W. Miltenberger, of Maryland ; H. R. Frost,

of South Carolina ; N. G. Pittman, of North Carolina ; W. H. Anderson, of Alabama ; A. H. Stephens, of New York ; Usher Parsons, of Rhode Island ; Jos. Carson, of Pennsylvania ; H. Adams, of Massachusetts ; Thos. Reyburn, of Missouri ; Jas. Jones, of Louisiana ; W. Parsons, of Rhode Island ; J. B. Flint, of Kentucky ; John Sloan, of Indiana ; C. Boyle, of the District of Columbia, and J. B. Lindsley, of Tennessee.

The nominating committee, through their Chairman, then read the subjoined names as suitable candidates for officers of the Association for the ensuing year, viz :

Dr. JAMES MOULTRIE, of S. C., President.

Dr. GEO. HEYWARD, of Mass ; Dr. R. D. ARNOLD, of Geo. ; Dr. B. R. WELFORD, of Va. ; Dr. J. B. FLINT, of Kentucky, Vice-Presidents. Dr. H. W. DESAUSSURE, of S. C., and Dr. ISAAC HAYS, Secretaries. Dr. P. C. GOOCH, Treasurer.

On motion of Dr. La Roche, of Penn., the report was accepted, and the gentlemen thus nominated be the officers of the Association for the ensuing year. This motion was adopted, and the officers were invited to take their seats.

The President elect then took the chair, and in a few appropriate remarks, returned his thanks for the honor thus conferred on him by the Association.

The Secretary read a report transmitted to him from the committee of Unfinished Business, appointed at the Session of 1850.

On motion of Dr. Arnold, of Georgia, the Report was accepted, and laid on the table.

On motion of Dr. Gaillard, of South Carolina, the following resolution, offered by Dr. Drake, of Cincinnati, at the Session of 1850, be taken up for consideration :

Resolved, That the second section of the Regulations of the Association, be so amended as to require that candidates for membership by invitation, be nominated in writing by five members ; that when elected they shall enjoy all the rights of Delegates, and that all permanent members shall be entitled to vote.

After some discussion, on motion of Dr. A. H. Stevens, of New York, the Resolution was referred to a Committee, consisting of Drs. Drake, of Ohio, Wood, of Penn., and Wellford, of Virginia.

Dr. Stevens, of New York, offered the following resolution, which was discussed by Drs. Storer, of Mass., and Moore, of Georgia, and finally rejected.

Resolved, That a committee be appointed to report to the Association the business before it, and to offer such suggestions as they may deem advisable for the due discharge of the same.

On motion, the Association adjourned to meet to-morrow morning at 10 o'clock.

MORNING SESSION.—MAY 7TH.

The Convention met this morning pursuant to adjournment, the President in the Chair. The minutes of the previous meeting were read and confirmed.

Dr. Wood asked and obtained leave to read the following report, on amending the Constitution, on behalf of himself and Dr. B. R. Wellford:

The committee to whom was referred the Proposition of Dr. Drake, for an alteration of the rules in relation to the admission and rights of members, have the honor to report as follows:

There are two distinct branches of the proposition; the first of which, relates to the invitation of medical men, not delegates to participate in the proceedings of the Association; the second, has in view the extension of the right of voting to permanent members.

The committee agree in the general purport of the first part of the proposition. As it now stands, the rule admits of a too easy admission to the privileges of members, and it is susceptible of great abuse. It might happen, in a place where the number of resident Physicians was very considerable, that sufficient might be introduced to control the decisions of the delegates. To guard against such a result, the committee recommend, that, in addition to the provision that none should be invited by the Association, unless upon a previous written proposal, by five delegates, the existing rule should be so altered as not to confer upon the invited members the privilege of voting.

In relation to the second part of the proposition, that, namely, which gives the privilege of voting to permanent members, the committee do not consider its adoption advisable, on the following grounds: This Association is essentially a representative Body. Its opinions are supposed to be those of the Societies or Associations by which the delegates are appointed, and go forth to the world with the authority in some degree of the medical profession generally. Now, if permanent members were permitted to vote, they would express their own individual opinions, and support their own individual preferences; both of which might be in direct opposition to those of the delegates, and not fairly representative of general medical sentiment. It is easy to conceive, that combinations among permanent members might be formed, more powerful than the properly delegated body, which might thus be overruled in its decisions. The opinions or wishes of a comparatively few individuals might thus go forth to the world, as those of the profession at large; and private purposes might be answered at the expense of the general good. This would defeat the main objects of the Association, and prevent it from continuing, what it may now be considered to be, the exponent of enlightened medical sentiment in this country.

The committee, therefore, recommend that the question on Dr. Drake's proposition be taken separately upon its two branches; that the first be adopted with a modification, withholding the right of voting from invited members; and that the second, which confers this right upon permanent members, be not adopted.

GEO. B. WOOD,

B. R. WELLFORD.

Charleston, S. C., May 7, 1851.

Dr. Drake then read the subjoined minority report:

The undersigned, a minority of the committee, to whom was referred

the resolution for amending the second section of the Constitution, begs leave to report, that in his opinion, it is expedient and will be found promotive of the great objects for which the Association was formed, that "members by invitation" should not be admitted, except under a written nomination, by five members; that when thus chosen, they should enjoy all the rights and privileges of Delegates, including permanent membership; and that all permanent members should be entitled to vote. With these views the undersigned respectfully submits a revision of the resolution into the following:

Resolved, That members by invitation shall be nominated, in writing, by five members, which nomination shall be made a matter of record; that when elected they shall enjoy the rights and privileges of Delegates, and remain as permanent members of the Association.

Resolved, That all permanent members shall have the right of voting.

Respectfully submitted,

DAN. DRAKE.

Dr. J. Hays moved to take up the majority report, which motion was carried.

Dr. Arnold spoke against the article of the Constitution, authorizing invited members to vote.

Dr. Wood explained his report, and urged its adoption.

Dr. Davis, of Chicago, said that there was much misunderstanding in regard to the intention of the Constitution in respect to the members by invitation. He hoped that the Constitution would be strictly acted up to, and that members should be invited only "from sections not otherwise represented."

Dr. Wood said his was an amendment, and not a repeal of the old provision.

Dr. Drake responded—He had waited for arguments against his resolution, but had heard none. He then entered into a long argument in favor of *popularizing* the Association with the Profession in the United States, and took ground in favor of a permanent place of meeting at Washington City.

Dr. W. Atlee, of Lancaster, said he could see no harm in giving the privilege of voting to invited members who came from unrepresented localities, but was opposed to the right of voting proposed to be given to permanent members.

Dr. Meigs, of Philadelphia, asked whether a gentleman would be invited to attend without any privileges, and went on to say that he hoped the Convention would have five, ten or even twenty thousand in attendance at some future period.

Dr. Hooker, of Connecticut, begged to be allowed to offer the following resolution, the resolution of Dr. Drake, having been laid on the table for the present,

Resolved, That no member be permitted to speak longer than 10 minutes at one time in any one debate.

Dr. Philips, of New York, offered to amend the resolution by inserting "15," which motion was lost.

The resolution as offered by Dr. Hooker, was then adopted.

Dr. Hays moved to lay the subject on the table, and added that by a constitutional provision it was required to lay over one year.

The motion was seconded by Dr. Tucker, of Virginia.

Dr. Dickson, of South Carolina, asked if the motion swept off the whole resolution, and was answered affirmatively by Dr. Hays.

Dr. ———, said if the matter was postponed now they would not be out of difficulty, because all that is necessary to defeat it next year would be to move to amend it, and it would have to lay over a year again, &c.

The matter was finally laid on the table.

Dr. Wood, of Pennsylvania, called up the second part, or that portion giving to Permanent Members the right to vote.

The majority committee, accepted the substitute of the minority committee, which was read as follows, viz :

Resolved, That all permanent Members shall have the right of voting.

Dr. Dickson urged the adoption of the above resolution.

Dr. Hays, of Philadelphia, remarked that the Constitution had not been studied by the gentleman who had urged the adoption of the Resolution; and spoke in opposition to the measure.

Dr. Thompson, of Delaware, supported Dr. Hays, and hoped that the whole matter would be laid over for a year.

Dr. Dickson observed that he had been accused of ignorance of the Constitution. He hoped to have these gentlemen always here to instruct him.

Dr. Bond, of Maryland, took part in the discussion.

Dr. Adams, of Massachusetts, remarked that they ought to strike out the words—"Permanent Delegates"—from the Constitution, and was proceeding with his remarks, when the gentleman was called to order.

The question was here taken on the adoption of the Resolution, which was lost by a large majority.

Dr. I. Hays, the Treasurer of the Association, then read the Report of the Committee of Publication, and also the Report of the Treasurer.

The subjoined Resolutions, appended, were read and adopted.

1. *Resolved*, That the assessment for the present year shall be three dollars.

2. *Resolved*, That those Delegates who pay the assessments, shall be entitled to one copy of the transactions for the present year; and that the payment of two dollars, in addition, shall entitle them to two additional copies.

3. *Resolved*, That permanent members shall be entitled to one copy of the transactions for the present year on the payment of two dollars, and three copies on the payment of five dollars.

4. *Resolved*, That Societies which have been represented in the Association shall be entitled to copies for their members on the same terms that copies are furnished to permanent members.

5. *Resolved*, That permanent members, unless present at the meeting as Delegates, shall not be subject to any assessment.

6. *Resolved*, That any Delegate who is in arrears for his annual assessment shall not be considered as a permanent member.

7. *Resolved*, That the several committees be requested to bring to the meeting of the Association, their reports, correctly and legibly

transcribed ; and that they be required to hand them to the Secretaries as soon as they have been read.

All of which is respectfully submitted.

ISAAC HAYS,
Philadelphia, April 20, 1851. D. FRANCIS CONDIE.

Dr. Drake, of Ohio, moved that the Report on Surgery be read first. Adopted.

Professor Eve, Chairman committee on Surgery, then proceeded to read his report.

A motion was made by Dr. Davis to commit the same to the committee on Publication ; which was adopted.

Dr. Hays moved to read Dr. Flint's Report by its title—Practical Medicine—and refer the same to the Committee on Publication, which motion was adopted, and several hundred copies printed and furnished by the author were directed to be distributed.

A motion was then made to adjourn till 5 o'clock, P. M., which was adopted, and the Convention adjourned.

AFTERNOON SESSION.

The President having organized the meeting, Dr. Boyle, of Washington, offered a resolution that the Association in future meet at Washington City. Dr. C. P. P. Johnson, of the Virginia Medical Society, extended an invitation to meet at Richmond ; Dr. Jones, of the University of Louisiana, to meet in New Orleans ; Dr. J. P. Johnson, of Missouri, to meet in St. Louis. The resolution and invitations were referred to the Committee on Nominations.

The President suggested to the Society, the propriety of appointing the Standing Committees at an early date.

Dr. Wood remarked that there was a proposition to abolish Standing Committees.

Dr. Hays said he was opposed to those Committees, but would not press an alteration.

Dr. Tucker moved that the appointment of the Standing Committees be referred to the Committee on Nominations, which motion was adopted.

Dr. Jones, of Louisiana, resigned as a member of the Committee of Nominations ; and Dr. Fenner, of New Orleans was appointed in his place.

Dr. Parsons then moved that the Committee on Nominations be requested to resume its labors, which was adopted.

Dr. Wragg, of Charleston, moved that the Report of the Committee on Prize Essays, be read, and then the Obstetric Report be brought up.

The Report on Prize Essays was then read, and certain resolutions appended thereto were adopted.

When, on motion of Dr. Ready of South Carolina, the whole matter was referred to the Committee of Publication.

Dr. Storer, of Boston, Chairman of the Committee on Obstetrics, read the Report on that subject. He stated that he had received a letter from Dr. Thompson, of Illinois—that he was the only member

of the committee who had aided him in any degree. He mentioned this fact, because he had to hold himself entirely responsible for all the inaccuracies, &c.

Dr. Phelps, of New York, moved that the Report be referred to the Committee of Publication.

Dr. Robertson, of South Carolina, moved that the statistics, alluded to in the Report, be stricken out, as the author was not a reliable man.

Dr. Storer seconded the motion.

Dr. Bond moved to postpone the Report until morning which was seconded by Dr. Gilman.

A short debate here ensued; when it was finally agreed to re-commit said portion of the Report, to be corrected and laid before the Association in the morning.

On motion, the Convention adjourned to meet to-morrow morning.

MORNING SESSION.—MAY 8TH.

The President, Dr. James Moultrie in the Chair. The minutes of the previous meeting were read and after some slight amendments were confirmed.

Dr. J. M. Smith, of Mass., moved that the Report of the Committee on Medical Education be made the special order, after the disposal of the Report of the Committee on Obstetrics.

Dr. Gaillard, on behalf of the Committee of Arrangements, read a list of Delegates reported as registered since the last report.

Dr. Campbell, of Georgia, presented a model of a mal-formation of the knee joint, the patella being absent.

Dr. Wood, of Penn., offered the following resolution:

Resolved, That Colleges, exclusively of Dentistry and Pharmacy are not recognized by the Association, as among the bodies authorized to send Delegates to its meetings.

Dr. Wood, of New York, moved to amend, by dividing the resolution, so as to take the question, first, on the reception of Delegates from Colleges of Dentistry; secondly, on the reception of Delegates from Colleges of Pharmacy.

The amendment having been accepted, the question of the reception of Delegates from Colleges of Dentistry was debated.

Dr. Lamb moved an indefinite postponement of the Resolution, which was lost.

Dr. Yardley, of Pennsylvania, asked and obtained leave to read the subjoined Resolution, presented by the Philadelphia County Medical Society.

Resolved, That all the Medical Colleges in the United States are hereby earnestly and respectfully requested to hold a Convention, through Delegates respectively chosen by them at least once in every six years, to take into consideration the proper method of harmoniously elevating the standard of Medical education in the said Colleges.

The discussion of the original question was then resumed.

A motion was finally made by Dr. Hays of Pennsylvania, that the whole resolution of Dr. Wood, including colleges of Dentistry and

Pharmacy, be referred to a special committee of five members, which resolution was adopted.

On motion of Dr. Yardley, of Pennsylvania, the resolution presented by the Philadelphia county Medical Society was also sent to the same Committee.

Dr. Jones, of North Carolina, offered the following resolution :

Resolved, That all the Medical Colleges in the United States, are hereby earnestly and respectfully requested to hold a Convention, through Delegates respectively chosen by them, at least once in every six years, to take into consideration, the proper method of harmoniously elevating the standing of Medical Education in the said Colleges.

The order of the day was then called up, when Dr. Storer reported that he had erased the statistics referred to yesterday, and that he placed the report in the hands of the Association. Dr. S. said that there was objections to the remarks on the subject of Dr. Gilman's paper on the speculum reilm. He asked that he be permitted to remove the unnecessary expression of opinion in regard to that subject. He further added that he had taken from the journals these facts, &c., and was not therefore responsible for the correctness of the papers, &c.

Dr. Bond, of Md., remarked that there were charges in these reports which he did not individually endorse ; but which go out in a book, under the sanction of the Association.

On motion of Dr. Davis, the report was referred to the Committee of Publication.

At this stage of the proceedings, Professor S. S. Holdeman, of Lancaster, City, Pennsylvania, through Dr. John L. Atlee, presented to the Association an Essay on Latin Pronunciation, of which he is the author ; and which, on motion of Dr. Atlee, was referred to the Committee on Medical Literature.

On motion, the regular order was suspended for the reception of the Report of the Committee of Nominations, which was read and laid on the table.

Dr. Hays then called up the Resolution on page 43, vol. 2, of the transactions of the Association, and moved to strike out "all that relates to Committees," &c.

The motion was seconded by Dr. Stevens, and urged by Dr. Drake, who read some ten or twelve special points, which he said ought to occupy the Association instead of being occupied with Epitomes of Rankin & Braithwaite.

Dr. Hooker, of Conn., spoke of the looseness of committees and editors of the Journals.

Dr. Davis thought that they could decide on the matter at once.

Dr. Hays proposed to dispense with the Standing Committees. The question was then taken on the resolution, which was adopted.

Dr. Wood, of Penn., offered the following resolution, which was adopted :

Resolved, That a committee be appointed to take into consideration the arrangement of a committee for future action, to report as speedy as possible.

The Chairman of the Committee on Medical Education was about to read the regular report on that subject, when Dr. Drake moved the suspension of the reading till after the recess, as it was a very long report.

On motion of Dr. Johnston, of Missouri, the report of the Committee on Medical Literature was then taken up.

Dr. Desaussure announced that Dr. Davis would read a paper entitled an experimental enquiry concerning some points connected with the process of Assimilation and Nutrition.

Dr. Boyburn of Missouri, presented and read the report of the Committee on Medical Literature. In the course of his reading the Report, he gave way to a motion to adjourn.

AFTERNOON SESSION.

The President took the Chair at half-past 5 o'clock and organized the meeting.

The Secretary announced the following gentlemen as having been appointed by the President, under resolution of this morning, concerning a committee for the arrangement of business, for the occupation of the Association in future: Drs. G. Wood, of Pennsylvania; J. Hays, D. Drake, A. H. Stevens, W. Hooker, B. R. Wellford, and S. H. Dickson.

The following gentlemen were appointed a committee under a resolution in regard to Schools of Pharmacy and Dental Surgery, viz: Drs. Hays, Stevens, Yardley, Storer and Jones.

Dr. Dickson moved the following Preamble and Resolutions, which were seconded by Dr. Bond, and unanimously adopted without debate:

Whereas, efforts are being made to repeal the law of 1847, which confers protective rank on the members of the Medical Department of the Army, therefore

Resolved, That the American Medical Association, views, with regret, the existence of hostility to the act of Congress, approved February 11, 1847, which confers legal rights, and equality with other Staff Departments on the Medical Officers of the Army, and gives them a position to which the importance and character of the profession entitles them.

Resolved, That copies of these resolutions, with the resolutions of the Association, passed at its last annual meeting, on the same subject, be transmitted to the Secretaries of War and of the Navy, through the Chiefs of the Medical Department of each service, and to the presiding officers of the Senate and House of Representatives of the U. States.

The reading of the Report of the Committee of Medical Literature was then concluded.

On motion, the Report was adopted, and referred to the Committee on Publication.

The Report of the Committee on Medical Education was then called for, and as the hour was late, the Chairman read only so much of it as relates to Demonstrative Midwifery, which had by special resolution been referred to the committee.

On motion, the Report was accepted, and referred to the Committee of Publication.

Dr. Dickson then offered the following resolution which was adopted:

Resolved, That this Association unanimously approve of the opinions expressed in the Report of the Committee on Medical Education in respect to Demonstrative Midwifery.

The Convention then adjourned to meet to-morrow morning at 10 o'clock.

MORNING SESSION.—MAY 9TH.

The President, Dr. James Moultrie, in the Chair.

The Minutes of the last meeting were read and confirmed.

The report of the Committee on Medical Education being the special order, Dr. Stevens, of New York, asked and obtained leave to introduce the following resolutions:

Resolved, That the members of this Association cannot separate without expressing their grateful sense of the hospitalities, and numerous delicate attentions received from their Medical brethren of South Carolina, and the citizens of Charleston.

Resolved, That a Committee be formed, to procure a tablet with a suitable inscription, commemorative of this meeting, and the feeling it has elicited, to be placed at the disposal of the Medical Association of South Carolina.

This tablet is here placed by the American Medical Association, to commemorate their annual meeting in the city of Charleston, in May, 1851, and to signalize their gratitude for the extraordinary professional and social enjoyments that accompanied it.

The resolutions having been seconded were adopted; and Dr. Stevens, further moved that Drs. Hayward, of Mass, and F. A. Ramsey, of Tenn. and himself constitute the Committee.

Dr. Ramsey, of Tenn. asked and obtained leave to read a letter from Dr. E. D. Fenner, of Louisiana, and offered the following resolution on the subject, which was adopted:

Resolved, That the efforts of Dr. Fenner to place on a firm and durable basis, an annual publication, embracing Medical Reports from the whole Southern portion of the Union, merits the commendation of this Association and should receive solid support from American physicians.

Dr. Hays, of Pa. asked and obtained leave to call up for consideration, so much of the report of the Nominating Committee, as relates to the selection of the next place of meeting of the Association, and the appointment of the Committee of Arrangements and the Committee of Publication, the other Standing Committees having been abolished. The report having been read, Dr. Drake, of Ohio, made an urgent appeal in favor of Washington City as the next place of meeting. The question being taken on the adoption of that part of the Report of the Committee, which proposed Richmond, (Va.) as the next place of meeting, it was adopted by a large majority. The question being taken on the confirmation of the Committee of Arrangements and Publication, the nominations of the Committee were confirmed.

Richmond, Va., was selected as the next place of meeting by the Association, and the following gentlemen appointed a Committee of Arrangements, viz:—Drs. R. W. Haxall, Chairman; Carter P. Johnson, James Beale, Chas. B. Gibson, S. Manpas, R. D. Haskins, C. S. Mills and M. P. Scott. Committee of Publication—Drs. Hays, of Pa., G. Emerson, of Pa., D. F. Condy, of Penn., H. W. Desaussure, of So. Ca., J. Parrish, of Penn., P. C. Gooch, of Va. and G. W. Norris, of Penn.

Dr. Hooker, of Conn., Chairman of the Committee on Medical Education, completed the reading of the report of the Committee, and offered the following resolutions:

Resolved, That the abuses which exist in the modes of Medical Education pursued in this country, demand the serious consideration of the profession.

Resolved, That free discussion in relation to these causes is an important means of effecting their removal.

Resolved, That in the opinion of this Association no effort to remove these abuses can succeed, that is not based upon a reform in the public sentiment, both of the profession and of the community.

Resolved, That this reform, so far as the profession is concerned, is to be effected mainly through its organization, and that it is therefore incumbent upon every physician to do all that he can to give them character and efficiency.

Resolved, That this Association have confidence in all proper efforts which have for their object a reform in the sentiments and practice of the community in relation to Medicine and the Medical profession.

Resolved, That the recommendations of this Association at its former meetings in regard to Education, both preliminary and medical, be re-affirmed, and that both the schools and private preceptors be still urged so to do their duty as to secure to the community a well educated profession.

Resolved, That in the work of Medical reform, while all precipitate movements should be avoided, we should aim at a steady advance, from year to year, till a thorough system of education be established by the profession throughout our country.

Dr. Wood, of Pennsylvania, asked leave to suspend the order usually taken with reports. Permission being granted, he read the following report, which was adopted:

The committee to whom was referred the subject of arranging a plan of committees, for future action, in place of the standing committees, abolished by the Association, have the honor to report as follows:

It appears to them that the most feasible plan of accomplishing the objects of the Association, is to select certain subjects, which may be considered as suitable for investigation, and to refer these subjects to special committees, to be appointed before the close of the present session, and to report to the next. Such a selection the committee have accordingly made, and will offer to the consideration of the Association.

As an additional means of securing valuable contributions, they propose, also, the appointment of a committee, whose business it shall

be, in the interval between this and the next session, to receive original volunteer papers, upon any subject which their authors may choose; to decide upon the merits of these papers; and to present to the Association, at its next session, such of them as they may deem worthy of receiving this direction. With a view to increase competition, they think it advisable that a prize of fifty dollars, or a gold medal of that value, be awarded to each of the five papers presented to the Association, or any smaller number of them, which the committee may consider most meritorious, and the Association may resolve to publish.

In reference to the resolution presented in the report of the Standing Committee on Medical Literature, and referred to the present committee, they have only to observe that, as its ends will probably be most effectively obtained, by the adoption of the general plan which they have already brought before the notice of the Association, they do not consider it expedient to make any further report.

As to the appointment of the special committees referred to, your committee think that the most convenient plan will be to refer to a special committee, the nomination of a Chairman for each, who shall then select, at his convenience, two individuals, to aid him, with the restriction only, that the persons so selected, shall be members of the Association.

To the same nominating committee, may be referred the appointment of the general committee, whose business will be to receive and judge whatever papers, as the members of this general committee must frequently compare opinions, it will be desirable that they should reside near each other; and it is accordingly proposed, that they should be chosen from one neighborhood. If the plan be found to work well, this locality may be changed every year, so that each section of the Union may, in its turn, be charged with this duty. The committee would suggest that the general committee should be first chosen from members of the Association, residing in Boston or its neighborhood, as the most northern point.

To embody these suggestions in due form, the committee offer the following resolutions:

I. *Resolved*, That committees of three be appointed to investigate and report severally on the following subjects:

- 1st. Causes of the Tubercular Diathesis.
- 2d. Blending and conversion of the types of fever.
- 3d. The mutual relations of Yellow Fever and Bilious Remittent Fever.
- 4th. Epidemic Erysipelas.
- 5th. Acute and Chronic Diseases of the Neck and of the Uterus.
- 6th. Dengue.
- 7th. The Milk Sickness, so called.
- 8th. Endemic prevalence of Tetanus.
- 9th. Diseases of Parasitic origin.
- 10th. Physiological peculiarities and diseases of negroes.
- 11th. The action of water on lead pipes and the diseases which proceed from it.

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- 12th. The alkaloids which may be substituted for quinia.
 - 13th. Permanent cure of reducible hernia.
 - 14th. Results of Surgical operations for the relief of malignant diseases.
 - 15th. Statistics of operations for removal of stone in the bladder.
 - 16th. Cold water dressings.
 - 17th. The sanitary principles applicable to the construction of dwellings.
 - 18th. The toxicological and medicinal properties of our cryptogamic plants.
 - 19th. Agency of the refrigeration produced through upward radiation of heat as an exciting cause of disease.
 - 20th. Epidemic diseases of New-England and New York.
 - 21st. Epidemic diseases of Pennsylvania, New Jersey, Delaware and Maryland.
 - 22d. Epidemic diseases of Virginia and North-Carolina.
 - 23d. Epidemic diseases of South-Carolina, Georgia, Florida and Alabama.
 - 24th. Epidemic diseases of Mississippi, Louisiana, Texas and Arkansas.
 - 25th. Epidemic diseases of Tennessee and Kentucky.
 - 26th. Epidemic diseases of Missouri, Illinois, Iowa and Wisconsin.
 - 27th. Epidemic diseases of Indiana, Ohio and Michigan.
- II. *Resolved*, That a Committee of Nomination be appointed, whose duty it shall be to nominate one Chairman for each of the above committees.
- III. *Resolved*, That each of the Chairmen thus nominated shall select, at his earliest convenience, the members of the Association to complete the committee.
- IV. *Resolved*, That a committee of five members be appointed, to be called the Committee for Volunteer Communications, whose duty it shall be, in the interval between the present and the next succeeding sessions, to receive papers upon any subject from any persons who may choose to send them, to decide upon the merits of these papers, and to select for presentation to the Association, at its next session, such as they may deem worthy of being thus presented.
- V. *Resolved*, That the Committee for Volunteer Communications, shall have the power to form such regulations as the mode in which the papers are to be presented, and as to the observing of secrecy, or otherwise, as they may think proper.
- VI. *Resolved*, That the selection of the members of this committee be referred to the same Nominating Committee, whose duty it will be to appoint the Chairman of the several Special Committees, as above directed; with this restriction, that the individuals composing it shall reside in the same neighborhood.
- VII. *Resolved*, That a prize of fifty dollars be awarded to each of the volunteer communications reported on favorably by the committee, and directed by the Association to be published: Provided, that the number to which the prize is thus awarded do not exceed five; and

provided, also, if the number approved and directed to be published exceed five, that in such case, the prize be awarded to the five which the committee may determine to be most meritorious. All of which is respectfully submitted.

Charleston, May 9th, 1851.

GEO. B. WOOD, Chairman.

Dr. Hays, of Penn., gave notice, that at the next meeting of the Association, he should offer an amendment to the Constitution, line 4, so as to read \$10 instead of \$3.

Dr. Atlee, of Pennsylvania, remarked on the value of the Report of the Committee on Medical Education, and offered the following resolution, which was adopted :

Resolved, That it be recommended to the several State Medical Societies throughout the Union, to procure a re-publication of the Report of the Committee on Medical Education, for general distribution among the profession.

Dr. Drake offered the following resolution :

Resolved, That in the opinion of the Association, the students of our schools should be required to matriculate within the first days after the opening of the Sessions, and continue their attendance to the end of the terms, taking with them evidence of the same, to be presented with tickets of the Professors when they become candidates for the Degrees.

The resolution was adopted, and Dr. Gibson moved to defer the filling up of the blank. Some discussion arose on this point, when the resolution was left to read, "within the first days," &c.

The Report of the Committee on Medical Science was then called up, when a letter was read from Dr. Dowler, Chairman of said committee, regretting his inability to be present, and the necessity of sending it.

Dr. Fenner then read the outlines of the report, and asked permission to retain the same for revision, copying, &c., which was granted.

Dr. Mauren offered the following resolution, which was adopted :

Resolved, That the Committee on Publication, be instructed to print conspicuously upon the title page of the forthcoming volume of the transactions, the following declaration, viz : "The American Medical Association, although formally accepting and publishing the Reports of the various Standing Committees, holds itself wholly irresponsible for the opinions, theories or criticisms, therein contained."

Dr. Storer moved the following resolution, which was adopted :

Resolved, That the hearty thanks of this Association, be presented to their late Secretary, Alfred Stillé, M. D., for his constant unwearied and invaluable services since its first organization.

The report of the committee on Adulterated Drugs was read. A motion was made to refer the same to the Committee on Publication, which was lost, and a motion to lay it on the table adopted.

Dr. Gaillard, of South Carolina, Chairman of the Committee on Hygiene, presented an outline of the report on that subject. Referred to the Committee of Publication, with authority to append thereto, a paper now in preparation, on the Mortuary Statistics of certain cities.

Dr. Drake, of Ohio, offered the following amendments to the Constitution, which were read and ordered to lie over under the rule.

All members by invitation, must be nominated in writing by five members of the Association, whose names shall be recorded in the minutes. When elected, they shall enjoy all the rights and privileges of Delegates, and remain permanent members of the Association.

All permanent members shall be entitled to vote, and when they attend a meeting of the Association, their respective names shall be registered, and each shall pay the sum required from a delegate.

The Secretary read a Protest from the Iowa University, against the representation of Rush Medical College in this Association.

Dr. Jervey moved to refer the Protest to a special committee, to report at once.

Dr. Wood moved to refer it to the Committee on Colleges of Pharmacy and Dentistry, which was carried. Dr. Jervey withdrawing his motion.

Dr. Wood read the following report of the Committee of Nominations, which was adopted.

The committee to whom was referred the nomination of the Chairmen of the several Special Committees, to report at the next session, and also, of the committee for volunteer communications, report that they have fulfilled the object of their appointment, and offer the following list of Chairmen, to the committees first referred to.*

The President read an invitation from the Committee of Reception, to a steam boat excursion on our waters.

Dr. M'Intyre, of New York, proposed that the Code of Ethics and Constitution of the Association, be recommended to be published by the several State Societies. Proposition adopted.

Dr. Grimshaw offered the following resolution :

Resolved, That Medical Colleges, in publishing statements of the number of Medical and Surgical cases treated at their Dispensaries, act contrary to the spirit of the Code of Ethics adopted by this body.

Adjourned.

AFTERNOON SESSION.

The Association re-assembled at 5 o'clock, Dr. B. R. Wellford, of Virginia, Vice-President, in the Chair.

The special order was called for, and Dr. Davis, of Ill., read a paper on the influence of certain diet on the function of Respiration and Calorification, &c.

The President, Dr. James Moultrie, resumed the Chair.

Dr. Hays moved to proceed with the consideration of unfinished business.

Dr. Grimshaw offered the following resolution, which was adopted :

Resolved, That the thanks of the Association be returned to Dr. Davis, for the paper just presented by him.

Dr. F. A. Ramsey, of Tenn, called up, as unfinished business, the resolution offered yesterday by Dr. Jones, of Tenn., and not then acted

* The names of the gentlemen composing the Committees will appear in our next number.—EDITOR.

upon, to which Dr. Grimshaw offered the following Amendment—"And that the first Convention be held before the first of May, 1852." The question being on taken the Resolution, and the Amendment, they were both negatived by a large majority.

Dr. Phelps, of New York, offered the following resolutions, which were unanimously adopted :

Resolved, That the warmest thanks of the Association be tendered to the Trustees of the St. Andrew's Society, for the gratuitous use of their very convenient and eligible Hall ; and to all those other Institutions and Reading Rooms, which have been so freely thrown open for the inspection and use of the members.

Resolved, That the Committee of Arrangements receive our most grateful acknowledgments for the very handsome, and indeed magnificent manner in which they have provided for the entertainment and pleasure of the Delegates from abroad, during their sojourn in the city of Charleston.

Resolved, That not only the Profession of Medicine, but also private munificence, and the kind attentions of the citizens generally, have conspired in manifestation of that urbanity of manner, and that unwearied and kind attention, which commands not only our profound admiration, but will be followed by the most pleasing recollections so long as life and thought shall endure.

On motion of Dr. Stevens, the above resolutions, with those offered by him at the Morning Session, were ordered to be published in the city papers.

Dr. Johnston, of St. Louis, moved to adjourn *sine die*, which was adopted.

The Vice-President, Dr. Wellford, of Virginia, then congratulated the Association on the happy termination of its labors, and declared it adjourned, to meet again in Richmond, Va., on the *first* Tuesday in May next.

Medical Professors in Spain.—The Faculties of Medicine in Spain have been organized. The number of Professors in Madrid are eighteen, three of whom are Specialists. The Specialities are the syphilitic and cutaneous diseases and affections of the eye. The Colleges at Barcelona and Seville have each fourteen Professors in the schools of Pharmacy. In each of these cities there are five Professors.—*Bul. Gen. de Therap., Dec. 1850.*

☞ The Editor will be absent a few months on a visit to Europe, but is happy to say that he has secured the kind assistance of highly competent friends for the supervision of the Journal until his return.

SOUTHERN MEDICAL AND SURGICAL JOURNAL.

Vol. 7.]

NEW SERIES.—JULY, 1851.

[No. 7.]

PART FIRST.

Original Communications.

ARTICLE XXI.

Surgical Cases. By HENRY F. CAMPBELL, M. D., Demonstrator of Anatomy in the Medical College of Georgia.

What we require for the perfection of *medical science*, is an accumulation of facts “capable of being used as data, and dealt with in our researches and generalizations as we deal with the data of *physical science*.” We have therefore regarded it by no means an unimportant duty, to record every thing which can in any way add to that vast amount of comparable facts, upon the accumulation of which the laws of our science are to be founded, and from which deductions are made for remedial treatment. It is more in accordance with this view of fact-gathering, than on account of any particular novelty investing them, that the following cases are reported :

CASE I. *Extensive Injury of the Elbow-joint, with Compound Fracture of the Olecranon process.*—Charles, a negro man, about 60 years of age, was severely beaten with a cudgel, by another negro. Besides other wounds on the head and about the body, we found on examination that the principal injuries had been sustained by the left superior extremity. The radius was fractured at about its middle third, and the elbow-joint opened by a blow on the olecranon process of the ulna. Ex-

ternally there was but little apparent injury. The fore-arm was permanently flexed at nearly a right angle with the arm, and there was inability to extend it, except by its own weight. On the posterior surface of the elbow-joint there was a wound in the integument about three-fourths of an inch in length which communicated with a fracture of the ulna about the middle of the greater sigmoid cavity, thus separating the olecranon process from the body of the bone. In examining the opening and fracture, the finger passed between the olecranon and the upper extremity of the shaft of the bone, and was pushed forward past the belly of the brachialis anticus muscle, when the pulsations of the brachial artery were distinctly felt. The trochleated extremity of the humerus was also in contact with the finger. The olecranon process had been drawn up about an inch and a half from its natural position by the action of the triceps extensor muscle. The only muscle connecting the humerus and ulna on the posterior aspect of the limb was the anconeus. There was considerable effusion of blood about the joint, though but little external hemorrhage besides the large clots removed from the wound by the finger. The orbicular ligament having been ruptured in consequence of the fracture of the ulna at the point of its attachment, there was a dislocation of the upper portion of the radius upward and forward upon the anterior surface of the external condyle of the humerus. In consequence of this luxation, complete extension could not be effected.

In consultation, it was determined to attempt the treatment of these injuries without amputation. The luxation of the radius was reduced without difficulty, and its two fractured ends brought in contact when the movements of the limb became more extended. Any attempt at union between the olecranon and the body of the bone was forborne, as we wished the limb kept in a flexed position in order that in case of ankylosis, which seemed inevitable, the patient might have the full use of the hand in eating, and other manipulations about his own body. Besides, his age still further precluded the hope of such union. After the removal of the coagula from about the joint, Roe's apparatus for fractures of the superior extremity was applied, and the arm kept in the flexed position. The

bandages were so arranged that the external wound could be dressed without their removal. Fever supervened—the limb became very much swollen and painful. Saline cathartics for depletion, and a solution of acetate of lead, as a local application, appeared to control the inflammation. Suppuration commenced in a few days; the wound was injected frequently with cold water to wash out the discharge, and afterwards the nitrate of silver, alternated by chloride of soda, was used as an injection once a day. Flexion of the limb was frequently made by varying the length of the extending rod of the apparatus, and thus ankylosis avoided. The discharge continued for about a month, at first quite profuse, becoming less and less, it finally ceased; the external wound healed completely, leaving the patient with full use of the joint in every respect, except that of extension otherwise than by the anconeus, which still remained perfect, always assisted by the weight of the forearm. Four years have elapsed since the injury: our patient is still a vigorous old man, and the limb having become better adapted by time to the altered condition of the joint, is used by him with great power and efficiency.

CASE II. *Injury of the Wrist Joint*.—Murphy, a laborer at the Georgia Rail Road Depot, while coupling two cars, received the following injury, by the coming together of the two connecting beams:—On the back of the wrist there was a deep wound down to the tendons of the extensor muscle of the fingers. Anteriorly, the laceration was much more extensive,—a corner of the beam appears to have divided the soft parts a little below the wrist, entirely across the hand. A flap was formed of the integument and fat of the palmer surface, which extended to the centre of the hand; several of the flexor tendons were cut, and the joint fully exposed; the ulnar artery was lacerated, but had not bled much, probably on account of the violence with which it had been opened. The patient was in excellent general health at the time of the accident.

The flaps were brought together and retained with sutures and adhesive strips, after which a bandage was applied. The suppuration was very profuse at the second dressing; the flaps then appeared healthy, but subsequently became very dark,

and finally sloughed away entirely. The granulations, however, were vigorous and healthy. About the twentieth day a darkened bone was observed in the orifice at the wrist—on examination, it was found loose: we removed it without difficulty, and it proved to be the trapezoides. After this, the supuration gradually diminished; when we last saw him it was very slight—there is still great tenderness of the palm. The hand is very much distorted, and will doubtless be much restricted in its movements, but in many respects it will prove a valuable and useful member.

The precept inculcated in the above cases is sufficiently plain not to require enunciation; and did we exercise more patience under similar circumstances, delaying amputation as the dernier resort, it is true, many a brilliant operation would be lost to the surgeon, but at the same time many a useful member would be preserved to our patients.

CASE III. *Occlusion of the Vagina.*—Charlotte, a negro woman, aged about 30 years, during a protracted labor sustained such injury to the vagina that extensive sloughing of the mucous membrane resulted, and occlusion of this passage supervened. She had been operated on previously, but in consequence of neglect on the part of the nurse to carry out the instructions of the surgeon, the difficulty had not been removed. When we saw her the occlusion was almost complete, there being only space sufficient for the passage of a very small probe with the exercise of considerable force, which caused great pain. Menstruation had been obstructed for many months, and during these periods she always suffered great pain in the loins and region of the womb and vagina. Her general health was in apparently good condition.

Finding that our examination verified the history of the case, viz: that the occlusion resulted from the loss of a considerable portion of the mucous membrane of the vagina by sloughing, and that the narrowing extended for more than two inches from the orifice of the urethra, just below which it commenced, we concluded not to attempt the restoration of the entire calibre, but only to dilate it sufficiently to admit of the egress of the menstrual fluid. To effect this object, we adopted the fol-

lowing treatment:—It being impracticable to introduce even the smallest sized catheter or bougie, we modified the stricture knife, by attaching to it a beak much smaller than the ordinary one, indeed it consisted of a small silver probe soldered on to the end of the instrument in the place of the ordinary beak. The patient was put under the influence of chloroform, and the beak of this instrument pushed steadily into the small opening left by the stricture. The knife was then protruded from the sheath with its cutting edge downwards, and thus was the whole course of the stricture traversed by the instrument. On withdrawing the knife, some blood and a quantity of dark grumous fluid were discharged, though the flow was by no means so large as we had expected, from the length of time during which menstruation had been obstructed. The after treatment consisted in frequent introduction of bougies of large size, occasionally substituted by the sponge tent. She remained at the Infirmary more than two months, during which time she had three menstruations, two of which were subsequent to the operation. These were quite profuse, and without any pain whatever. At the time of her dismissal, the opening in the vagina was sufficiently large to admit a female catheter of ordinary size. She was furnished with one, and we directed its use once a week, to prevent a return of the occlusion.

Occlusions of the vagina are presented in several varieties. They occur in the form of bridles, occupying only a *portion* of the calibre, or the entire calibre, constituting a ring, or there may be more than one; or the constriction may be still more extensive, and constitute a complete tube of dense indurated lymph deposited in the sub-mucous cellular tissue, entirely obstructing the passage for one or more inches. In these cases the knife has proved beneficial; for when the dense lymph forming the constriction has been cut through, the mucous membrane is allowed to unfold and occupy nearly its original expanse; but in the present case the occlusion was caused by the actual *destruction* of a large portion of the mucous membrane, and as this tissue is but little susceptible of stretching, and as we know of no process by which it can be reproduced, of course any attempt at restoring the full calibre of the vagina so as to admit of the exercise of the important

functions of coition and parturition, would have been altogether nugatory.*

CASE IV. *Adhesion of the Ear after almost entire separation from the Head.*—Goldberg, a Hungarian pedlar, was brought to our Infirmary after having received very severe injuries at the Georgia Rail Road, by being caught by a car and jammed against a brick wall. The injury, however, for which we report the case, was an extensive laceration of the left ear. It appears that it was caught by the projecting edge of the roof of the car, and we found it torn entirely off, with the exception of about one-fourth of an inch of skin at the upper portion of its attachment, and this narrow neck of skin had been raised for some distance up from the temporal bone. The cartilage was torn off from its attachment to the auditory process, the rough edge of which was observable in the centre of the denuded surface from which the ear had been torn. The ear was cold and blue—apparently without vitality.

Notwithstanding this seemingly hopeless condition, we concluded to attempt union. For this purpose we applied six sutures of silk, attaching the ear to the scalp, a thick pledget of lint was placed behind the ear, and another broader one over it to retain it steadily, and over these a bandage was applied. We administered a grain of morphine to the patient, for pain which he suffered from other and more serious injuries. On the fourth day, the dressings were removed, and to our surprise, the entire ear had adhered by means of coagulable lymph. A very small portion of the lobe, which had been badly bruised, had become dark, but the rest of the organ was red and warm, and in every way manifested sufficient vitality to convince us that our otoplastic operation had succeeded.

The patient remained with us for nearly two months, during which time the adhesion became perfectly firm. Although the ear had become attached, still the meatus was not preserved, in consequence of the cartilage not becoming attached to the auditory process. This opening being completely closed, deaf-

* In the sixteenth volume of the Dublin Medical Journal this subject is treated by Dr. Kennedy as the disease is found to exist in the unimpregnated female; and Dr. Doherty, in a more recent number of the same journal, treats of the affection as it occurs in the impregnated female.

ness was the result,—this was remedied by an incision over the meatus, and the introduction of short metallic bougies, which were worn to dilate the orifice. When he left us he was still wearing the bougie, and could hear distinctly whenever it was removed temporarily, though we did not consider it prudent at that time, to leave it out permanently, for fear of a return of the occlusion.

The above case needs no extended remark; the precept deducible from it is too plain to require any amplification—viz., that we should attempt union in cases like the above, even though the chance of success may be but a forlorn hope.

CASE V. *Lithotomy.*—We were kindly called to the following case, by our friend, Dr. J. L. Hamilton, of Stone Mountain. The patient, a boy nine years of age, was a native of De Kalb county, and has resided near the mountain from his birth: he had manifested symptoms of Stone from a very early age. For several weeks previous to the operation, his attacks had been unusually distressing. They were accompanied by great irritation and bearing down of the rectum, so that he was frequently affected with prolapsus ani. When we saw him, these paroxysms had become very frequent, and interfered materially with his sleep at night. We administered morphine to quiet the irritability of the bladder and rectum, and on sounding, the stone was easily detected. With the assistance of my friends, Drs. Quintard and Hamilton, we performed the bilateral operation, and an oblong calculus, presenting the physical characters of the oxalate of lime variety, with irregular incrustations of phosphatic deposit, was removed. On measurement, it was ascertained to be of the following dimensions: longest diameter, one inch and three-tenths; shortest diameter, seven-tenths of an inch. The stone is somewhat flattened, and therefore presents another transverse diameter, which is eight-tenths of an inch. Its weight, immediately after the operation, was two drachms and one scruple.

The rectum protruded during the operation; it was replaced, and the extraction completed without accident, and with but little hemorrhage. The patient was left in charge of Dr. Hamilton. A letter from that gentleman, informs us that he has entirely recovered, without an unpleasant symptom.

We would call attention to the diameters of the stone in the above case, singularly corresponding with those of a calculus recently crushed by Dr. Dugas, and reported in the April number of the present volume of this Journal.

ARTICLE XXII.

A Case of Hysteria, with Clonic Spasms—cured by Chloroform. By E. M. PENDLETON, M. D., of Sparta, Ga.

On Friday, the 16th inst., was called, about 3 P. M., to see Dinah, a negro woman belonging to Dr. Terrell, some four or five miles in the country. Learned that she had complained of a headache during the morning, and had evinced some awkwardness in cooking dinner, and made several foolish remarks, as if absent-minded. After dinner she complained of violent headach, and laid down. Not long after she was found by a fellow servant in a speechless condition, breathing hurriedly and frothing at the mouth. No effort could arouse her, or make her notice any thing around her. She remained pretty much in this condition till I arrived, with perhaps the exception of throwing herself about occasionally. Mr. J., the overseer, had bled her about a half pint, as he told me—was afraid to take more, as her pulse was almost imperceptible: he found it became stronger, however, under the bleeding. I found her pulse nearly as slow and soft as natural, but had not finished my examination before a paroxysm came on of all the voluntary muscles of the body. It seemed impossible to hold her in bed, and required three persons to do it. During her exertions the blood began to flow, which was encouraged until she bled about three pints. She sank back quite exhausted and fainty, her pulse giving way, a perspiration breaking out, and she soon uttered several coherent words, as "let me alone." An effort to give medicine proved abortive. I directed a large dose of ol. ricini, as soon as she could take it and injections of salt and water, *per anum*—pitchers of cold water to be poured on her head if she was turbulent, and another venesection if her pulse reacted.

The next morning heard from her by note. She appeared

to come to her right mind early in the night and rested tolerably; the medicine was taken and acted well; she seemed in a calm and quiet mood. In the afternoon received another note: that since 9 o'clock, A. M., she had been running, jumping and shouting almost incessantly, being wild, turbulent and ungovernable. I could not possibly see her till morning, I wrote directions similar to those formerly given. She was bled by the overseer, cold water poured on the head, a blister applied to the nape of the neck, and the ol. ricini repeated until free evacuations were produced. All, however, to no purpose; she continued in the same condition all night without sleeping any. The disease was paroxysmal—the intermissions, however, very brief. I arrived about 9½ o'clock, A. M., found her last paroxysm had just passed off and she was quiet, her eyes closed, pulse 115 per minute and soft, skin cool and moist, tongue slightly furred, which she protruded at my request.

In a very few moments her arms and legs began to jerk, the voluntary muscles being put in action, and she began to shout as loud as she could (her voice, however, was quite hoarse) prayers, exhortations, &c., formed the burden of her language; she seemed to know every person in the house, calling them by name and talking in the main coherently. There was however a dilatation of the pupils and staring of the eyes which indicated a morbid excitement of the sensorial functions. The paroxysm lasted about twenty minutes, during which time all the voluntary muscles of the extremities seemed to be in constant action, the pulse running up to 150, and quite feeble. I took advantage of the first calm moments to administer the chloroform by inhalation, which was continued unremittingly for about half an hour, during which time no muscular action was exhibited only on occasional movement of the flexor of the leg elevating the knees to nearly a right angle. Sleep was not induced at first. The pulse went down to less than 100. At the time for the next paroxysm it again went up to 120, and she seemed threatened with a return. The chloroform was persisted in, however, and at the end of one hour she was sound asleep, and her pulse down to 92, full and strong. I left her in this condition, with orders to repeat the chloroform whenever the paroxysm seemed likely to ensue.

Monday, 10 A. M. Found her "as calm as a summer evening," perfectly rational, pulse about 90 and soft: had slept well during the night; had no return of the paroxysm, though severely threatened at several times, which was always promptly relieved by the chloroform. There could not be the slightest doubt left on my mind of the cure being the result of this most potent narcotic and sedative, and my thoughts instantly recurred to several painful instances of a similar character which had lasted for days together, that might have been relieved by a similar application, I doubt not. A week has elapsed, and there has been no return.

She gave me the following history of her uterine functions for the last five months, which no doubt originated the whole mischief. Just before Christmas she was delivered of a child at full time, which died in a few days. Late in February she had a healthy return of the catamenia,—in March there was none. At the regular period in April she was taken with fever, violent headach, and bearing down uterine pains, which lasted several days. The headache continued at intervals up to the 16th inst., being then near the time of her next monthly period; it then became more violent, and resulted in the severe chronic spasms as described above.

Believing, as I do, that we have now a remedy for one of the most distressing diseases to which the female is subject, I have deemed it my duty to report this case, that others may be induced to give the chloroform a fair trial in nervous spasmodic affections of a like character with the above.

ARTICLE XXIII.

Remarks on Treatment of Scarlatina. By ROBT. C. WORD,
M. D., of Cassville, Georgia.

As the frequent and open interchange of opinions by the members of the profession is useful, and to a great extent necessary in advancing medical science, I trust it will not be deemed presumptuous or improper to notice an article in the May No. of the Southern Medical and Surgical Journal, on *Scarlatina*, by Dr. Ramsay, of Calhoun, Ga.

Although claiming no great experience in the treatment of this affection, I feel that I can scarcely be mistaken in the conviction that he attaches a degree of importance to his plan of treatment which the future will not realise; that it can not be resorted to with the success which the fortunate experience of the writer has led him to believe. "*Puke* the patient through the whole course of the disease with salt and water, or ipecac—never with tartar." Such is the ground work of the treatment of Scarlatina advised by Dr. R. Now with regard to emetics in this disease, it may be affirmed that most writers upon the subject have advocated their use. They have *before* been recommended, also, throughout the whole course of treatment. But the weight of authority, and general experience, indeed, confirms their employment to the early stages; none, however, that I remember use the *salt water* emetic. In this Dr. R. seems to think that he has found a *specific*. If he has I fear that its unpalatable nature will render it an unfortunate one.

The use of salt as an emetic, in the manner and to the extent advised, seems not only injudicious, but impracticable. In cases unattended with gastric inflammation, and in which it were practicable to get the patient to swallow the remedy, we pretend not to say that it would not do well as an emetic; but the quantity and exceedingly disagreeable taste of the dose, especially with children, seems certainly to constitute an insurmountable obstacle to its general use. The experience of the past, with regard to this class of diseases, make it highly improbable that a remedy will ever be discovered calculated to cut short the malady. Scarlatina, being one of the contagious exanthemata, is self limited in its nature, and not likely, by any course of treatment, to be prevented from running through its regular stages. Hitherto the practitioner in this disease has been able to do little else than to accompany it through its various phases: his duty being to palliate the severity of the symptoms, so far as may be, by a mild and unirritating plan of treatment, combating local inflammation, as in other affections, and when, in the severer forms of the disease, the excitement becomes excessive, to moderate the momentum of the circulation, by the *free use of the lancet*.

I am aware that a strong prejudice exists against the lancet

in this affection—but it is without any good reason. The lancet is, has been, and I believe will continue to be, the sheet-anchor of the profession in *all* inflammatory affections. And when in scarlatina, and other anginose diseases, the throat becomes very sore, swollen and injected, the breathing laborious and deglutition almost wholly obstructed, accompanied with high general excitement, the use of the lancet is indispensable. Scarifying the throat might give temporary and partial relief; but so long as the general circulation continues full and active, the severity of the symptoms will remain, without material abatement. In many instances, the throat is so swollen and inflamed, that nourishment cannot be taken, and even water is ejected, with a strangling sensation, through the posterior nares when an attempt is made. In such a condition it is difficult to conceive the practicability of the salt water plan of treatment. There is no room to swallow, and as little room for delay in the use of the proper remedial agent, to-wit, the lancet. To refrain from bleeding the patient under such circumstances, from the impression that “it deprives him of an essential element of cure—strength,” can find no proper sanction in reason, authority, or experience. Death need seldom be apprehended from mere debility. Let blood be abstracted freely—that the distended and engorged vessels of the inflamed surfaces may relieve themselves by contraction, inversely increasing the room for the passage of air—while the function of respiration becomes free—the fever diminishes—the inflammation abates, and the relief becomes general. The patient will now be able to take occasional draughts of cold water, which will usually be found both beneficial and refreshing in scarlatina as well as in other febrile affections. To the divisions of scarlatina, by Dr. Ramsay, I have no special objections to make—I am not prepared to admit, however, that scarlatina never exists without sore throat. Authors of great learning and experience have assured us that cases do occur unaccompanied with this symptom—(Armstrong and others.) So far as I have been able to learn, the disease, as it prevails in this country, assumes, usually, the anginose form of the older writers—the “*gravis*” of Dr. Ramsay. Dr. Armstrong uses the word “inflammatory,” which is certainly not inappropriate. Occasionally, when the disease

prevails extensively as an epidemic it assumes the malignant or typhoid variety. In this latter form the use of the lancet seems to be greatly dreaded by many latter day practitioners. Yet experience has, I think, furnished ample proof of its great utility when timely resorted to. The early stages of every form of the disease is unquestionably phlogistic. Speaking of the malignant form Dr. Eberle remarks, "However rapidly this form of the disease may pass into a low and malignant state, its outset is often characterized by highly inflammatory symptoms. The attack is vehement, and the febrile excitement at first tumultuous, tending rapidly to consume the vital energies; and in proportion to the violence of this exciting, though transient stage, will be the tendency of the disease to assume a putrid character. It is therefore of the utmost importance to break down promptly, by energetic means, the initial febrile commotion." Again, "As this stage is generally short it is of great importance to draw blood at once in its outset to the extent of producing a very decided impressien on the system." The same sentiments are advocated by Burserius, Larry, Rush and others. As it was not the design of this paper to go into a prolix account of the nature and treatment recommended of Scarlatina, but simply to enter an humble protest to some of the more objectionable features in the plan of treatment recommended by Dr. Ramsay, I will here take leave of the subject.

PART II.

Eclectic Department.

A Clinical Lecture on the use of Glycerine in the Treatment of certain forms of Deafness. By THOMAS H. WAKLEY, Esq., F.R.C.S., Surgeon to the Royal Free Hospital.

Nearly two years have now elapsed since I published in the *Lancet*,* a paper on the Use of Glycerine in the Treatment of Certain Forms of Deafness. Numerous facts justified that publication. I did not rush into print hastily, and without due consideration. Tried by the severe and scrutinizing test of experience, glycerine now takes its place amongst the most useful of our remedial agents, in the treatment of several com-

* October No. 1849, p. 304.

mon varieties of deafness. Speculation on the subject is at an end: indisputable facts constitute the data whence the opinions favorable to glycerine have been formed.

The peculiar chemical properties of this fluid have led to its use in other complaints beside deafness, and in some with signal advantage. But the results of its use in affections of the ear have been even more satisfactory than were anticipated. In the hands of several surgeons, the remedy has been used with undoubted benefit: still, its successful employment often demands much care and patience.

I must enforce the "oft-told tale," that an accurate diagnosis is half the cure. With equal truth it may be stated, that the indiscriminate use of a remedy is calculated to bring many a valuable medicine into disrepute. The surgeon who would successfully resist morbid action, of whatsoever kind, must patiently investigate the character of the malady, before he decides upon the plan of treatment; otherwise much perplexity will ensue, and the intended antidote may become the actual poison.

A new remedy is sure to be exposed to the misfortune of being recommended in cases that are not suitable for its adoption: from this cause, an important agent often falls into disrepute, and even disuse. In the treatment of deafness, failures of new remedies are the more likely to happen, as aural maladies find no favor with the majority of the profession. Many empirics owe all their success and ill-acquired wealth to this cause.

The introduction of Glycerine into the treatment of ear-diseases has produced some slight change in practice; and many cases, which not long since fell to the lot of the "aurists," are now in the hands of regular practitioners, greatly to the advantage and safety of the sufferers. The examination of the ears by competent practitioners has led to the discovery of diseases unsuited for the use of glycerine, but having fallen under the notice of competent surgeons, unexpected relief has been afforded to many desponding patients. Why, it may be asked, should the surgeon abandon any class of diseases, and thus invite the charlatan to enter a field of practice which legitimately belongs to the profession? The impropriety of so doing cannot be doubted. It is improper, because it is injurious to the professional character; and it is unwise, because it encourages ignorance, at the expense of a too credulous public.

Soon after my first publication, many unsuccessful cases of the employment of glycerine in deafness were reported to me. This was to be expected. Failure were sure to take place, from a variety of causes; the two most frequent being, the

inaptitude of the cases chosen for the employment of glycerine, and the impurity of the drug used. Several samples of glycerine were sent to me for examination. In only one instance was the specific gravity correct, and in several the fluid contained an admixture of lead and oil; such glycerine as this must always prove injurious. Sometimes it may, when thus impure, prove highly irritating, and instances of this kind have been mentioned to me by both London and country practitioners. If there exist oily particles in the glycerine, they become rancid, and the whole fluid is speedily vitiated: in this state it cannot be used with safety. I feel confident that the impurity of the article has been a frequent cause of failure. In other instances, the glycerine has not been used with sufficient diligence, nor for the requisite length of time. Structures that are almost disorganized cannot be restored to a normal state in a day.

The glycerine has now been employed in some hundreds of instances of deafness, and data have been collected that indicate the cases in which the remedy should be used, the duration of the treatment, and the probable or possible permanence of the cure. In prescribing the use of glycerine, care should be taken to discriminate between those diseases which are suitable for its employment, and others where the remedy would be introduced without the slightest prospect of advantage. In making this investigation and inquiry, the *history* of the malady cannot be too attentively considered. Did the defective hearing first occur after an eruptive fever?—an abscess in the face, or fauces?—a fall?—a blow?—a fit? Was there a discharge from the ear in the first instance?—if so, what was the character of the discharge? Did any sequestra escape? The *form* of the ear should be carefully examined, and the auditory canal and membrane tympani inspected by means of instruments especially constructed for the purpose. A silver speculum should be used, through which are reflected the rays of the sun or of a very strong artificial light. By these means we are enabled to examine carefully the auditory cul-de-sac, and especially the membrane tympani. The quantity and condition of the cerumen should be ascertained. If a stethoscope be placed over the external ear, and the patient be directed to close his mouth and nostrils, and then forcibly expel the air from his lungs, it will readily be discovered whether the Eustachian tube be open or not.

If the drum be entire, the air will be heard to strike forcibly against it. On the other hand, if the drum be perforated, the escape of air through the auditory passage will truly indicate the condition of the parts. All these points are entitled to atten-

tion; some of them, however, as you will soon discover in practice, are of much more importance than others. Catheterism of the Eustachian tube should not be practised on slight grounds. When a necessity for the operation exists, of course it should be performed, but not otherwise. In unpractised hands the operation may be productive, not only of annoyance, but of some mischief.

If the surface of the auditory canal be hard and inelastic, shining, and of a whitish appearance: if the natural secretion be wanting, and the *membrana tympani* be not painful to the touch, the glycerine may be employed with a tolerable certainty of success, even if a partial deafness has been of many years' duration. An uneven appearance of the external membrane of the drum is an unfavorable sign, as in some instances it may be caused by displacement of the bones of the delicate aural structure. When besides the sense of hearing, the other senses are deficient of action, the employment of glycerine alone offers no hope of success. In such cases the utmost possible attention should be paid to the general health of the patient, with a view to restore the activity of the nervous system. The existence of paralysis in any part, unless from a traumatic cause, is an adverse indication with respect to the use of glycerine. The modes of applying the remedy vary according to the state of the parts, and the effects sought to be produced. When the surface of the aural canal is dry and shining, the ears are to be carefully cleansed by means of cotton held within the blades of a pair of forceps, and moistened with warm water. The canal is then to be rubbed with dry cotton, held in a like manner. Next the glycerine is to be applied by the same means, the cotton, well soaked in it, having been repeatedly passed backwards and forwards in the external meatus, care being taken to diffuse it over the surface of the *tympanum*.

I shall now mention some cases, selected from my note-book, as furnishing good types of the diseases which have been relieved by this mode of applying glycerine. They might be multiplied to a very large number.

Mary R——, Gray's-in-lane; Nov. 19, 1850; aged forty-nine; a strong, healthy-looking woman, an out-patient of this hospital. Deaf six years; could not hear the highest power of sonometer; ears dry and horny; *membrana tympani* of the right side ulcerated after a discharge, (following scarlet fever,) lasting six months; in the other ear the membrane was sound. The ears were rubbed with glycerine in the usual way, and in a few days she heard successively Nos. 8 and 7 of the sonometer. In seven weeks she was quite cured, a healthy secretion of wax having been established.

Anne M. —, Gray's-in-lane; aged thirty-three; a spare thin woman; had been deaf since the birth of her last child, when she had discharge from both ears; this ceased, and left her very deaf. Her ears presented much the same appearance as in the last case, the principal feature being the dry condition of the meatus. Glycerine was applied with the forceps and wool, and the woman was soon relieved from the unfortunate impediment.

Louisa R—, Hampstead, August, 1850, aged twenty-seven, deaf nine years. Ten years since, had measles, followed by a discharge, which lasted four months; it then ceased in both ears and she has been deaf ever since that period. Hears better after washing her ears. The only peculiarity to be observed was a total want of wax. The glycerine was applied in the usual way, and gave almost instant relief. In six weeks her hearing was quite restored. This was a very remarkable case.

In other cases, where the ears are plugged with hardened, impacted wax, and where the membrana tympani is only coated with vitiated wax, the glycerine must be dropped into the ear three or four times during the day. In twenty-four hours the hardened mass will generally become sufficiently softened for removal—a little operation which requires some caution. If force be used, a portion of the delicate membrane of the drum may be torn away, and unpleasant consequences ensue. The mass will generally separate without force of any kind, if the means recommended be carefully followed, gentle syringing will also promote the separation. A pellet of fine sheep's wool, moistened with glycerine, should be placed in the meatus, in order that the newly exposed surface may be brought under the direct operation of the remedial agent. The pellet also would be of use in protecting the parts from the effects of cold and the sudden influence of the air. The removal of an impacted mass of exsiccated cerumen without these precautions may produce more deafness than the presence of the offending substance.

Master F—, City-road, aged six years, deaf in the right ear six months. Has had a constant roaring noise in that ear; which was frequently swollen, and very sore; he had been ordered purges and lotions, from which no benefit resulted. On examination, the meatus was found completely blocked up with hardened wax. I filled the ear with glycerine, and then fitted the plug. The next morning I easily removed the obstruction, and in it was found a good sized cherrystone. The hearing was painfully acute for a few days. Sheeps wool was kept in the ear. I mention this case to show how necessary it is thoroughly to investigate every case which presents itself to you. I remember another instance occuring in a member of

parliament, who was annoyed at intervals for two years by a discharge from, and painful swelling of, the right ear. At last the dens sapientiæ of that side became painful, and the gum inflamed; this tooth was removed, and the ear was soon well. This case I consider very instructive; and the close proximity and anatomical relations of the part justify the supposition, that the diseased condition of the tooth caused the deafness and discharge from the ear.

H. R—, aged forty-three; Sept. 1850; an out-patient of this hospital; very deaf in the right ear; could not hear No. 3 of the sonometer; cannot refer it to any cause; it appeared very gradually. He suffers from a “blowing sound” in that ear; when masticating his food, each fall of his jaw sounds like the report of a pistol; at other times he hears loud crackling noises. As I suspected in this case, an impacted mass of solidified wax was found, filling up the inner third of the meatus. Glycerine was poured in until the meatus was full; the bees-wax plug was then applied. In two days the wax was completely softened, and could be easily removed. The hearing was perfectly restored.

Another mode of applying the glycerine consists in soaking a pellet of sheep’s wool in the fluid, and pushing it gently into the meatus until it rests against the drum. The wool, when compressed, should be about the size and shape of the aural cul-de-sac. A plug of prepared bees-wax, warmed in hot water, and placed against the external opening, and retained there, will effectually prevent both the entrance of the atmospheric air and exit of the glycerine. This proceeding must be repeated every morning, the meatus being each time carefully cleansed by means of warm water, and made dry by passing backwards and forwards a small piece of dry cotton; then there will be a clear surface for the action of the glycerine.

One of the judges of the superior courts consulted me, with the concurrence of Sir B. Brodie. The learned judge was suffering from deafness in the right ear. The organ had become quite useless. The left ear was also partially deaf, but with the assistance of a very clever instrument, made by Mr. Rien, Strand, he was enabled to continue his judicial duties. On examination with the speculum auris, assisted by a powerful reflector, I found the lining membrane of the meatus dry and polished, the canal open and very straight, and the membrane of the drum of a pearly whiteness, the central part, projecting into the meatus, and presenting even more opacity than the other parts; no ceruminous secretion whatever. The air could be heard to strike against the tympanum in both ears; this membrane was intact on both sides. I applied the wool, well

saturated with the glycerine, to the membrane, fitting the bees-wax plug, which effectually prevents the entrance of atmospheric air or the exit of the glycerine, thus keeping the agent constantly against the part to be acted on. This proceeding was repeated every morning, the meatus being gently cleansed with warm water before fresh glycerine was introduced. At different intervals, four distinct layers of white, pulpy epithelium were removed; the ear was occasionally swollen and painful. The treatment was persevered in for more than two months, and the membrane of the drum was much altered in appearance, assuming more of the dark look of the healthy organ. I introduce this case, not as a successful one in its results, but as illustrating very beautifully the mechanical action of the agent in the worst case of epithelial thickening which I have ever seen. In this case, had the deafness been due to the mechanical obstruction, it would have proved beneficial, but the cause was purely nervous, as had been previously diagnosed by Sir B. Brodie. There can be little doubt, that in many cases this cuticular deposition is the cause of deficient hearing, but it may be that the paralysis is the exciting cause to this morbid action.

M. O——, Clerkenwell, aged fifty-three, August, 1850. Deafness in both ears; followed a severe attack of influenza; never had any discharge from either ear; occasional pain in both ears; when travelling in a railway carriage, hears better than other persons. On examination, the auræ cul-de-sac was found to be dry and inelastic, and to have the appearance of parchment; the membrana tympani looked white, and of a cartilaginous consistence; no wax or moisture of any kind; the meatus and tympanum painful to the touch. The glycerine was used as in the last case. From the right ear two pieces of epithelium, of a pulpy consistence, were removed, and one thick piece from the left ear. In seven weeks she discontinued her attendance, hearing, when at church, the clergyman of the parish, a blessing which (as she had stated) she never again expected to enjoy. I had twice to modify the treatment in this case, as some pain and swelling supervened.

Henry M——, Esq., a merchant, aged sixty-two, consulted me, Oct. 16, 1850. Had been deaf for more than twelve years in his right ear; he could not hear the loudest tone of the sonometer. The meatus had quite a "parchment appearance." The ear was filled with glycerine, and the bees-wax plug introduced. This was repeated every day. In fourteen days several pieces of soft skin-like substance were removed, with evident improvement to the patient, who could now hear No. 3 of the sonometer. I was obliged to modify the treatment once, as

the patient complained of pain. In this treatment the general health, especially the secretions, should always be watched.

Mary M—, Sept. 15th, 1850; Brentford; aged twenty-six; deaf in both ears; could not hear the highest tone of the sonometer; meatus dry and inelastic; tympanum much thickened; not perforated; not painful to the touch. Treated as in preceding case; treatment lasted six weeks. Two portions of cuticular soft substance came away from the right ear; the left did not appear much affected by the glycerine. In six weeks the patient heard No. 3 of the sonometer, at a distance of two feet from the ear. No improvement whatever in the right ear. I saw this patient a few days since; she states that the relieved ear fully answers all the purposes required.

When patients are treated in this way, they of course, require the careful attention of the surgeon. The *modus operandi* is simple enough; the glycerine being kept continually in contact with the part, acts mechanically, either absorbing or penetrating the epithelia coating and separating the individual particles. The ordinary time required for this treatment varies from two to eight weeks, according to the method employed. With respect to the permanence of the relief—some cases always require the presence of glycerine, as the best known substitute for the natural secretion of the aural membrane. The frequent introduction of glycerine tends to restore the external meatus to a healthy condition, and fit it for the proper transmission of sound.

I mention the following case as one amongst several which I have seen, proving that glycerine, if it be not quite pure, and made according to the proper formula, may cause irritation, and produce other unpleasant symptoms; and there is little doubt that from such cause this really valuable remedial agent has frequently suffered in repute.

A lady, living at the West-end, aged sixty-eight, consulted me two years since for deafness in both ears. They presented the appearance which indicated the use of glycerine. During eighteen months she lubricated her ears with this agent, receiving considerable assistance and comfort from it, and entirely laying aside the acoustic instrument which had been previously ordered. One morning I was hastily summoned to this lady; she was in considerable pain; the right ear greatly swollen and inflamed; she was much alarmed. She stated that she had used the glycerine regularly. The cause of the pain and swelling I soon discovered to be the stale and impure glycerine, which appeared quite changed in character, having an offensive smell, and being too light in color, of thin consistence, and deficient specific gravity. This *contretemps* was treated in the

usual way, and she again uses pure glycerine with the same advantage as before.

In the removal of foreign bodies from the ear the glycerine is often eminently useful. Some months since a solicitor of Gray's-inn consulted me respecting pain and deafness in the right ear; he had suffered much pain for five months. The annoyance had so affected his general health that he was quite incapacitated from following his professional pursuits. There was an occasional discharge from the ear. The meatus and external parts were swollen, sore, and emitted a light mucopurulent fluid. There was a mass of offensive looking wax. On being touched with a probe it was found hard and resisting. The glycerine was dropped into the ear in the manner just described. On the next day the impacted mass was easily removed. When examined, in the midst of it there was found a common *fly*, a foreign body which had evidently been the cause of the mischief. The use of the glycerine was continued for a few days, the pain ceased, and the hearing was soon restored.

The instrument called a *sonometer*, has proved of the greatest value in practice—not from any use it possesses as a curative agent, but that it proves, beyond a doubt, the effects of treatment, both to practitioner and patient. This must always be satisfactory. Thus you can test the progress of a case at stated intervals. It has done service to the profession.

It will be seen that sheep's wool is invariably used, instead of cotton wool. Its advantages are many: its elasticity enables it always to retain the position in which it is first placed; and for the same reason it is more easily withdrawn from the meatus. Cotton wool, when dry, changes its position, and in many cases is with great difficulty removed.

The mode of preparing the wool is very simple. The finest curled wool on the sheep's *head* is carefully cut with scizzors, and washed in hot water; when dry, it is ready for use. The best wool is that procured from a small German sheep. This, however, is not always to be procured, and the other answers every purpose well enough.

In the paper already alluded to at the commencement of this lecture, I published several reports of cases—and amongst them will be found that of a girl, an hospital patient, now living at Chad's-place, Gray's-inn, and that of a lady living in Adelaide-terrace, Islington,—the former had been deaf eighteen years, arising from scarlet fever; she was completely cured, and has not to this day any return of the distressing impediment. The other case is that of a lady, deaf thirty years, in whom the glycerine still acts as an artificial aid, and enables her to take

part in conversation. Her son was with me but a few days since, and he tells me that relief is always afforded by the application of the glycerine. These cases I have been enabled to watch for nearly two years, and it is satisfactory to know that the good effects in them have been permanent.

Glycerine is particularly useful in deafness following eruptive or other fevers; also in deafness arising from thickening of the drum, caused by an epithelial deposit. In the last-noticed condition, glycerine separates the epithelial excrescence, and thus restores the membrane to its natural state and appearance. Sir Astley Cooper, who at an early period of his brilliant professional career devoted much attention to diseases of the ear, used *nitrate of silver* for effecting a separation of the cellular formations in these cases. But the glycerine, perfectly innocuous, is a more effectual, and at the same time a safe, remedy. When the drum is perforated, the glycerine must be only applied to the walls of the meatus, care being taken not to use a sufficient quantity to admit of its being introduced or forced into the tympanic cavity. Should such an accident occur, tepid water ought to be immediately syringed into the ear, and the operation repeated three or four times. In this way the glycerine would be quickly removed.

In only one instance have I seen the glycerine produce pain or annoyance on its first application. A gentleman whom I saw in consultation with Mr. Guthrie suffered most acutely from the introduction of the remedy into his ear. A repetition of the operation was attended with a similar result. This is the only instance of the kind that I have witnessed. There was in that case a thickened tympanum, and an absence of cerumen. Apparently there was neither excoriation nor inflammation. The second application of the glycerine was made two days after the first. The effect precisely corresponded with the result in the first instance. It is almost needless to observe, that there was no repetition of the operation. The patient was highly nervous and excitable. Still I must confess that the cause of the pain felt by that gentleman remains entirely unexplained. In not another instance have I known any untoward event result from the introduction of glycerine into the human ear.

In old age there is usually a deficiency of cerumen, and the action of the glycerine is then very marked. It affords much comfort by allaying "irritation," which is the invariable distressing accompaniment of a dry meatus. The tinnitus in the ears is also frequently relieved by the soothing effects of glycerine, where that noise is occasioned by a dry meatus. At the same time, it must be remembered that many distressing cases

are due to abnormal conditions of the brain, nerves, heart, or bloodvessels.

The object of delivering this lecture is simply to confirm the reputation of Glycerine, and enlarge the boundary of its usefulness. For the relief and removal of some forms of deafness, Glycerine stands alone as a remedial agent. With a due perseverance in its employment by careful practitioners, it will often be the means of removing a distressing affliction.

It has been stated by some writers that Glycerine is an *oily* fluid, and therefore that is open to the objections that have been urged against oils. But you well know that it is not an oil, and cannot be classed with oleaginous substances. Let me remind you of its chemical properties, and of the method adopted for obtaining it. It is found in fatty oils combined with oleic, stearic, and margaric acids; its specific gravity is 1.252. Glycerine is a syrupy liquid, miscible both with alcohol and water, insoluble in ether, slightly inflammable, inodorous and of a sweet taste.

The most convenient mode of preparing it is by the saponification of olive oil, by means of litharge and a little water. Sulphuric acid will separate the oily matters, leaving an aqueous solution containing the alkaline salt along with the glycerine. The mixture is evaporated to dryness, and treated with alcohol, which again dissolves the glycerine, and leaves the alkaline sulphate undissolved. The glycerine may be purified from oxide of lead, by passing through it a current of sulphuretted hydrogen.—[*London Lancet*.

On a Functional Affection of the Spine, liable to be mistaken for Organic Disease. By HENRY KENNEDY, A. B., M. R. I. A.

The time of the society will be taken up but for a very few minutes while their attention is directed to an affection of the spine, of which several instances have now come under my notice, but which I do not recollect to have seen described in any work treating of the diseases of this part of the body.

One of the most striking features which practical medicine exhibits at the present day is the great advance which has been made in the diagnosis of disease. No part of the frame but has shared in this improvement; and though diagnosis, like everything else, may be carried too far, and so be useless for practical purposes, still it must be allowed to be one of the means by which we may hope to render medicine a more certain science than it at present is. It is with this impression that I venture to bring forward the following remarks.

The diseases of the spine are both numerous and important, and may be divided into the organic and functional. To even glance at these would be foreign to my present purpose, which is merely to speak of one in particular, and which may be arranged in the second class—that is, amongst the functional.

The affection I allude to is essentially a disease of the young, being seen most frequently between twelve and twenty years of age. I have, however, met with it as early as nine years, and as late as twenty-five. For so far I have only seen it in private practice, and it is more common amongst males than females, in the proportion of at least two to one. Its essence consists in a pain in the back, combined with a sense of weakness, and this is always referred to the lumbar region; at least I have never seen it higher up. The pain commences gradually, and may or may not be attended with feelings of weakness; and occasionally it is only the latter that is complained of. The patients will say that if they have occasion to stoop—as, for instance, to tie their shoe—the rising up gives the feeling as if the back would break. When we come to examine the spine, the patient is nearly always able to refer the suffering to a particular part; but I have seen cases where they could not do so, the feeling being then more diffused; and it is particularly worthy of notice that a rough examination of the part may be made—the spine may be twisted, or percussion strongly used, and yet the patient will not complain of it. When left to their own feelings, they invariably prefer the recumbent posture. Walking is much less irksome than sitting, and particularly when they have no support for the back. In addition to an ordinary chair, they will use a cushion, so as that it may press on the spine where they complain; and even when reclining at full length, it is not uncommon to see a cushion placed in the hollow of the back, and in this way to cause direct pressure. In fact the feeling of support is one they cannot do without, and they will use many devices to attain it.

But some may here ask, what is there of moment in the affection I have been speaking of; it is nothing but a pain in the back. They will probably think differently when they chance to meet a case of the sort I allude to. It has happened to me to have seen several instances where the individuals were obliged to give up their business, not only for weeks but for months, and owing to this pain; and the fact is enough itself to show that it is worthy of our notice. Those whose business leads them to stand and work at a desk, seem peculiarly liable to the affection. But probably an outline of one or two instances of the sort will give a better idea of the matter.

CASE 1. Mr. ———, a young gentleman of 18, whose em-

ployment was in an office in Dublin, where he spent six to seven hours a day, partly standing and partly sitting, at a desk, began to suffer from pain, in the small of the back. He was a person of small size, but of a highly developed nervous system, as was shown when he labored under any common indisposition; as, for instance, a cold. The pain in the back was trifling at first, though constant; so that he was able to pursue his usual avocations for about four months, when it became so distressing that he was compelled to confine himself to a sofa the greater portion of the day, and when he did sit up he always used a cushion between his back and the chair. On examination, nothing could be detected with the part of the spine of which he complained most; which was about the third lumbar vertebra. It could be twisted, and otherwise roughly handled, without causing any inconvenience, but to sit without support could not be endured beyond a few moments. And it is particularly to be observed, that when he had any inducement he could go through an amount of exertion on his feet, which seemed totally incompatible with the other symptoms present. His general health appeared good; his tongue clean; his appetite not impaired; and, in fact, except for the complaint of the back, he was otherwise well.

He continued in this state for four months, during which time he was seen by the late lamented Mr. Carmichael, and a variety of treatment was adopted; but I could not say that any means used was of decisive benefit. On this part of the subject I shall, however, speak again. In two months more he had recovered so far as to be able to resume his business, and at a still later period recovered completely; nor has he ever since, though some years have passed, suffered from the same affection.

CASE 2. A brother of the last patient, at the age of 21, began to suffer from pain in the back, attended by a feeling of weakness. He was also in an office where there was both standing and sitting, at a desk, necessary; but it was in a country town. At first it was merely an inconvenience; by degrees, however, it became worse, so that he could scarcely go through his duties, and finally he had to leave, and come to Dublin for advice. He was here seen by Mr. Cusack and myself and the symptoms were exactly those detailed in the last case, except that they were not of so severe a character. In this instance, too, there were some signs of dyspepsia present. After a certain period, but not so long as in the former instance, this patient also got well, and has remained free of any suffering from his back since, a period of five years having now elapsed,

Several other cases of a similar kind have come under my notice, but they are really so like the one to the other, that it appears to me quite unnecessary to do more than allude to them.

Of the exact nature of this affection I must confess my ignorance. At times I have thought that there might be some connexion between its causes and the remarkable changes which occur in the constitution at the period of life at which it is most usually seen—that is, between boyhood and manhood. On the other hand, I have seen it at a time of life when it might be supposed that these changes had all ceased. Thus, in the last case given the patient's age was 21; and I have seen the affection in persons even older than this. Again, I have thought that there might have existed some derangement of the general health, which though not tangible, was not the less real; for I presume few will question the fact that there frequently is deranged health without its being cognizable to our senses. In support of this view of the matter some reasons will be adduced further on. As a whole, then, I would be inclined to say—though it be anything but proved—that this affection arises from causes incident to the period of life at which it occurs, conjoined with some obscure derangement of the general health. But I must again repeat that this is but conjecture.

The diagnosis of this affection is of some moment; for the symptoms are of such a character as to lead one to dread the existence of serious disease; or it may be confounded a functional with an organic disease—a mistake of no little moment, and which has, I know, occurred. A young lady while abroad got the affection to which I have been directing my attention. She was advised to keep the recumbent posture, and to have issues put in. This advice was persevered in for some months, when she was brought home, and seen by Sir Philip Crampton, who at once ordered the issues to be dried, and the patient to get up, and go about. This was done, and the lady recovered, though slowly; for her general health had been injured by her long confinement. Similar cases have, I believe, come under the notice of others. The diagnosis is then of importance, and will, I rather think, be found to turn on the presence or absence of constitutional symptoms; such as a quick pulse, night sweats, &c. I have not met any of those in the affection now under notice; nor indeed any marked derangement whatever of the general health; such as exists in the great majority at least of the cases where organic disease either exists or is about to exist. But in addition, we have also the fact, that even a rough examination of the spine does not cause any pain to the patient—a marked contrast as I take it between the two

cases; and lastly, we have the situation as affording us some assistance; for in the one affection it is, as far as I have seen, always in the lumbar region, and sometimes even at one side of the vertebral column; while in the other, organic disease is not probably so common here as higher up; but on this point I am doubtful.

From what has preceded, it may be guessed what our prognosis, in the class of cases under consideration, should be. The patient may be assured that he will get well; but the important point to keep in mind is this, that his recovery will occupy some time. I have seen no instance where the affection was completely got rid of till six months had elapsed; and in some of the cases it was much longer than even this. It is an affection, too, that I have known return, after the patient had appeared to be well for months. To say that no case could run into more serious disease, would be going further than common prudence would justify; for in truth this affection, when well marked, is sufficiently distressing. But I may state that I have not met any case of the sort. In one instance, indeed, a patient labored under it when she was between eleven and twelve years of age. She recovered perfectly, but about two years subsequently was attacked with caries of the cervical vertebræ, which ultimately proved fatal by the disease extending to the membranes of the brain. With this exception—and it is not, you will observe, a case exactly in point—I have known no fatal result in connexion with any of the cases of that form of spinal affection now under the notice of the meeting. In some instances, however, I confess serious apprehensions occupied my mind, and it was only the complete recovery of the patient dispelled them.

On the subject of treatment, I have nothing of a very definite nature to offer. A considerable variety of means have been used, of both a local and constitutional kind. The former include local bleeding, dry cupping, blisters, frictions, the cold douche, and galvanism; and the latter aperients, tonics, change of air, and relaxation from business. Of these two, the latter have, in my experience, proved by much the most useful. I should state, however, that I have seen benefit follow the application of small and repeated blisters, as also the use of a weak stream of galvanism, applied daily, or every second day, according to circumstances. The patient, too, has often got great relief from wearing a stiff belt; indeed, this is a measure which should not, in any case, be forgotten. Still the general measures are the more important. In all the severer cases the patient will have to give up his business for a time; two or three months complete relaxation must be enjoined, and if the

patient can change his air so much the better. In one case which I saw with Mr. Cusack, he advised the patient going to one of the watering places in England for some weeks; and great benefit followed this measure. With this was advised a strict attention to the bowels, and also a course of dry cupping. It is only due to Mr. Cusack to state that he seemed to be perfectly familiar with the affection.

With these means is connected the question how far the patient is to be advised confinement in the first instance. As the result of the experience I have had, I should say that exercise, more or less, according to circumstances, should be advised to be taken daily; not of course to be carried to fatigue: at the same time that the patient, while at rest, may with advantage assume the horizontal position, propped up or supported in the way most agreeable to him. I have already alluded to a case where strict confinement was enforced, and certainly with no improvement to the patient, and I believe that such will do much more harm than good in the class of cases that I wish more particularly to bring under notice.

Besides the relaxation from business, and change of air already spoken of, there are other general measures, such as the cold douche, frictions, &c., all of which are worthy of trial; but which need not be enlarged upon here. As to medicines, I have tried a number, more particularly those of the tonic class; excepting strychnine, however, from which in some of the cases benefit has arisen, the others have appeared to produce little or no effect.

Such are the observations I have to offer on this affection. I have been induced to bring them forward here, though it is highly probable that several gentlemen, whom I address, are quite familiar with the affection itself. In looking over the several works, however, which treat of the diseases of the spine, I do not find this one noticed; it appears to myself to present features of a distinct character, while it is certainly of importance to be aware that such an affection exists, and that it may be readily confounded with other and more serious diseases.

A discussion followed, in which Messrs. Bagot, Rumley, Fleming, Egan, and others, took part. Cases were adduced in which symptoms similar to those mentioned by Dr. Kennedy were the result of nocturnal emissions, and similar discharges. But it was acknowledged that in some instances no such causes could be assigned.—[*Dublin Medical Press.*

Detail of a Case of Phagedenic Chancre, with some remarks on that Disease. Taken from the Lectures of RICORD. By WILLIAM H. ANDERSON, M. D., of Mobile, Ala.

No man is better known to the medical profession throughout the world, than the distinguished surgeon of the Hôpital du Midi. His deep researches into the most disgusting diseases that afflict civilized society, and the clearness and accuracy with which he describes the results of his investigations, induce us to seize with avidity, and treasure up, as eminently useful, all scientific matter that escapes from his pen, and from his lips. They who have followed him up in his wards, can well attest to the great benefit which he has been to mankind, and if they remember, with pain, the loathsome objects of human suffering which they have seen, they must at the same time recall with pride the triumphs of modern Medicine, and have the most pleasing recollections of the personal vivacity, lively style and agreeable bon mots of the distinguished lecturer. While there is no branch of venereal disease which he has not elucidated by his researches, there is certainly none for the correct treatment of which he has done more, than for the one which heads this article. Having lately had under treatment a case of this nature, I proceed to detail the case and make some remarks upon it.

J. F., a man of 22 years of age, consulted me in October last, to get advice about the treatment of a syphilitic phagedenic chancre, of which he had been, for the previous four months, the unfortunate victim. He was of sanguine and lymphatic temperament, with a predominance of the former; had enjoyed from childhood excellent health, and, until lately, was rather robust than otherwise. His general condition, when he presented himself, was pitiable in the extreme: body, much emaciated; countenance, pale; eyes, sunken; expression, languid; strength, feeble, and appetite gone—to which symptoms I may add general anæmia and protracted diarrhœa. He had contracted syphilis eight months previous, and had been under treatment from the first appearance of the chancre. The usual quantity of mercury had been administered, without removing, or even benefiting, the disease, and he had taken large quantities of sarsaparilla and hydriodate of potash. On examining his chest, to ascertain the cause of a slight cough, I found that he had a distinct souffle, produced at the aortic orifice, and continuing itself into the carotids, where it might be distinctly heard.

The chancre itself occupied a large portion of the dorsum of the penis, was oblong and irregular, and gave rise to a foetid

but rather scanty discharge. Its size was about that of a dollar. The edges were ragged and burrowed; the bottom of the sore was irregular but not granulated, and the depth did not reach beyond the subcutaneous cellular tissue. From the bottom, a fœtid pulraceous fluid constantly exuded, and formed itself into tough masses, about the consistence of the half-dried secretion of the Schniderian membrane. This chancre had been burned with caustic repeatedly, and had exhausted a variety of local applications, such as the black wash, the yellow wash, calomel aromatic wine, opiated solutions, etc. From a close examination into the history of the case, I felt satisfied that there was nothing left for me to do, but to put him on a new course of general and local remedies. I had no reason to believe that the constitution of the patient was affected with the syphilitic taint. He had never had bubo of the groin, nor had he suffered with any of the cutaneous eruptions, with ulcerated throat, or rheumatism. I considered the disease, therefore, as one essentially local, which, by the continued discharge and ill condition of the ulcer, had impaired the blood, dejected the spirits, and undermined the constitution. I gladly embraced the opportunity of using the tartrate of iron and potassa, a remedy which I had seen used in phagedena with great success, and on the administration of which I heard a lecture by Ricord, which, on account of its practical value, I will subjoin to this paper. Under the general and local treatment of this remedy, the patient improved, and was discharged at the end of three months, cured of his chancre and restored to general health.

That form of chancre called 'phagedenic,' according to Ricord, does not belong to the class of *primitive* ulcers. It is always the result of some vice which owes its origin to a local cause, or to a predisposition, either general or acquired. The local vice which occasions it, may be either simple or inflammatory œdema, inflammation without œdema, or strangulation. It is sometimes owing to dressings which have been ill-timed and badly applied. The cause is sometimes very difficult to appreciate, but come from whatever source it may, it always brings about the same results. As a general rule, one of the following causes will be found, in any given case of phagedena, to exist:—Weak constitution, chloro-ancæmia, great privations, excesses of any kind, living in damp, unwholesome districts, lymphatic temperament, scrofula, abuse of mercury, scorbutis, old or recent syphilis. The local condition, already mentioned, may be joined to any of these general cases.

In rare cases, any of these causes may exist, and yet produce no appreciable effect on the constitution of the patient. In such cases, the only phenomena observable are those which the

ulceration presents, and on these *local* appearances, the treatment with tartrate of iron and potassa is based. No practitioner is ignorant of the obstinacy of these phagedenic ulcers, and of the uncertainty of the remedies which have been opposed to them. The object of this paper is merely to present a *resumé* of the results obtained by means of the remedy in question. To this end, an investigation into the nature of phagedenic ulcers will the better enable us to appreciate the sanitary influence which the martial preparations exert over them.

In the first place, phagedena is by no means a proof of *constitutional* infection. On the contrary, when it is not a sequence of *indurated* chancre, it is almost always a guarantee against this infection. Acting on this principle, Ricord never administers mercury as an antiphlogistic, in the treatment of this modification of syphilitic ulcer, excepting only in those cases where the phagedenic state is owing to an anterior syphilitic affection.

The ulcer may take on several different forms: it may be gangrenous, serpiginous, pultaceous or diphtheritic; but is always covered with a pultaceous layer, of greater or less thickness. Without describing, minutely, each one of these varieties, it may be well to give a description of that which we most frequently meet with.

Phagedenic ulcer, then, is more or less large, generally superficial, rarely extending in depth beneath the subcutaneous cellular tissue. Its shape, sometimes round, is more often irregular; its edges are of a brownish color, and are so much burrowed as to fall into the ulcer. Their base is a little engorged. The bottom of the ulcer is irregular; it presents here and there little cicatrized spots, but the greater part of its surface is covered with a tough greenish-yellow matter, which it is sometimes difficult to detach, and which comes off in distinct flaps. Fleishy granulations, are rarely seen, and if they do exist, they are pale, flabby, without color, and resemble vesicles. Most generally, the surface is of a grayish color, and scattered over with little red points, which easily bleed; the pus is thin, grayish and fœtid, and holds in suspension the *débris* of the tissues, and little flocculi of pultaceous matter. In the *progressive* stage of the ulcer, this pus will answer the purpose of inoculation. The duration of the ulcer is always long, and cicatrization, under any treatment, is slowly accomplished.

Ricord maintains that individuals affected with phagedenic ulcer are most generally exempt from constitutional infection. Often they have no other symptoms or lesions than those belonging to the local disease; but it is not to be denied, that cases do exist where the same poison which has produced the

phagedena, has told, also, severely on the general health. Hence, we sometimes see patients suffering with great languor, headache, palpitation of the heart, carotid murmurs, neuralgic pains in the stomach, pallor of the skin and of the mucous tissues, cutaneous eruptions, both general and immediately around the sore. When the ulcer is very large, and furnishes an abundant suppuration, there is a great wasting away of the body, which adds to the difficulty, and sometimes renders the cure protracted and uncertain.

Phagedenic ulcer, such as has been described, has always been considered a very serious disorder. We need no better proof of this than the accounts which the best authors give of it, and particularly the great variety of treatment which has been recommended for the treatment of it. It is unnecessary to refer to all these therapeutical remedies; suffice it to say, that they are all feeble and unimportant, when compared with the tartrate of iron and potassa. The administration of iron in phagedena is not of recent date, but hitherto it has been given only in small doses, at least when compared to the quantity which Ricord now thinks it necessary to administer. Previous to the last two or three years, fifty or sixty grains a-day was considered sufficient, but at the present time it is well ascertained that we may commence with half an ounce, and reach, gradually, double that quantity. The effect of such doses on the ulceration soon begins to display itself; even as early as the third day the benefit has been appreciated, and the ulcer has shown evident signs of improvement. The pultaceous matter which covers the bottom of it, first commences to be more easily detached. The suppuration assumes a more healthy appearance, and does not coagulate on the surface of the wound; the fleshy granulations, from being pale and transparent, become more red; the pus, which was thin and serous, and loaded with the detritus of the ulcer, is more homogeneous; the edges of the ulcer take on a more natural color, become by degrees absorbed, and rounded off, as it were. This latter is the first sign of cicatrization, and indicates a return of the phagedena to a simple sore, and a disposition to heal without delay. The method of cure, however, is subject to some variations, worthy of mentioning. It sometimes happens, for instance, that the cicatrization takes place with great difficulty, commencing late in the course of treatment. In such cases, the borders of the ulcer draw up, as it were, towards the centre; the wound undergoes a sort of crisping process, which greatly reduces its size, and the surface, deprived of its flabby granulations, seems to draw up rather than to cicatrize. Occasionally, too, the wound will undergo a process of cure by regular granu-

lation; but that is exceedingly rare. When the ulcer covers a very large extent of the penis, the cicatrix will form at several points, and proceed from centre to circumference. Finally, in that variety of phagedenic ulcer called *serpiginous*, cicatrization will be going on in one place, while ulceration is extending in another. This, however, should not give the physician uneasiness, because the one process always goes on faster than the other, and the wound heals.

The local treatment of these ulcers has been very variable. It has been the custom to dress the surface with aromatic wine—with the decoction of poppy heads, the solution of iodine, the powder of charcoal, of Peruvian bark, etc. But lately, Ricord uses, exclusively, the solution of the tartrate of iron and potassa. The first effect of this dressing may discourage the physician who has never seen it employed, for it gives to the wound a very bad appearance, owing to the color it imparts to the tissues; a little perseverance, however, soon manifests a marked difference in the general appearance of the ulcer. Without denying the healing property of other local means, in conjunction with the internal use of the iron, we feel justified in saying that they can claim only a secondary reputation, when compared with the curative agency of the remedy in question.

An accidental circumstance which twice presented itself to Ricord, furnished incontestible proof of the utility of the tartrate of iron. He was obliged to suspend its employment at two different times, and once for the period of eight or nine days. In both instances the sores retrograded, and the patients complained bitterly of renewed appearances of phagedena. The wounds returned to their original state; the bottom resumed its pultaceous, grayish appearance, and the discharge became sanious and unhealthy. The re-administration of the remedy did not fail to produce decided improvement.

The length of time during which this ferruginous treatment should be employed, cannot be definitely stated; and, indeed, it must be based on the actual state of the ulcer. In general, it is best to continue the treatment until cicatrization is *complete*. If there be the smallest point of a phagedenic ulcer still unhealed, and we omit the use of remedies, it will soon spread itself in the newly cicatrized tissue around it, and will be the more difficult to heal, from the very fact of its having invaded this peculiar tissue. It is best, then, to push the remedy to a complete cure. The shortest continuation of a phagedenic ulcer, after the commencement of the use of the iron, is seventeen days. This relates to a case of phagedena occupying the extremity of the penis. In other patients, the cure does not go on so rapidly. Three patients were upwards of three months

in regaining their health, but in all three it was necessary, once or twice, to suspend the remedy, in order to combat other symptoms of importance.

The pathological effects produced by the tartrate, in large doses, are scarcely ever serious enough to suspend its employment. All the patients, even the most prostrate, seem to bear it well. At the end of the third or fourth day, sometimes later, the skin and mucous membrane regains its healthy appearance, the patients suffer with a little heaviness about the head, the pulse becomes stronger, and the vitality of the system is evidently augmented. The arterial souffle, which existed before administration of the medicine, disappears by degrees. A very large dose sometimes produces pain, and a sense of weight about the stomach, but rarely occasions vomiting. As a general rule, the appetite increases, the evacuations become dark, and sometimes there is a disposition to diarrhœa. The color of the teeth is in no wise affected, but they retain their whiteness throughout the treatment.

The daily dose of the tartrate is from half an ounce to an ounce, dissolved in water. It is best to begin with about two drachms, and at the end of ten days to arrive at the full dose.

In investigating the *rationale* of the treatment above alluded to, I must confess that I am at a loss to account for the reasons why the remedy in question possesses such a controlling influence over phagedenic chancre. It coincides, to be sure, with our experience as to the therapeutical effects of the ferruginous preparations, but then other salts of iron seem to be so far inferior to the tartrate, that we are naturally led to believe that there is something specific in this preparation. The carbonate of iron, the sulphate, the muriated tincture, have all been tried, and although each has its effect in restoring, to a certain extent, the general health, yet no one of them can claim the same efficacy as the tartrate. What virtue the potash contained in the preparation is entitled to, is a subject of speculation. Probably it is productive of no important results, since potash has been given along with the other preparations, without increasing their power.—[*New Orleans Med. and Surg. Journal*.

On the Treatment of Ophthalmia in general. By Professor
LANGENBECK.

Professor Langenbeck, of Erlangen, lays down some general principles for the treatment of inflammation of the eyes, which may be often usefully borne in mind.

1. A slight *revulsion on the intestinal canal or skin* suffices for the cure of mild inflammations of the eyelids and conjunc-

tiva, especially in children, provided that neither the inflamed part nor the patient's constitution have undergone any material change. When the inflammation is consequent on the irritation of foreign or chemical bodies, or is sympathetic of a disordered state of the general economy, active exercise in the air for some hours, an antiphlogistic regimen, and care in using the organ, are required.

2. When the inflammation is more active, but the constitution still sound, a *more active revulsion* is required, and may be procured by strong purges and enemata, and the frequent application of large blisters or sinapisms to the calves, thighs, sacrum, or nucha. Friction of the feet with *Spt. sinapismi æthereus*, and then covering them up, soon induces revulsion. In blenorrhœal, scrofulous ophthalmia, &c., these means are still of use, though not alone curative.

3. The *irritation of the nasal mucous membrane* is an excellent means when the disease is not removed in a few days, and is inclined to become chronic or relapsing, and especially if it assumes a distinct catarrhal character. A pinch of Spanish snuff may be taken every two hours, a little black pepper applied, or the infusion of chamomile inhaled. This last, accompanied by a blister to the neck, soon disperses very obstinate cases, occurring in persons predisposed to angina.

(We may observe that M. Taignot is a strong advocate for exciting revulsive action on the Schneiderian membrane, in the subacute stage of scrofulous, and some other forms of ophthalmia. To this end he either touches the mucous membrane daily with a pencil of nitrate of silver, or with a little ointment containing 1-10th of this substance. But to these means he prefers, when the patient is old enough to know how to use them, stimulating substances mixed up with iris powder, and taken as snuffs. Thus he uses a powder formed of 30 parts of iris powder, camphor 1 part, and sulph. zinc or copper from 2 to 8 parts. Or 2 parts of nitr. silver, or 1 of cantharides may be substituted for these salts, retaining the camphor. (See *L'Union Médicale*, No. lxxix.)

4. *Special excitement of the functions of the skin.*—Suppose the disease takes on the form of scleritis or rheumatic ophthalmia, with great irritability of the eye, and disposition to relapse on slight atmospheric changes. In such cases, besides the local treatment to be adverted to, great benefit accrues from exciting the functions of the skin by *tartar emetic vomitings*, repeated three or four times, continuing the medicine in smaller doses for a while afterwards, and keeping the patient in as warm a medium as possible. In other cases great benefit results from administering the *Sp. mindereri* in bland fluids,

and keeping the body hot for twenty-four or forty-eight hours, frequently the while applying friction to it with a flesh brush, or passing a hot domestic iron over it when covered with a blanket.

5. *Derivation to the glands.*—In scrofulous ophthalmia, which is very frequent in the author's neighborhood, he finds great advantage from exciting inflammation or even suppuration by repeated blisters, or other revulsives in glands liable easily to become inflamed.

6. *Derivation by issues.*—This is especially indicated in those forms of ophthalmia, in which opacity of the vitreous humour is a common result, as hyaloiditis, keratitis, iritis, uveitis, and periphakitis. In such, into an issue opened in front of the ear or on the temple, a little powder of equal parts of salt and borax may be placed several times a day, and forms a powerful adjuvant to antiphlogistics.

7. *Revulsion on the joints* is a powerful adjuvant in persons who have already suffered from gout or rheumatism, or who by age or constitution seem especially predisposed to them, the joints being covered by flying blisters, or strong sinapisms. Dr. Langenbeck has frequently had recourse to this means prior to operations for cataract or artificial pupil, in order to prevent subsequent inflammations in such subjects.

8. *Bleeding*, whether local or general, is usually useless in superficial inflammations of the eye, unless they acquire great intensity, and threaten to implicate the deeper structures, when small general bleedings or leeches are necessary. In children even, bleeding from one to three oz. is usually better than leeches, which when used should not be applied to the cheeks, temples, or mastoid processes, where they may even do mischief, but at a distance, and especially along the course of the carotids.

9. *Local application of cold.*—Upon this subject Dr. Langenbeck offers some very minute directions, not only in respect to the cases to be chosen for its use, but its mode of application. He observes, that in many cases wherein cold is useful, wet is mischievous, and in others where this is not the case, it becomes so through faulty management. If merely permanent cold is required, he employs hollow horn rings, into which pieces of ice wrapped up in rag are introduced, which are replaced, when melted, by other pieces. When applied, the apparatus looks like a monster pair of spectacles, and is large enough to be supported on the orbit without compressing the globe of the eye at all. A piece of sponge is laid on the cheek to receive the fluid as it melts. Another plan of producing great cold, to which he gives a preference, is to place the patient on his back,

with his eyes shut, and having deposited a small portion of a powder, formed of equal parts of nitre and sal ammoniac, in the inner angle of the eye, to allow water to fall on it *guttatim* until it is dissolved; the fluid so formed may be retained on the eye until heated, and then renewed. The author attributes the production of the good results which follow this plan, not only to the intense cold which is produced, but to the antiphlogistic effect of the salts which gain admission into the eye, on the partial opening of the eyelid. Repetition of this from four to eight times, at intervals of from ten to twenty minutes, is equivalent to glacial applications for twenty-four or forty-eight hours; and, indeed, the two, if necessary, may be combined. Persons who have no assistance, may manage the powder by inclosing a certain quantity in linen rags, and moistening it. So too glass globules may be filled with ice or this refrigerant.

As a general rule, *the local use of cold is proper in all cases which are not dependent upon evident constitutional cause or dyscrasis.* Certain exceptions to this must, however, be observed; as—1. When inflammation of the fibrous textures of the eye has extended to expansions of the motor apparatus of the globe. Recent *rheumatic* ophthalmia is, however, benefited by cold affusions, lasting from three to ten minutes, and chronic cases by dry cold. 2. In *erysipelatous* inflammations, cold affusion is interdicted. The temporary application of dry cold, and the covering up the forehead and cheek with taffetas, induces active transpiration. 3. *Blennorrhæal* inflammations are not dangerous to the internal structure of the eye in general, only inasmuch as the cornea becomes injured by the irritating discharges; and it is chiefly for the purpose of cleansing these from it, that washes are resorted to. In *acute ophthalmia of infants*, the author opens the eye every ten minutes and inserts a piece of ice within, or a few drops of a concentrated solution of alum and tannin; and however long the disease may last, no injury will accrue to the cornea if the discharge is removed as fast as formed by cold water. 4. Chronic ophthalmias, and especially in the aged, are far more favorably influenced by the use of cold from time to time than by its continuous use, the dilated vessels and exudations being thus more advantageously modified; and exudations are sometimes much influenced by sudden douches propelled with force, even if composed of warm water. These form, too, one of the best stimulants in nervous affections of the eye.

A second general rule is, that in the treatment of all ophthalmias, dependent on a constitutional cause, but not on a dyscrasis or cachæmia (as hæmorrhoidal, menstrual, abdominal ophthalmias,) the local use of cold, applied as energetically as possible,

is the best means. If severe the *douche* may be continued for six or eight hours. It is especially when there is a varicose state of the vessels of the eye left, or that *engorgement* of the choroid accompanies conjunctivitis, that these douches are so useful. So in a tonic injection of the conjunctiva, especially the palpebral, the *douche*, repeated several times daily, is far more useful than any astringent injection.

A third rule is that in ophthalmias, seated on a tissue in a state of dycrasis from evident alteration of the blood, whether it is a mere local symptom of such altered condition, or whether it has been induced by external agency, dry permanent cold, not moist cold, which would favor softening of the cornea, is indicated.—[*Annals d'Oculistique. Med. Chir. Rev.*

Observations on the Nature and Treatment of Various Diseases.

By ROBERT J. GRAVES, M. D., F. R. S.

Singular Defect and Impotence of Memory after Paralysis.—A farmer in the county of Wicklow, in comfortable circumstances, when fifty years of age, had a paralytic fit, in the year 1839; since that time he never recovered the use of the affected side, and still labors under a painful degree of hesitation of speech. He is, however, able to walk about, take a great deal of active exercise, and superintend the business of his farm. His memory seems to be tolerably good for all parts of speech except noun-substantives and proper names; the latter he cannot at all retain; and this defect is accompanied by the following singular peculiarity—that he perfectly recollects the initial letter of every substantive or proper name for which he has occasion in conversation, though he cannot recall to his memory the word itself. Experience, therefore, has taught him the utility of having written in manuscript a list of the things he is in the habit of calling for or speaking about, including the proper names of his children, servants and acquaintances: all these he has arranged alphabetically in a little pocket dictionary, which he uses as follows:—If he wishes to ask any thing about a cow, before he commences the sentence he turns to the letter C, and looks out for the word ‘cow,’ and keeps his finger and eye fixed on the word until he has finished the sentence. He can pronounce the word ‘cow,’ in its proper place, so long as he has his eye fixed upon the written letters; but the moment he shuts the book it passes out of his memory, and cannot be recalled, although he recollects its initial, and can refer to it again when necessary. In the same way when he comes to Dublin, and wishes to consult me, (for my name is among the indispensable proper names in his dictionary,) he comes with his dictionary

open to the hall-door, and asks to see Dr. Graves; but if, by accident, he has forgotten his dictionary, as happened on one occasion, he is totally unable to tell the servant what or whom he wants. He cannot recollect his own name unless he looks out for it, nor the name of any person of his acquaintance; but he is never for a moment at a loss for the initial which is to guide him in his search for the word he seeks.

His is a remarkably exaggerated degree of the common defect of memory, observed in the diseases of old age, and in which the names of persons and things are frequently forgotten, although their initials are recollected. It is strange that substantives and proper names, words which are the first acquired by the memory in childhood, are sooner forgotten than verbs, adjectives, and other parts of speech, which are a much later acquisition.

A lady, about fifty years of age, who was laboring under what is popularly termed a breaking-up of the system,—that is, a simultaneous decrease in the energy of all the vital functions,—showed among the first symptoms a defect of memory similar to that which I have related above. The first name which she was perceived frequently to forget was that of a family with whom she was very intimate, and whom she saw almost every day, and she was much tormented by this defect, whenever she had occasion to refer to any of its members in conversation. After a time this defect extended to the names of other persons and things; in the course of a few months she lapsed into a general want of memory, and weakness of intellect.

It is interesting to compare such cases with the temporary loss of memory which is produced by inebriety, and the permanent loss of the same faculty that shows itself in old age. Such a comparison proves that diseases of the brain occasion a defect of memory, which is but an exaggeration of that observed in old age and in inebriety; and it is, therefore, to be attributed, not to any affection of any particular portion of the brain, but to a general derangement of the cerebral functions. Some medical men are inclined to think that where, under such circumstances, the memory is very deficient and the intellect weak, softening of the brain exists; but the preceding observations show that such a conclusion is derived from a very partial view of the subject, inasmuch as the patient, whose case I have first referred to, is still living, and is much in the same state that followed the paralytic stroke eleven years ago.

The effects produced on the memory by paralysis are by no means proportionate to the loss of muscular power that the disease gives rise to; and the same disproportion exists also

with respect to the generative powers. Thus I have known several cases in which young men who were attacked with apoplexy and hemiplegia, from which they recovered with a very imperfectly restored muscular power of the limbs and speech, became subsequently the fathers of several healthy children. On the other hand, I have seen two cases where the cerebral attack was so slight as not to produce more than a transitory giddiness, a passing feeling of terror, and some hesitation of speech with a little subsequent numbness in the arm and cheek, and slight weakness of the leg at the same side. All these palpable symptoms passed away within twenty-four hours, leaving behind scarcely an evident trace of diminished power in the limbs, and no impairment of any of the senses, articulation, or memory; yet the cerebral attack occasioned, from the very moment of its occurrence, a complete impotency, which in both cases has been for many years permanent, although, as I have said before, both individuals are in other respects quite healthy.

Paralysis affecting the Teeth.—In a former paper I remarked that although the teeth are possessed of an exquisite sense of touch, and are frequently the seat of intense pain, yet no one (as far as I could ascertain) had observed in paralysis a loss of sensation in the teeth. I have been for years on the watch for this symptom, and have at length detected it in a gentleman who has had several attacks of hemiplegia, each accompanied by complete numbness of all the teeth at one side of his mouth.

Lethargy. It is curious how certain derangements of the functions of the brain occur without being accompanied by other notable symptoms of disease. Thus, I know a gentleman advanced in life, and of plethoric habits, who has been for several years affected with lethargic symptoms, but without any headache, tendency to paralysis, or impairment of his general mental energies. He is frequently attacked, however, even at his meals, with unconquerable sleepiness, and it is surprising how suddenly it comes on; thus, he will be sitting, talking quite cheerfully, and unexpectedly he drops into a sleep, which lasts for about half a minute or a minute, and then he arouses himself, and continues awake for a few minutes longer. This happens so often that he cannot now venture to go into company. And, as I have said before, this drowsiness comes on so quickly that at one meal he has broken three or four glasses by becoming unconscious after the act of filling the glass, and during the time he was raising it to his mouth. He was consequently obliged to have an attendant to watch him going to

bed, lest he might fall asleep in an inconvenient place or position, or might endanger the safety of the house by allowing the candle to fall. Whether his immunity from other symptoms arises from a seton in his neck, which he was advised in London to have inserted, I cannot tell; but this state of the cerebral functions, existing so long, and without any additional symptoms, is very curious.—[*Dublin Quarterly Journal*.

A Case of Poisoning from Opium, successfully treated by Electro-Magnetism. By J. B. BIDDLE, M. D.

The following case illustrates, I think, very strikingly, the value of the electro-magnetic current as a means of relieving the coma produced by narcotic poisoning.

At about half-past twelve o'clock of the night of the twenty-eighth of April last, I was called to visit a woman, described by the messenger as being in a fit. No history or explanation of the case could be obtained, except that the patient had gone out at about half-past seven o'clock to get something at an apothecary's for a cramp colic; that she had upon her return home eaten her supper as usual, then gone to bed, soon fallen into deep sleep, and finally, at about midnight, from her unusual respiration and the impossibility of rousing her, excited the alarm of her husband and family.

I found her in a state of profound torpor; her breathing extremely slow and interrupted, stertorous and gasping, with spasm of the throat, lividity of the countenance, inability to swallow, utter insensibility to the most violent agitation, pupil contracted to the size of a pin's head, pulse scarcely perceptible at the wrist—in short, all the symptoms of an advanced stage of asphyxia. That it was a case of narcotic poisoning, rapidly approaching a fatal termination, was, I thought, evident, and I at once so expressed myself—the family, however, still professing themselves unable to explain or account for it.

Acting, however, upon this opinion, I obtained the assistance of my friend, Dr. Goddard, who lives in the neighborhood, and the use of his electro-magnetic apparatus; and, the doctor coinciding in my view of the case, we determined, although with no very strong hope of saving the woman's life, to resort to this agent. An attempt was made to introduce the stomach tube, but was unsuccessful, owing to spasm of the pharynx, and its introduction could have been of no service, as, at the lapse of more than five hours, the poison must have been altogether absorbed from the stomach.

The electro-magnetic machine employed consists of two coils

rotating between the poles of two horse-shoe magnets—an unusually large and powerful instrument, producing a rapid succession of violent shocks. One pole was applied to the nape of the neck, the other to the pit of the stomach. For about two minutes after the battery was started no effect was produced. The patient then began to make convulsive efforts with her hands, as if to put away something annoying her, and, in perhaps half a minute more, she opened her eyes with a ghastly stare. The battery being still kept in action, she rose up in bed, and was able to mutter some indistinct answers to questions put her.

Upon withdrawing the electric current, the woman immediately sank back into the state of torpor in which I had found her. But, as soon as it was renewed, artificial vitality was again restored. When the current was a second time stopped, after about the same period of application as at first, the woman continued for some two or three minutes awake, gradually, however, relapsing into *coma*. After each application of the battery, the interval of consciousness became longer, and, at the end of two hours, she remained roused for a full half hour, in which she was able to let us know what she had taken.

It appears that she had bought “three cents” worth of *laudanum*, and, never having taken it before, she supposed it was a proper dose, and swallowed it all. It amounted, as she said, to some three tea-spoonfuls—probably two fluid drachms, as this is, I believe, the quantity usually sold for that price. I think it probable that she was also previously somewhat under the influence of whiskey, as we detected it on her breath, and this must have increased the narcotic effect of the *laudanum*.

We now gave her some volatile alkali, and strong coffee, but they were not long retained. After half an hour’s consciousness, stupor slowly crept on again, and a further resort was had to the battery, which was followed with rapid, and, as it proved, a final revival.

The patient now got up, walked about, conversed clearly, was able to keep some coffee on her stomach, and it was apparent that she had at last struggled through the effects of the narcotic. Some disposition to somnolence remained, but this was easily overcome, without recourse to the battery. I remained with her till half-past four—an hour and a half from the last application of the electricity, and then left her in charge of her friends, directing them not to suffer her to sleep until I saw her again.

Between eight and nine I found her very comfortable and completely awake, although begging hard to be allowed a nap.

Three or four hours natural sleep now took place, and left her completely recovered.

It may be worth mentioning, that in the successive applications of the poles of the battery, while one was kept constantly to the nape of the neck, the other was placed indifferently at the pit of the stomach, the arm-pit, and in the hand; and the effect did not appear to vary.

Since drawing up the notes of this case, upon mentioning to my friend, Dr. Mütter, I found that he had lately resorted to electro-magnetism with success under similar circumstances; and he kindly offered the history of his case for publication with the foregoing.

MAY 14th, 1851.

Dear Doctor:—In accordance with your request, I send a brief outline of the case of “poisoning with opium,” to which I referred in our interview the other day.

Last spring, my colleague, Prof. Pancoast, and myself, were summoned about 11 o'clock, P. M., to visit a young gentleman residing at the corner of Ninth and Market streets. On our arrival we found that a large quantity of laudanum had been swallowed accidentally, and although strong and very appropriate means had been immediately taken by several medical students who lodged in the same house, no impression seemed to be made upon the influence of the drug. All the evidences of rapidly approaching death were manifest, and as all other measures had been unsuccessfully employed, we determined to employ *electro-magnetism*. An instrument was accordingly obtained, one pole placed upon the nape of the neck, and the other over the epigastrium. Almost on the instant, the muscles of respiration were violently agitated, and the patient sprang up in bed, opened his eyes, and answered questions. The pain in a few moments was so severe, that we were obliged to change the position of the poles of the machine. Keeping one steadily applied to the back of the neck, the other was made to touch different points of the thorax, throat, abdomen and upper extremities. The *burning* sensation occasioned by the fluid, was almost intolerable, causing the patient to complain loudly, and effectually preventing any return to the lethargy from which he had so happily been aroused. We deemed it most prudent to continue our efforts, even after the patient was fully restored to consciousness, but I think not more than *an hour* elapsed between the first application of the remedy and the complete relief of our young friend.—[*Medical Examiner*.

On so-called Chylous Urine. By H. B. JONES, M.D., A.M.F.R.S.

The definition given of chylous urine is, that it is urine which is white from the suspension of fatty matter in it. An opportunity of observing a case of this disease having occurred to the author, he was led to make the experiments described in this paper. A harness-maker, æt. 32, half-caste, who had lived in London for twelve years, had been passing such water for nine months. On examination of the water made at 2 P. M. it solidified, looking in ten minutes like blanc-mange. It was very feebly acid, contained fibrin, albumen, blood-globules, and fat; specific gravity = 1015. 1000 grs. of this urine gave—

44·42 grs. total solid residue.

8·01 grs. total ash.

14·03 grs. albumen.

8·37 grs. fat.

13·26 grs. urea and extractive matter.

·75 grs. loss.

955·58 grs. water.

In order to watch the variations produced by food and exercise in the appearance of the urine, every time the urine was made, for five days and nights, it was passed into bottles marked with the hour. From these observations, and more particularly from the third, fourth, and sixth days, it was evident that the fibrin and albumen appear in the urine when no fat is there, and that the albuminous urine occurs before food has been taken, and disappears during the night with perfect rest. Thus the fourth day, at 7h. 15m. A. M., on first getting up, the urine contained the slightest trace of albumen. The specific gravity = 1027; the precipitate by alcohol = 0·8 gr. per 1000 grs. urine.

At 9h. 50m. A. M., just *before* breakfast, the urine formed a solid coagulum, free from fatty matter, but contained a visible deposit of blood. Specific gravity, = 1015·6; the precipitate by alcohol = 14·1 grs. per 1000 grs. of urine.

At 11 A. M., the urine was chylous or white from fatty matter.

Further experiments on the influence of rest and motion in lessening or increasing the albumen in the urine previous to food, are then given.

On five different mornings, by rising early or late, and by collecting the precipitate from the urine by alcohol, the influence of rest and motion was determined. The author states that he could fix beforehand whether the urine should be albuminous or not, by directing the patient to get up, or to lie still.

The patient was bled, and the serum was opalescent, but did not clear with æther; the blood contained no excess of fat. 1000 parts of blood gave—

2·63 grs. fibrin.
159·3 grs. blood-globules.
78·1 grs. solids of serum.
240·03 grs. total residue.
759·97 grs. water.

The urine made the same day was examined at different hours; that made immediately before the bleeding was quite white, and that made an hour and a half afterwards was very milky also. Specific gravity = 1018. 1000 grs. of urine gave—

56·87 grs. total residue.
10·88 grs. total ash.
13·95 grs. albumen.
7·46 grs. fat.
24·06 grs. urea, &c.
·60 grs. loss.
943·13 grs. water.

The conclusions from these experiments are,—

1. That so-called chylous urine, besides fat, may contain albumen, fibrin, and healthy blood-globules.

2. That, although the fat passes off in the urine after food is taken, yet the albumen, fibrin, and blood-globules are thrown out before any food has been taken. During perfect rest the albumen ceases to be excreted; and it does not appear in quantity in the urine even after food is taken, provided there is perfect rest. A short time after rising early the urine may coagulate spontaneously, although no fat is present; and this may happen previous to food, when the urine is free from fat.

3. Though the urine made just before and a short time after bleeding was as milky as it usually was at that hour of the day, yet the serum of the blood was not milky: it did not contain a larger quantity of fat than healthy blood does.

The general results are,—

1. That the most important changes in the urine in this disease take place independently of the influence of digestion.

2. That the urine in one respect only resembles chyle, and that is in containing, after digestion, a large quantity of fat in a very fine state of division. The supposition that the disease consists in an accumulation of fat in the blood, which is thrown out by the kidneys, carrying with it albumen, fibrin, blood-globules and salts, is altogether disproved, both by actual analysis of the blood, and by the frequent occurrence of a jelly-like coagulum in the urine when no white fatty matter can be seen to be present.

3. The disease consists in some change in the kidney by which fibrin, albumen, blood-globules and salts are allowed to pass out, whenever the circulation through the kidneys is in-

creased; and if at the same time fat is present in the blood, it escapes also into the urine. That this change of structure is not visible to the naked eye on *post-mortem* examination, Dr. Prout long since demonstrated; and in a case of this disease which was in St. George's Hospital, and was examined at Plymouth, no disease of the kidney was observed. From the total absence of fibrinous casts of the tubes from the urine, it is not improbable that by the microscope a difference may be detected in the structure of the mammary processes, rather than in that of the cortical part of the kidneys.—[*Philosophical Transactions*, 1850. *Brit. and For. Med. Chir. Rev.*

Vesico-Vaginal Fistula.

We find in the Boston Medical and Surgical Journal an interesting article from Dr. Geo. Hayward, detailing a number of cases of Vesico-vaginal Fistula treated by operation, from which we make the following extract:

Before the discovery of the anæsthetic powers of ether, I found that the most difficult and painful part of the operation consisted in bringing the bladder down to the os externum. It is now done with comparative ease, and without causing the slightest suffering to the patient. I have administered the ether in the three last operations of this kind, and have been able to bring the bladder down, pare the edges of the fistula, introduce the ligatures and the catheter, and restore the bladder to its place, in twenty minutes; when in all the cases before, in which I did not use it, the same process required an hour, and during the most of that time the patient was suffering severely. Besides, the fistula is sometimes in such a situation, as when it is near the fundus of the bladder, that without this agent, or some similar one, it would be impossible to bring it in view.

The patient being thoroughly etherized, the bladder can be brought down by introducing a large sized bougie (one made of whalebone, highly polished, is to be preferred) into the urethra, to the very fundus of the bladder, and carrying the other end up to the pubis. In this way the fistula is readily brought in sight. Its edges can be pared with the scissors or a knife, though usually both these instruments are required; and this part of the operation is much facilitated by holding the edges by means of a double hook. In all the cases that I have examined, these edges are thick, hard, and usually of a white color. It is not difficult, therefore, to dissect up the outer covering from the mucous coat of the bladder to the distance of two

or three lines. The needles are then to be passed through the outer covering only, and as many stitches must be introduced as may be found necessary to bring the edges of the fistula in close contact.

Since my first operation, I have used a short needle with the eye near the point, made to fit on to a long handle. The instrument, when the two parts are together, looks not much unlike a tenaculum, though not so much curved, and considerably broader near the point.

As soon as the needle is passed through one side of the fistula, it is immediately seized by a forceps, the handle is withdrawn, and the needle is then carried through. It is to be then again fitted to the handle, and carried through to the other side in the same way. As many stitches as may be thought necessary to bring the parts into close contact can in this way be taken with great ease. One thread of each stitch is to be cut off; it is convenient to leave the other, as it enables the operator and patient to know when the ligatures have separated from the bladder.

A large sized female catheter is then to be introduced into the bladder, and secured there by means of a T bandage. The patient should be laid on her side, with the upper part of the body somewhat raised, so as to facilitate the flow of water through the catheter. This should be removed at least once in every twenty-four hours, as it is very likely to be obstructed by mucus, coagula of blood, and occasionally calculous concretions. In three days I think it safe to remove it altogether, but then it should be introduced at least once every three hours, for ten or twelve days more, so as to prevent any accumulation of urine in the bladder, and consequent strain on that organ.

The diet should consist entirely of liquid, mucilaginous food; such as an infusion of slippery elm, gum Arabic and water, flax-seed tea, arrow-root, and milk and water. This diet, in my opinion, should be continued till the ligatures come away.

The bowels should be opened by some mild laxative a few hours before the operation; but it is desirable that they should not be moved again till some days after.

I think it best for the patient to use the catheter once or twice a day for several weeks, and at any rate during that time to avoid making any strong efforts to expel the urine by the contraction of the bladder.

It may be proper to add, that I have never had any troublesome hemorrhage from the operation, nor any alarming symptoms after it. In some cases the pain has been severe for two or three days, and once or twice it has run down the limb, apparently in the course of the sciatic nerve. When performed

in the way that I have recommended, I believe it to be attended with very little if any danger, as the bladder is not subjected to any considerable degree of violence, nor any part injured to a great extent.

Fistula in Ano in a Child three years and a half old.

We have had several opportunities of pointing out how interesting a branch of surgery is that which takes cognizance of the ailments of children, and we ventured to state that the surgical diseases of childhood were important enough, and sufficiently distinct from those of adult life, to warrant the special attention of an experienced surgeon. The more we come in contact with the practice of hospitals, the more we are convinced that our idea is worthy of some attention. There are certain affections which generally come under the care of the surgeon, to which it is not exactly known whether children are liable or not; and others, with which they are often afflicted, but which are treated on general principles, modified of course according to circumstances, and the judgment of the practitioner. Could not certain rules be laid down for the treatment of infantile surgical ailments, as has been done with the medical affections of the same class of patients? These rules every practitioner carries about him in his head, but they are yet awaiting system and arrangement by a good and faithful hand.

We beg to put upon record a case of an infantile surgical affection, which, as far as we are able to judge, has not been frequently met with, and we hope thereby to be making an interesting addition to the facts mentioned above. The case is one of fistula in ano in a very young child, and runs as follows:

The patient is a stout, healthy-looking boy, three years and a half old, who was brought to Mr. Forster at the Surrey Dispensary, with a small opening on the left side of the anus, about one inch distant from the anal orifice. The child's parents are healthy, and previous to this opening forming, he had not suffered from any infantile disorder, though he has had measles and hooping-cough since. The mother stated that about fourteen months ago (the child being then only *twenty-eight months* old) she found, without any premonitory symptom, a hard lump forming in the left ischio rectal fossa, and a small pimple, which after swelling considerably, broke and discharged a tablespoonful of pus. There was no constitutional disturbance, and the abscess to all appearance healed shortly afterwards; but the scab which had formed fell off, more discharge came away,

and from that time until the present the opening has been sometimes closed, and at others discharging profusely. Several applications were advised, but the true nature of the affection seems scarcely to have been suspected, judging from the means employed for its cure.

Mr. Forster passed a small probe very readily along the track of a sinus, the walls of which were dense and callous, up into the cavity of the rectum. The sinus seemed to communicate freely with the intestine, as the probe entered the latter without any force being used. Upon introducing the finger into the anus, the end of the probe was easily felt and brought out, and Mr. Forster divided the sphincter and intermediate parts, thus completely freeing the probe.

The hæmorrhage was very slight; a piece of lint was put into the wound in the usual manner, and the child had two motions before sufficient suppuration to free the lint had taken place. Mr. Forster is not in the habit of using any applications after the lint; he merely takes care that the parts be kept clean and considers the practice of passing a piece of lint daily into the wound quite unnecessary, unless the latter be very sluggish. The wound healed very rapidly, as might be expected in a patient of such tender years.

We leave this case to the consideration of our readers; similar ones may perhaps have occurred here and there in practice, but we do not recollect having heard or read of fistula in ano in so young a child.—*London Lancet.*

Pruritus of the Vulva in Children. By M. VALLEZ.

It is not very uncommon to find young children complaining of distressing itching of the vulva and anus. This forces them to rub these parts, which leads sometimes to violent irritation in them. The affection is serious in consequence of its occasionally inducing bad habits which may continue to be practiced after the original cause of the itching has disappeared, namely, the presence of the genital regions of small ascarides, designated by Rudolphi by the name of *oxyures*.

Of this affection M. Vallez has observed two cases. In one, after having unsuccessfully used a local treatment for some days, he carefully examined the parts, and was astonished to find in the fossa navicularis, and around the fourchette, a quantity of small worms which, by their motions, produced the irritation. He immediately prescribed hip-baths, each containing in solution half a pound of saltpetre. After the third bath, the child was quite cured.

In the other case, the patient, a young girl, had for two years

endured great suffering from the irritation of the vulva. A great variety of means had been resorted to in order to relieve her, but with no effect. On examination, M. Vallez detected the presence of the *oxyures*. Two of the saltpetre hip-baths effected a cure.—[*Bul. Gén. de Thérap. Buffalo Med. Jour.*

M. Sedillot on Blue Suppuration.

The matter discharged from suppurating wounds, the urine, milk, perspiration, &c., have occasionally presented a blueish color, the cause of which remains unknown. M. Dumas supposed that it arose from the production of hydrocyanic acid, but this was soon shown to be an error. It was also thought that the color arose from the development of a peculiar fungus, the *agaricus nosocomiorum*; but Professor Fee, of Strasbourg, was unable to detect any such organic matter in the blue pus submitted by him to the microscope. Nine cases of blue suppuration have occurred in the practice of M. Sedillot. After various experiments and careful observation, it was discovered that the blue color did not arise from the pus, but from an accidental coloring matter which was developed and acted on the dressings. This coloring matter is probably formed by the serum of the blood; indeed this would appear to be certain, for the blue color was produced when all the other elements of pus were eliminated. The various experiments performed by the chemical professors at Strasbourg to ascertain the nature of this coloring matter, show that it is probably of vegetable origin. It does not arise from the formation of Prussian blue or phosphate of iron; it is soluble in water, and, at the same time, extremely stable, not being altered by sulphurous acid.

M. Sedillot inclines to think that blue perspiration, urine, &c., depend on the same cause, viz: some change in the serum of the blood.—[*Gaz. Med. de Paris.*

Miscellany.

To the Editors *pro tem.*

Justice to an absent friend—Professor DUGAS, Editor of this Journal—now in Europe, demands a notice of some “Remarks” upon his Case of Lithotrity, found in the April No. of the Western Journal of Medicine and Surgery, published at Louisville, Ky. Please re-publish the case with the “Remarks” from the accompanying Journal.

A Case of Urinary Calculus, attended with peculiar circumstances and treated by Lithotrity. By L. A. DUGAS, M. D., Prof. of Surgery in the Medical College of Georgia.

The following case is reported because of certain peculiar features presented during its progress. The patient, Mr. John L. B., of Hall county, Ga., is 30 years of age, was kindly directed to my care by Dr. Richard Banks, the distinguished surgeon of Gainesville, and arrived here on the 5th of February last. Having suffered from early childhood with phymosis and an almost complete closure of the orifice of the prepuce, (which he believes was congenital,) the difficulty of voiding his urine caused this to distend the prepuce into a considerable bag, to accumulate enormously in the bladder, to stagnate in the pelvis of the kidneys, and to induce very great impairment of the general health. The preputial orifice was so small as not to admit, without much difficulty, the introduction of a knitting needle; the urine was therefore never passed off in a jet, but the patient was subjected to all the inconvenience of a continual stillicidium; he had frequent and violent attacks of nephritic pains, attended with protracted chills, fevers, and the unusual concomitants of retention of urine. Yet it was not until the 20th year of his age that he sought professional aid and was circumcised by Dr. Banks. From that time his health improved rapidly; but he continued subject to occasional paroxysms of severe nephritic pains, which now became confined to the left side. These pains would extend down along the course of the ureter and continue one or more days, leaving him in a debilitated state, from which he would, however, soon recover. He is not aware of ever having passed gravel or anything like calculous matter, although his urine would sometimes present a very copious sediment.

This state of things continued until the middle of April last, when, although in good health and not having had any nephritic pain for about three months, he felt a calculus drop into his bladder. Attending to his usual avocations, he stepped out to urinate, did so without any difficulty whatever, and when in the act of buttoning up his garment, distinctly felt something fall into the bladder. He immediately mentioned the fact to a friend, and added that “it must be a stone, for its fall produced a sensation like that of a buck-shot allowed to drop into a bag.” A few hours afterwards, on again attempting to urinate, the stream was suddenly arrested by the engagement of the calculus

in the urethra—the sensation being so distinct that he instinctively carried his hand to the perineum in order to force it out—but in vain;—and the same difficulty has ever since attended his micturition. These details are given as establishing conclusively the facts that he did know the *precise moment* at which the stone came into the bladder, and that this occurred so late as about *three months after the last nephritic attack*. He has experienced no pain whatever about the kidney since that. In May he was sounded by Dr. Banks, who readily detected the stone.

On the arrival of Mr. B. here, I examined him, detected the calculus, found it to be small and determined to crush it as soon as circumstances would permit. The patient was directed to use dilating bougies, to remain quiet, to drink freely of slippery elm tea and super carbonate of soda, and to take a hip bath every night. In a week he was found to be sufficiently prepared, and (on the 12th of February) the operation was performed with Heurteloup's "*brise pierre*," as modified by Charriere. The bladder being filled with tepid water, the calculus was readily seized and crushed three times, without pain. A few fragments were passed off with the water and others during the night with the urine. On the following day, finding the patient very comfortable, without any symptoms of irritation, and very anxious to get home as soon as possible, I again introduced the instrument and crushed the remaining fragments, sufficiently to allow them all to be passed out during the night. He now expressed himself "entirely relieved, and feeling like a new man." The baths, etc., were continued and on the 16th February, I explored the bladder carefully, without being able to detect any vestige of the stone. The patient was therefore discharged.

The dimensions of the stone were accurately ascertained by the crushing instrument to be about one inch in length and half an inch in thickness. Professor Means having kindly subjected some of the fragments to analysis, informs me that they consisted of Oxalate of Lime. The stone was exceedingly hard, and tested to the uttermost the fine temper imparted to the metal by Charriere's unrivalled skill.

[*Southern Med. and Surg. Journal.*]

"REMARKS.—This certainly presents 'certain peculiar features,' both in anatomy and surgery, and we are utterly at a loss to understand some of them. The fault may be ours, but there can be no wrong in stating the difficulties.

"1st.—It is somewhat remarkable that a phymosis should have created so great a resisting power in the prepuce as to dilate even the ureters. This strikes us as a very remarkable peculiarity. The wonder is increased considerably when we find that notwithstanding the *ureters* were thus dilated so as to permit the passage of a stone of novel dimensions, the *urethra*, which should have synchronised liberally in the dilatation of the ureters, was so little inclined towards anything of the kind, that it stopped the stone which had fallen through the ureter! The extravagant dilatation of the ureter is inexplicable;

but, assuming the claim as a fact, the dilatoriness of the urethra is rather marvellous.

"2d.—The statement of the patient that he "heard something drop," and therefore knew the exact moment of the entrance of the calculus into the bladder, seems to have made a profound impression upon Professor Dugas, for he unhesitatingly gave credence to the statement. The patient may be excused for thinking that a calculus could fall from the ureter into the bladder, but we have some difficulties in our faith. The ureters enter the *bas fond* of the bladder, very obliquely, and a stone would have to fall *up* in falling from the ureter into the bladder. And then when we remember the pathological truths of Mr. Aldridge, which seem to show that the oxalate of lime is not secreted in the kidneys, when we remember that there is no kind of evidence that the ureters in this case were dilated even in the slightest degree, and that the passage of a mulberry calculus through the ureter would have made a man feel a multitude of other things besides the falling of the calculus, we must remember that we have before us what may be called the difficulties of faith.

"3d.—We feel some difficulty about the dimensions of the calculus. We have seen between two and three hundred specimens of calculi, and have heard from various other collections, and we have neither seen nor heard of any calculus, except this one in Georgia, that was just one inch in length, and a half inch in thickness. These dimensions are such a wide departure from that uniformity of proportion found in calculi, that we think there must be some mistake in Professor Dugas's measurements. There must be a want of accuracy. Did it not strike the Professor that the growth of his specimen was altogether too rapid for a case of oxalate of lime calculus? There seems to us a wonderful celerity in every branch of this case.

"4th.—The calculus in this case was 'oxalate of lime,' and the stone was crushed with Heurteloup's '*brise pierre*,' at two sittings, on two consecutive days, and the fragments were allowed to be passed off during the night. This is certainly the most remarkable achievement yet effected by Heurteloup's instrument. It is enough to excite the envy of Civiale, and put an end to the lateral operation. If a calculus of oxalate of lime, one inch long, and a half inch thick, can be utterly crushed in two sittings, in two successive days, so that no vestige of it is left, what apology can there be for cutting instruments for lithotomy? We have seen various efforts with Heurteloup's instrument, and have been sometimes surprised with the result, but this success in breaking down, in two sittings, a stone of oxalate of lime, of the size of the one recorded by Professor Dugas, certainly takes the lead of all achievements we know of in lithotritry. We have seen vesicle stones of oxalate of lime removed by the lateral operation after lithotritry had failed, and in which the most persistent efforts with the drill for many sittings had failed to make any more impression than if it had been used on a piece of Syenite. But if the improved apparatus of Heurteloup can break up at two sittings, a mass of oxalate of lime, and remove it entirely in two days, lithotritry is making

rapid strides, and M. Roux is an accredited prophet, when he says: 'lithotritry has assumed her function, and no surgeon hereafter will attain sufficient experience to reach the highest degree of adroitness in lithotomy.'

"We suppose these new claims of lithotritry will come before the American Medical Association, and if they receive the endorsement of that body, we may expect to see renewed evidences of the envy felt by European surgeons for the rising reputation of American Surgery, and we shall hear them again denouncing American surgeons for a proneness to exaggeration. B."

[*Western Journ. of Med. and Surg.*

Were the Reporter of the case here present, he probably would leave it to the Profession of this country, without suggestion, to give their condemnation to a criticism so manifestly dictated by a censorious, unjust, and ungenerous spirit; but the writer cannot withhold this passing notice.

The dilatation of the ureter is evidently referred to by Professor Dugas, as a state necessarily to be inferred, in order to account for the *fact*—that a calculus, which in February measured "about one inch in length and half an inch in thickness," did pass into the bladder, only ten months before. Is not the evidence given by the patient—fairly presuming that to be true—and detailed by the reporter, sufficient to establish the fact, that a calculus did pass from the ureter into the bladder, on the occasion indicated by the patient? About the middle of April, at a well-recollected occasion—viz., at the end of a micturition—the patient represents that he distinctly felt something fall into the bladder: before the time of this marked sensation, he was subject to occasional paroxysms of severe nephritic pains; since then these have ceased, and instead of them the patient experiences sudden interruptions to the stream of urine during micturition, which he never did before said occasion. This critic can find no evidence of dilatation of the ureter, under the circumstances of this extraordinary case—his ideas cannot travel out of the usual course—as violent pain attending the passage of a calculus through the ureter; the better man, the Reporter justly infers this state to account for the confessedly unusual fact—the passage of a large mulberry calculus from the ureter into the bladder, without pain.

If the writer could sufficiently command his patience, he would teach this critic how much greater the probability, that the circumstances of this case should produce dilatation of the ureter rather than of the urethra. Suffice it to say—the *fact* that the stone was in the bladder for 10 months, and did not pass the urethra, is worth more

than all the exclamation marks and all the shallow wonderings of the critic. The attempt by the Remarker to hold the Reporter responsible for the terms in which the *patient* declared his sensations, is as unfair as his reference to the anatomy of the organs is puerile; the Reporter makes the legitimate use of the patient's facts, to infer that the patient knew the moment of the entrance of the stone into the bladder.

"Difficulty about the dimensions of the calculus." The writer has not seen three nor two hundred specimens of calculi; but it is not a little remarkable, that within a few days, he has seen one calculus, of the mulberry variety, extracted from a child nine years old, by Dr. Henry F. Campbell, Demonstrator in the Medical College of Georgia,* the longer and shorter diameters of which, although a little greater, were yet very nearly in the same *ratio* with those of the one in question. Those familiar with the philosophic character of Professor Dugas, will find the whole sentence describing the stone, to be strikingly characteristic of the man. He says—"The dimensions of the stone were accurately ascertained, by the crushing instrument, to be about one inch in length and half an inch in thickness." Yes, *accurately* ascertained, because, as is fairly presumable, the stone being caught at different times, during the exploration, between the jaws of the instrument, each measurement was *accurately* made, because plainly shewn by a graduated scale upon the moveable branch of the instrument; but as it was not within the positive knowledge of the Reporter, that he at any time caught the stone, by the very longest or very shortest of its diameters, he writes, "about an inch," &c. Even strangers, on the strength of this single point of intrinsic evidence, shall mark the Reporter as the exact and careful man. How shall this critic fare, when thus tried by strangers, on the internal evidence of his own Remarks?

The Reporter, with philosophic exactness, writes, "*about* one inch in length and half an inch in thickness." The critic represents him to say, "*just* one inch in length and a half inch in thickness." But, see here again: "The statement of the patient, that he 'heard something drop,' and therefore knew," &c. The quotation marks, embracing the words *heard something drop*, are those of the critic himself; yet no where can they be found in the reported case! The reporter writes that the patient "*felt* something fall into the bladder." The writer calls no man names, but the Record brands this critic—*false quotator*. In the light of that maxim settled by the learned legal

* See Case in the present No. of this Journal.

profession, as a part of the law of evidence—*falsus in uno, falsus in omnibus*—of what value becomes the flourish of the critic about his two or three hundred stones, and the cases he has seen of lithotrity amended by lithotomy !

But, further: He says, "We think there must be some mistake in Professor Dugas's measurements. There must be a want of accuracy." He thus charges a want of accuracy of measurement upon a man, who has in his hands an instrument capable of measuring with mathematical exactness; so that, if there is a mistake, as charged, it must be a wilful mistake—a mistake in spite of the figures and marks upon the graduated scale before his eyes; so that the record marks this critic, the *insinuator* of the charge of falsehood—that is, *if he knew* that Heurteloup's instrument has such scale upon it; if he did not know this, the charge on this count is not fully made out. But the simple record, without the aid of argument, fixes this charge upon the critic. Let the candid reader only answer to himself, what is the force of these words?—"There seems to us a wonderful celerity in every branch of this case." In what stronger terms could such insinuation be expressed than these—"If a calculus of oxalate of lime, one inch long and a half inch thick, can be utterly crushed, in two sittings and in two successive days," &c., and repeated again, "But if the improved apparatus of Heurteloup can break up at two sittings," &c. Indeed, this insinuation of the charge of falsehood, pervades the whole of these "Remarks."

Let not the critic be surprised at the manifestation, in these quarters, of indignation at these insinuations against the veracity of Professor Dugas; who, be the insinuator hereby certified, is known, in his place, as the *true gentleman*.

Pity that this critic had not yielded to the whisperings of his better spirit, which seems to have warned him against this doubtful labor, rather than to have wrought himself to full assurance, by the answer—"there can be no wrong!"

The writer might well feel satisfied with this vindication of his absent friend, from such an unaccountable attack from him who fills the critic's chair, in this Louisville Journal; but as it could not have been suggested by malignant feelings engendered by personal rivalry, seeing that the parties are widely separated and have no relations whatsoever, personal or professional, it must therefore be inferred that this criticism is a fair manifestation of the spirit and character of the Remarker. In this point of view, the Profession generally has an interest in this subject. Let this critic, therefore, come to the bar of

the Profession and answer. He who assumes the position of the critic, does not place himself beyond the reach of impeachment, but becomes responsible to the Profession for the manner in which he discharges its high duties. Nor shall he escape its censures, if he be found lacking in the qualities of the critic's character which that profession demands, or exhibiting those which it condemns. Whilst it demands an independence, which fears not to attack and expose error, even in high places and shadowed by authority, it also demands the manliness of truth—that stern integrity which fears also to misrepresent even an enemy. It demands of him who holds the Judge's seat—"Nothing extenuate nor set down aught in malice." It demands candor, fair dealing and justice—which is proof against all seduction. It demands of him intelligence to estimate justly, the value of the subjects upon which he passes judgment, and their bearing upon the general interests of the science or upon its special questions. Has this critic discharged himself aright of this last obligation, in seeking to discredit this case, so strikingly valuable in relation to that question of nice judgement, the choice between Lithotrity and Lithotomy, in specific cases?—to brand with the suggestion of falsehood, this most remarkable case of successful Lithotrity on the records of the Profession.

Does this critic of the Louisville Western Journal fulfil the requisitions of the Profession upon their critics? Let him await, at the bar, its just judgment

L. D. F.

Case of Quintuple Birth of Living Children. By Dr. SERLO.—Dr. Serlo, of Krossen, relates the following remarkable case. The mother, æt. 34, had had five favourable labours, and was now pregnant for the sixth time. During the last few weeks, she had become so large and cumbersome as to be obliged to keep her bed. Dr. Serlo saw her the day before her delivery, and found her abdomen enormously distended in every direction, and hard, and projecting much towards the right. The foetal movements were feeble. She was weak, and had a small, rapid pulse, with œdema of the thighs and legs. On examination the os was found partly open, and the membranes flaccid; but no part of the child could be felt. As the pains proved very inefficient, Dr. Serlo next day delivered her, by the forceps, of a small living child, and soon after of another, which presented by the feet. In like manner three others were successively delivered by the feet, the accoucher breaking the bag of waters in each which presented while he was in search of the placenta. Contraction of the uterus was produced after some minutes.

All the children were alive and crying, but the 2d died in three hours, the 4th in twelve, the 3d in seventeen, the 5th in twenty-five;

and the 1st, which had been delivered by the forceps, in nine days. The author supplies the weights and admeasurements of the children and the funes; but we are not aware of the exact relation which those of that part of Germany bear to our own.

LENGTH.				WEIGHT.	
Child.		Funis.		Child.	Placenta.
1st child.....	15 inches.....	24 inches.....	3½	civil pounds.....	28 oz.
2d “	12 “	11 “	2¼	“	14 “
3d “	13 “	15 “	3⅛	“	25 “
4th “	14 “	14 “	3	“	20 “
5th “	14 “	11 “	3	“	20 “

[*Med. Zeit.*, 1850. *Brit. and For. Med. Chir. Rev.*

On the Duration of Life among the Clergy. By Dr. SCHNEIDER.—In this paper Dr. Schneider furnishes an account of the ages of 794 of the clergy who have died within the bishoprick of Würzburg since 1824. They belonged to the Catholic religion, and may be compared with the 637, almost all Protestants, recorded by Casper in his work. In the diocese of Würzburg, including priests, professors and students, the mean annual number of this profession amounts to 1050; and between 1824–47, there have died 800. The ages of 794 persons are indicated, and are thus compared with Casper's numbers:

Ages.		Schneider.		Casper.	
From 21 to 30	.	45	.	21	.
“ 30 “ 40	.	66	.	33	.
“ 40 “ 50	.	57	.	39	.
“ 50 “ 60	.	102	.	95	.
“ 60 “ 70	.	188	.	191	.
“ 70 “ 70	.	217	.	188	.
“ 80 “ 90	.	111	.	62	.
“ 90 “ 97	.	8	.	8	.
		794		637	

Casper, speaking of the high age which the priestly order attains, refers in explanation to the regularity and sobriety of their lives, the absence of excess of mental or bodily stimulus and efforts, freedom from anxiety, and the wholesome alternation of moderate mental activity with corporeal exertion in the open air. Most of these conditions apply likewise to the Catholic priesthood, and their *celibacy* constitutes their only peculiarity; but this, contrary to what is observed in other positions of life, does not shorten their lives, inasmuch as so large a proportion as 217 out of 794 lived to between 70 and 80. On the contrary, they live longer than the Protestant clergy; for while, according to Casper, the mean duration of life with them is 65, and therefore higher than any other class, that of the Catholics of Würzburg is 70½. The Protestant clergy have, owing to their families, more cares and anxieties, without better incomes in proportion.

[*Casper's Wochenschrift*, 1850. *Ibid.*

Case of Small-Pox occurring a third time after Vaccination, when it proved fatal.—Dr. Webster, after alluding to the fact, that whooping-cough, measles, and scarlatina, generally occur only once during the lifetime of an individual, exceptions, nevertheless, to the above rule, as well in these complaints as in small-pox, have been recorded by authors. Three well-marked examples, of the recurrence of small-pox, met with in the same family, are related, one of which terminated fatally. The case especially referred to by Dr. Webster was that of H. A. N——, who had been vaccinated satisfactorily in 1827, when three months old. Notwithstanding this circumstance, he became attacked by small-pox in 1833, along with an elder brother, who had been likewise vaccinated. Both patients recovered, and nothing more was thought of the matter till 1838, when the two lads were again attacked by variola, along with another—that is, a third—brother, likewise regularly vaccinated. However, all three got quite well in due time. Subsequently, Mr. H. N. N——, whose case is now just mentioned, went to India in the Company's service, where he was seized, in April last, with the usual and well-marked symptoms of small-pox, which soon became confluent, and proved fatal at Dharwarinth, on the 13th of that month; this making the third time this gentleman had been attacked by variola, although previously vaccinated.—[*London Lancet*.

Impermeable applications in Inflammations of the Viscera.—We find in the *Union Médicale*, (April, 1851,) an article by M. R. Latour, in which he recommends very highly impermeable applications in visceral inflammations. He states that, having found these applications so unquestionably beneficial in rheumatism, gout, &c., that he was induced to apply it in inflammations of the viscera, and in peritonitis. In the latter diseases the applications have always exercised a beneficial influence, and relieve the patients in from one to three days. M. Latour explains the *modus operandi* of these agents in this way. Heat is one of the essential elements of inflammation, and under its influence the blood-vessels dilate, which admits of an increase in the quantity of circulating fluid, causing redness. The increase in the amount of blood also produces a distention of the vessels and an effusion through their coats, which causes swelling. The writer thinks that the development of heat, in part, is dependent upon the presence of air. If, therefore, the air can be cut off from the diseased tissue, heat and the other characteristics of inflammation cannot be developed. It is a powerful antiphlogistic. He states that he has combatted successfully with these agents many cases of ovaritis and peritonitis, one of which we will relate.

A girl, 22 years of age, was taken with a chill that lasted an hour, and was succeeded by high fever, pain in the head and abdomen.

The abdominal pain continued to increase and extend until the whole abdomen became extremely painful to the touch. The respiration was hurried, the abdomen swollen; there was high fever, and finally vomiting set in. This then was a veritable case of peritonitis. Not a drop of blood was drawn, but the abdomen was simply covered with a coating of collodion. The vomiting was immediately arrested, and in a few hours the anxiety passed away, and the abdominal pain was sensibly diminished. In short, in less than a day the skin regained its natural freshness, and the pulsations resumed their normal frequency.

Treatment of Ascites in Children.—The Journal des Connaissances, March, 1851, contains the details of the treatment of this disease in children, as practiced by M. Trousseau. He administers fractional doses of calomel and applies poultices of powdered conium maculatum to the abdomen. The dose of calomel for each day is half a grain, divided into six parts. In a few days salivation supervenes, and the calomel is stopped. Diarrhœa is sometimes induced by the calomel. When salivation comes on the hemlock poultices are had recourse to. On account of the cost of the article, the poultices are not made entirely of hemlock. M. Trousseau recommends them to be made in the following way:—Take two table-spoonfuls of the powdered hemlock, and mix it with a sufficient quantity of thick flax-seed tea to form a consistence nearly equal to that of paste. This is spread upon the surface of a flax-seed poultice, and applied to the abdomen. M. Trousseau has long since recommended this application in phthisis pulmonalis. He has found the calomel and hemlock very serviceable even in ascites dependent upon an organic lesion, as abdominal tubercles.

Tannin in affections of the Eye.—The Journal des Connaissances relates the treatment of ocular diseases with tannin, by M. Hairon. Tannin has been advised in catarrhal ophthalmia in the form of a weak solution. M. Hairon asserts that this solution is inert. He employs it in a concentrated solution, one part of tannin to three parts of distilled water. This solution has been found serviceable in gonorrhœa, both acute and chronic, in granulations of the eye-lids, in vascular and ulcerated cornitis, and especially in pannus. Tannin differs from most of the astringents, in the fact that it does not cause pain, that it does not act as a caustic, nor does it leave any durable incrustations. It is simply a styptic, and to this property is attributable its efficacy in diseases of the eye.

Hysteria.—A very singular scene recently occurred in one of the national manufactories of Tobacco at Lyons. About sixty women were at work in one of the rooms, when one of the number in consequence of a violent paroxysm of passion, went into hysterical convulsions, many of her companions rushed to her assistance, but one after another were attacked with the same nervous symptoms until the number attacked amounted to twenty. The other women were ordered out of the room or others would in all probability have been similarly affected. Boerhaave, in the commencement of the last century, upon a similar occasion had some heated irons brought into the room and threatened to cauterize the first who should fall into hysterical convulsions. This threat produced the desired effect and the singular contagion immediately ceased.—*Bul. Gen. de Therap, Feb. 1851.*

Poisoning by Strychnine cured by inhalations of Chloroform—The Journal des Connaissances, March, 1851, extracts a case from the Medical Gazette of poisoning by strychnine, that was cured by inhalations of chloroform. All the remedies recommended in such cases were tried without success. Chloroform was then given in inhalation, and an immediate amelioration and a subsequent cure was obtained.

Medical Statistics of Paris.—The number of Physicians in Paris has diminished this year, but not in as great a proportion as in 1848. There are now in Paris 1351 Physicians; in 1849 there were 1389, making a diminution of 38. Among the 1389 Physicians of 1849, 68 are dead and 86 have left Paris; of these 86, 12 have gone to California. Among the 1351 Physicians that compose the list of 1851, there are 113 new names; there were 114 new names in the list of 1848.

There are now 178 officers of health, which are more than the number of Medical men during the reign of Louis XIV. The number of Druggists is 381, and the Sages-femmes number 380. The number of Graduates slightly increased in 1851—there were then 236, whilst in 1849 there were 230.—*Jour. des Connaissance, Jan. 1851.*

At the late meeting of the American Medical Association, the following gentlemen were appointed Chairmen of the several Special Committees. The Committees are to consist of two other members, to be selected by the Chairman.

1st. Dr. D. F. Condie, of Philadelphia, chairman to the committee on the causes of the Tubercular Diathesis.

2d. Dr. S. H. Dickson, of Charleston, S. C., on the blending and conversion of the Typhes of Fever.

3d. Dr. James Jones, of New Orleans, on the mutual relations of Yellow and Bilious Remittent Fever.

4th. Dr. Jno. B. Johnson, of St. Louis, Mo., on Epidemic Erysipelas.

5th. Dr. Charles D. Meigs, of Philadelphia, Acute and Chronic diseases of the Neck of the Uterus.

6th. Dr. J. P. Jervey, of Charleston, S. C., on Dengue.

7th. Dr. Daniel Drake, of Cincinnati, Milk Sickness—so called.

8th. Dr. Lopes, of Mobile, Ala., Epidemic prevalence of Tetanus.

9th. Dr. Geo. B. Wood, of Philadelphia, on diseases of Parasitic Origin.

10th. Dr. R. D. Arnold, of Savannah, Geo., on the Physiological Peculiarities, and diseases of Negroes.

11th. Dr. Horatio Adams, of Waltham, Mass., on the action of Water on lead pipes, and the diseases which proceed from it.

12th. Dr. Jos. Carson, of Philadelphia, on the Alkaloids, which may be substituted for quinia.

13th. Dr. Geo. Hayward, of Boston, Mass., on the Permanent Cure of Reducible Hernia.

14th. Dr. S. D. Gross, of Louisville, Ky., on results of Surgical Operations for the relief of Malignant Diseases.

15th. Dr. James R. Wood, of New York, Statistics of the Operation for the Removal of Stone in the Bladder.

16th. Dr. Charles A. Pope, of St. Louis, Missouri, Water, its topical uses in Surgery.

17th. Dr. Alex. H. Stevens, of New York, Sanitary principles applicable to the Construction of Dwellings.

18th. Dr. Porcher, of Charleston, S. C., Toxicological and Medicinal Properties of our Cryptogamic Plants.

19th. Dr. G. Emerson, of Philadelphia, Agency of the Refrigeration produced through upward Radiation of Heat, as an exciting cause of disease.

20th. Dr. Worthington Hooker, of Connecticut, on the Epidemics of New England and New York.

21st. Dr. John L. Atlee, of Lancaster, Penn., on the Epidemics of New Jersey, Pennsylvania, Delaware and Maryland.

22d. Dr. Robert W. Haxall of Richmond, Va., on the Epidemics of Virginia and North Carolina.

23d. Dr. Wm. M. Boling, of Montgomery, Ala., on the Epidemics of South Carolina, Georgia, Florida and Alabama.

24th. Dr. Ed. H. Barton, of Louisiana, on the Epidemics of Mississippi, Louisiana, Texas and Arkansas.

25th. Dr. Sutton, of Georgetown, Ky., on the Epidemics of Tennessee and Kentucky.

26th. Dr. Thos. Reyburn, of Missouri, on the Epidemics of Missouri, Illinois, Iowa and Wisconsin.

27th. Dr. Geo. Mendenhall, of Ohio, on the Epidemics of Ohio, Indiana and Michigan.

The following gentlemen were appointed on the Committee for Volunteer Communications, viz :—Drs. Geo. Hayward, J. B. J.

Jackson, D. H. Storer, and Jacob Bigelow, of Boston, and Dr. Usher Parsons, of Providence, R. I.

Signed in behalf of the committee,

GEO. B. WOOD, Chairman.

Charleston, Friday, May 9th, 1851.

BIBLIOGRAPHICAL.

The Pharmacopœia of the United States of America. By authority of the National Medical Convention held at Washington, A. D., 1850. Philadelphia, Lippincott, Grambo & Co. 1851. 1 vol., 8vo. pp. 317.

We have received from the publishers the new issue of our National Pharmacopœia. As our readers are probably aware this work is the result of the labors of a Convention of Physicians and Pharmacutists from all sections of the United States, which met in Washington City in May, 1850. The present edition is doubtless thoroughly accommodated to the improvements which the medical sciences have undergone during the last decennial period. The plan of the work is that adopted at the revision of 1840. "The changes have been altogether in the individual contents of the Pharmacopœia. A few names have been altered; definitions and references have been modified in numerous instances; some medicines have been transferred from one of the two catalogues of the *Materia Medica* to the other; new medicines and preparations have been introduced; and many of the processes have been amended, replaced by others, or altogether omitted. The section of fluid extracts is quite new."

The present edition is handsomely printed, but the price is so high as to restrict its circulation, which is certainly unfortunate, as it is a work that should be in the hands of every practitioner.

Operative Surgery. By FREDERICK C. SKEY, F. R. S. pp. 661. Octavo, 1 vol. Philadelphia: Blanchard & Lea. 1851.

In a monthly journal, like the present, an extended review of works submitted to our notice is not appropriate, and hardly to be expected. It is therefore our habit to give such works a careful examination, and to express such an opinion as their character and merits, in our humble judgement, may entitle them to.

The work before us is one which—although we cannot recommend as containing *all* required in operative surgery—is still of the most useful character. The subjects are treated in a most definite, we might say, graphic manner; the Surgeon and Anatomist have combined in all his directions. That vagueness and indefiniteness so

common and so embarrassing in the instructions of many teachers are not found in those of Mr. Skey. His object seems to have been to produce a practical work which is reliable and which would embody in a few words, all the principles required in emergency. The work is practical, and certainly reliable; but there are many subjects in which he has sacrificed clearness and completeness of description to a terse and compendious design. While we would regret these defects in a work, otherwise unexceptionable, we cannot in justice withhold our recommendation of it as one of great value to the student and practitioner in Surgery, an art, the library of which cannot be comprised in one, but must be in many treatises.

H. F. C.

A Treatise on Fractures and Dislocations of the Joints. By Sir ASTLEY COOPER, Bart., F. R. S. Edited by B. COOPER, F. R. S., &c., &c., with additional observations, and a memoir of the author. A new and enlarged American Edition. pp. 496. Blanchard & Lea, Philadelphia. 1851.

In presenting this standard work to our readers we find no difficulty in giving an opinion. Its reputation is too long and too well established to require any commendation of ours. In this enlarged edition, it will be found that the interest and value of the original work has been much enhanced, while the style and execution do credit to its enterprising publishers.

H. F. C.

Nashville Journal of Medicine and Surgery.—We have received the first numbers of this new bi-monthly journal, published at Nashville, Tenn., and edited by W. K. Bowling, M. D., Professor of the Institutes and Practice of Medicine in the Medical Department of the University of Nashville. From the specimen before us, we think it bids fair to become a valuable laborer in the field of medical science.

We have received a communication from Dr. H. A. Ramsay, just as this number was going to press, requesting a suspension of public opinion in relation to charges made against him at the late meeting of the American Medical Association, which he states are "in a train of investigation, and will be given to the medical public in due season."

SOUTHERN MEDICAL AND SURGICAL JOURNAL.

Vol. 7.]

NEW SERIES.—AUGUST, 1851.

[No. 8.]

PART FIRST.

Original Communications.

ARTICLE XXIV.

Physiology of the Pneumogastric Nerve and Spinal Accessory of Willis. By JURIAH HARRISS, M. D., of Augusta, Ga.

The varied distributions of the pneumogastric, and its intimate connections with many of the vital functions of our organic system, place it among the most important and interesting nerves in the economy. It is, perhaps, to be equaled in interest only by the great sympathetic. The immediate relation that it has with many of the vital functions of the body, renders it imperative upon every one to study, with particular attention, its anatomy and physiology, who may wish to understand, perhaps, the most beautiful branch of our science,—the operation of the economy in health. It is rather a trite saying, but one that cannot be too often repeated, that it is essential to understand the functions of the system in health, else it will be impossible to interpret the phenomena that are presented to us in disease. Understand the healthy man, and then undertake to explain and medicate the morbid conditions of the economy. Probably much that we shall say, will not be entirely new to some, yet we feel assured that the subject will prove interesting to many: first, on account of its importance, and secondly, because the nervous system is too generally neglected in this country. The spinal accessory is so intimately connected, anatomically and physiologically, with the pneumogastric, that it will be impossible to understand one properly, without study-

ing the other. It would be impracticable in the limited space that I have, and would probably prove uninteresting, for me to trace out the relations and functions of all the branches of the accessory and pneumogastric nerves. I shall therefore confine myself to the most important branches and their functions.

The pneumogastric arises by eight or ten filaments, from the restiform bodies, which are but continuations of the posterior spinal columns. These fibres soon unite and form one nerve, which enters a common sheath of the dura mater, with the spinal accessory to pass out of the cranium through the foramen lacerum.* At the point of exit from the cranium the pneumogastric presents an enlargement resembling a ganglion, (ganglion of the pneumogastric). Some anatomists do not believe this to be a ganglion. This ganglion should not be confounded with the plexiform enlargement, (renflement,) that exists below, and after the nerve has passed out of the cranial cavity. It is at this gangliform plexus that the spinal accessory gives distinct filaments to the pneumogastric. From this point, also, the pneumogastric gives branches to the glossopharyngeal, facial and carotidean branch of the superior cervical ganglion.

The spinal accessory nerve arises from the lateral columns of the spinal cord, passes up into the cranium, anastomosis with the two superior cervical nerves,† and sends a few filaments to the pneumogastric, in company with which nerve it passes out of the cranium. The accessory then passes behind the pneumogastric and divides into two branches—the external and internal. The external branch passes to the sterno-cleido-mastoid and trapezius muscles, and the internal unites to form a common trunk with the pneumogastric. In the cervical region the pneumogastric and the internal branch of the spinal accessory give off, 1st, a pharyngeal branch, and 2d, the superior laryngeal nerve. In the thoracic region the pneumogastric and spinal accessory are intimately connected, and give off, 1st, the inferior laryngeal or recurrent nerves; 2d, the

* Quain and Wilson say they do not pass out in a common sheath. Cruveilhier, Longet, &c., affirm they do.

† Cruveilhier says, this anastomosis with cervical nerves is not constant.

cardiac branches; 3d, the pulmonary, and 4th, the œsophageal nerves.

Thus, the pneumogastric unites with three nerves of motion: 1st, spinal accessory; 2d, facial; 3d, hypoglossal; and with one of sensation, the glosso-pharyngeal. It also unites with the great sympathetic, to form the pharyngeal, inter-carotidean, lingual, pulmonary, and solar or epigastric plexuses.* It will be seen, then, that these two nerves, after they have united in the neck, are distributed with the sympathetic to the principal organs of the functions of digestion, circulation and respiration—viz., stomach, liver, heart and lungs. This pair of nerves is also distributed to the larynx, trachea, pharynx and œsophagus. They have never been traced to the spleen or kidneys.

Willis described the spinal accessory as a distinct nerve, and believed that its use was to assist in respiration. Bischoff, however, looked upon it and the pneumogastric in the light of a spinal pair of nerves. He supposed the pneumogastric to be the sensitive, and the spinal accessory the motor branch. Bischoff instituted experiments upon animals to prove that these two nerves were at least physiologically similar to the spinal nerves. All of his experiments failed, except one, which was upon a bird. He cut the spinal accessory, and the bird lost its voice; from which he concluded that this nerve was for motion, and the pneumogastric for sensation. The experiments of M.M. Gaerres, Arnold, Sarpa, and Longet,† go apparently to substantiate the opinion advanced by Bischoff.

These experimenters affirm that the origin, union and distribution of these nerves resembled very much the spinal pairs. M. Longet gives the results of his experiments, which would seem to corroborate the opinion of Bischoff. He says, "When in my experiments upon dogs and horses, I applied galvanism to the spinal accessory, very evident contractions were observed in the larynx, pharynx, and upper part of the œsophagus. No motion was perceptible in the lower part of this tube, nor in the stomach. When galvanism was applied to the pneumogastric, no muscular contractions were perceived." M. Longet concludes, as the pneumogastric arises as do the spinal sensitive nerves, from the posterior columns, or *restiform*

* Longet, Anat. and Phys., p. 261, vol. 2

† Longet, Anat. and Phys.

bodies, as it is provided with a ganglion, like these nerves, and finally, as galvanism applied to this nerve does not cause muscular contractions in the organs to which it is distributed, that it is simply a nerve of sensation. And upon the other hand, as the spinal accessory arises from the lateral columns, and as a galvanic stimulus applied to it will induce muscular contractions, he believes that it is a nerve of motion, and that these two nerves stand in the same relation to each other as do the spinal pairs—that is, that the pneumogastric furnishes sensibility to the mucous membrane of the larynx, trachea, bronchia, pharynx, œsophagus and stomach, and perhaps influences the secretion of bile. And the accessory of Willis supplies the muscles of the larynx, the contractile tissue of the trachea and bronchia, the constrictor muscles of the pharynx, and finally the sterno-cleido-mastoid and trapezius muscles. M. Bernard thinks that Bischoff and Longet generalized too much from the experiment of cutting the spinal accessory and destroying the voice. His experiments led him to the conclusion that the motor power of the spinal accessory stopped at the larynx, and did not extend to all the organs to which the pneumogastric was distributed. The spinal accessory is not sensitive at its origin, but soon becomes so by anastomosing with sensitive nerves.

External branch of the Spinal Accessory.—This nerve is distributed to, and controls the movements of the sterno-cleido-mastoid and trapezius muscles. According to Sir C. Bell, these muscles are influenced in their movements in respiration by the external branch of the spinal accessory, but their contractions in the movements of the head are caused by the cervical nerves. Bell exposed this branch in an ass, and after accelerating the respiration he cut the nerve. The sterno-cleido-mastoid and trapezius lost all motion in respiration, but retained their power of contraction in the movements of the head. Bernard states that this nerve is not one of respiration, save through the will, but is simply a nerve of motion. He thinks that the spinal accessory is a spinal nerve, presiding over the voice, and a voluntary one, as the voice is produced by the exercise of the will. The external branch assists in the production of vocal sounds, by acting

in concert with the internal branch, and arresting respiration. It is essential to the production of voice, that the glottis should be contracted and respiration stopped. If, then, the external branch be cut, the respiration cannot be arrested, and voice cannot be produced. A harsh, raucous sound may be heard, but not a clear, distinct voice. If the internal branch, be cut at the same time, a similar noise may be produced. The two spinal nerves of Willis may be cut, and yet the pneumogastrics will carry on respiration, circulation and digestion. An animal thus experimented upon will live, as he has only lost the power of producing vocal sounds.

Superior Laryngeal Nerves.—This is a mixed nerve, composed of fibres from the pneumogastric and spinal accessory. It is distributed to the crico-thyroidean muscles and the laryngeal mucous membrane, and is principally a sensitive nerve. If it be cut, says Longet, just above the thyroid cartilage the voice is not modified, but if it is cut before it divides into the external and internal branches, the voice always becomes harsh. This alteration in the voice is caused by cutting the external branch of the superior laryngeal nerve, and there by paralyzing the crico-thyroidean muscles. The internal branch does not influence the voice. The same result will be obtained by cutting the external branch, as by severing the superior laryngeal itself. The mechanism in the production of aphony is very different in this case from that that causes the same effect when the external branch of the spinal accessory is cut. In one, the crico-thyroidean muscles are paralyzed, and the vocal cords cannot be put upon the stretch; in the other, the respiration cannot be stopped, in consequence of the paralysis of the sternocleido-mastoid and trapezius muscles. When the superior laryngeal is cut, the sensibility of the glottis, epiglottis and larynx, up to the base of the tongue, is destroyed. The respiration continues, though there is more or less complete aphony.

Inferior Laryngeal or Recurrent Nerves.—These nerves exercise a very decided influence upon the voice and respiration. Their influence upon these functions has been seen in tumours, and other pathological changes that implicated these

nerves. Their agency in the production of the voice and respiration is easily demonstrated by cutting them.

Galen, two thousand years ago, cut these nerves in animals, and invariably destroyed the power of producing voice; hence he styled them vocal nerves. Their agency in producing vocal phenomena has been observed by all succeeding experimenters, but their influence upon respiration was reserved for Lagallois to discover. M. Magendie thinks that cutting the recurrent nerves does not modify the voice, or, at least, such has not been the result of his experiments. Sedillot says that, sometimes aphony is induced, and at others it is not, but he does not give any explanation as to the cause of this diversity in the results. Longet admits the assertion of Sedillot, in regard to the divers effects observed. He, however, offers the same explanation as did Lagallois, to explain the different results obtained in connection with the respiration, when the recurrent nerves were cut. The results are not in every instance the same, in consequence of the difference in the form of the glottis in the old and young animal. Longet has kept animals, after cutting these nerves, for five or six weeks without a reproduction of voice, and in others he has not been able to abolish this function at all. The glottis presents an anterior or vocal part, bordered by the vocal cords, and a posterior or respiratory part. The anterior part is proportionally more developed as the age approaches the period of birth. In the young, the crico-thyroidean muscles contract, make tense the vocal cords, and approximate the cartilages of the same name, to produce sound; but in the old, the posterior or respiratory part of the glottis is very large, and cannot be narrowed, as the arytenoidean muscles are under the control of the recurrent nerves, and are consequently paralyzed. In the young, it is not necessary for the posterior part of the glottis to be narrowed, as it is much smaller than in the older animal.

The production of vocal phenomena is then under the influence of three nervous branches: 1st, the external branch of the accessory of Willis; 2d, the superior laryngeal; and 3d, the inferior laryngeal or recurrent nerves. The concurrence of all these nerves is necessary to the production of a clear and distinct voice. In order that articulation or a distinct vocal

sound may be produced, it is necessary that the respiration should be arrested and the vocal cords be put upon the stretch, which is done by the concurrence of the external branch of the spinal accessory and the inferior laryngeal nerves. In addition to this, the arytenoidean muscles must contract to approximate the cartilages of this name, which will narrow the posterior or respiratory part of the glottis, and enlarge the anterior or vocal part of the same organ. These muscles are supplied by the inferior laryngeal nerves.

[To be continued.]

ARTICLE XXV.

A Case of Senile Gangrene of the Inferior Extremities. By ROBERT CAMPBELL, M. D., Assistant Demonstrator in the Medical College of Georgia.

The rareness in our region of this disease, will perhaps invest the subjoined case with some interest:—

E. C., aged about 50 years; of spare habit and florid complexion; carriage maker by trade—was brought from Anderson C. H., South Carolina, and admitted into the Augusta Hospital on the 11th May last. I saw him at 4 P. M., in consultation with Prof. Jos. A. Eve, the attending physician. Found the whole of the right and the anterior third and heel of the left foot, perfectly black, without sensation or sensibility, with vesications studding their upper surface, and yielding the peculiar offensive "*mortification odour*:" indeed these parts were in a completely sphacelated condition. He complained, when the extremities were handled, of pain only about the ankle joints; the legs were œdematous almost to the knee joints—pitting on pressure. Extremities cold and dry; pulse small, unresisting, and 150 in frequency; tongue coated with thick, yellow fur; appetite deficient; mental faculties somewhat impaired.

All we could gather concerning the history of the case was, that a short time prior to this he had gotten his feet very wet in a rain, having to travel some distance in the rail-road car, and not being able to change his boots, suffered extremely with swelling and a burning sensation in the feet. He had been a

man of intemperate habits, and had, at a former time, lost several of the toes of the left foot from a previous attack of mortification.

Diagnosis.—Mortification by ossification of the arteries.

Prognosis.—Of course, very unfavorable.

Treatment.—Amputation presented to our minds the only possible hope of staying the dilapidation so fast ensuing. Hence the immediate removal of the right (the worst) leg was determined upon; and at 6 o'clock I amputated the leg by the circular operation at about the junction of the upper with the middle third—the patient under chloroform. The arteries opposed a considerable resistance to the knife, the passage of which conveyed an unusual, grating sensation, as from collision with petrified, rather than ossified vessels. Their ligation was effected with more than usual facility, inasmuch as they protruded beyond the contracted surrounding tissues, instead of retracting within them.

We left the patient comfortable, having lost very little blood, and experienced no pain during the operation; nor would he be convinced of its execution until his attention was directed to the absence of the limb.

Prescription.—Port Wine, . . . f℥ij.

Quinine, . . . 5 grs.

To be repeated every six hours. Diet—Chicken soup, freely administered.

12th. Patient under the influence of quinine; complains of some pain in the stump; extremities still cold; pulse rather more resisting, and only 100 in frequency; will take but little nourishment.

Prescription.—Continue wine every three hours, and quinine three times a-day, in doses as before. Enemata to relieve the bowels; and should pain continue, laudanum 30 gtt., repeated pro-re-nata. Left foot treated with cloths saturated with the chloride of soda.

13th. Much weaker; extremities of still lower temperature; pulse very feeble and intermittent; delirium complete.

14th. Died at 7 A.M.

It would have been an interesting investigation to have ascertained to what extent the arterial system had been subjected

to this ossific deposition. This I intended to have accomplished, had not my own indisposition at the time prevented. But the perfect character of the ossification at the point examined, the slight effect of the stimulus, and the apparent absence of any recuperative energy in the constitution of the patient, notwithstanding so large a proportion of the contaminating mass had been removed, and without the loss of blood or the shock of pain—are circumstances which seem to indicate the circulatory apparatus to have been much embarrassed in its functions, from the extensive pervasion of the disease.

ARTICLE XXVI.

The Removal of an ounce and a half of Calculous matter from the Bladder, by Lithotomy. By HENRY F. CAMPBELL, M.D., Demonstrator of Anatomy in the Medical College of Georgia.

Mr. B., a young man aged about 20 years, had from early childhood manifested symptoms of calculous disease. Some months ago, he applied for relief to our friend Professor Miller, of Rome, who kindly referred the case to us. On sounding, a stone was readily detected; but on account of the irritability of the bladder, together with the patient's general low state of health at the time, we were unwilling to distress him with often repeated examinations. The fact being well ascertained that a stone, which was very movable, existed, we proceeded to prepare him for the operation of lithotomy—lithotrity being impracticable, on account of the contracted state of the urethra and the enfeebled condition of the patient. The patient was put under the influence of chloroform, and assisted by my friends, Drs. Miller and Word, we operated by the bilateral method. On cutting into the bladder, we found that, instead of one, there were three calculi, each of which were so large that when grasped in the forceps they presented such a diameter as to prevent their passing without great difficulty through the opening made in the perineum. One of them was extracted whole with the forceps, but the others, being larger, were crushed and removed piece-meal. The bladder was thoroughly washed out with a syringe and warm water, so that nothing of the debris remained. A catheter was left in the

urethra for a short time, but was afterwards removed. The urine continued to flow from the wound, as is usual, for a few days, but finally resumed its natural passage, and the wound cicatrized completely without any irregularity. Dr. Miller informs us, by letter, that he is entirely well, and that no unpleasant symptom has succeeded the operation.

The amount of calculous matter removed weighed one ounce and a half, and consisted, most probably, (we did not analyze it,) of the uric acid deposit. The stone which we succeeded in removing entire, weighed three drachms and twelve grains, and was very rough and nodulated, which was the case with the larger pieces of the two that were broken. To this feature in our case we find it difficult to give a satisfactory explanation. It is usual, when more than one calculus exists, to find them of a smooth, even of a polished surface, from the trituration to which they are subjected during the constant shaking of the bladder in walking and other movements of the body, whereas in our case, notwithstanding the number, size, and softness of the calculi, they each presented as complete mulberry surfaces as we have ever seen in any solitary stone. On this account this case may be considered somewhat instructive, as it tends to establish the precept that on the removal of one stone, we must not feel certain that another does not remain, merely because the first presented a rough and granular surface, but should ascertain by more positive proof that our patient has been entirely cleared before we end the operation.

PART II.

Eclectic Department.

On the Diagnosis of Yellow and Bilious Fevers. By ASHBEEL SMITH, M. D., of Texas, M. A., formerly Surgeon-General of the Army of the late Republic of Texas, &c., &c. Read Sept. 6, 1848.

I shall trace the differences between bilious and yellow fevers, first by reference to some general laws which govern these diseases, and afterwards by a comparison of their symptoms and pathology.

Those persons who maintain the identity of yellow and miasmatic fevers, generally hold the former to be an aggravated

type of the latter. If this opinion be correct, then it would seem that every case of yellow fever should be necessarily of great severity or malignity. This is not the fact; so far from it, that after the first period of a yellow fever epidemic, a large proportion of the cases, often more than half, are of a very mild character, sometimes only a febricula of a few or several hours' duration, or a brisk ephemera. It would be scarcely accurate to say that these mild cases are only some general effects of the prevailing epidemic influences; for such mild attacks afford immunity against the disease during the same epidemic, and also in subsequent epidemics, so far as my observation and information extend. Besides, these very slight or mild cases are in their physiognomy as distinctly characterized to the practitioner familiar with yellow fever, as are the most malignant cases. Perhaps my meaning will be more explicit by stating at length a parallel between yellow and bilious fevers. From the mildest intermittent, amounting only to a sensation of indisposition or *malaise*, recurring at regular intervals, to the most ferocious congestive fever, we trace a regular, uninterrupted gradation. And in my section of country it is common to see in the same individual, one grade converted into or succeeded by another within the period of a single interval; that is, a mild intermittent become congestive, or assume the form of a malignant remittent; and, *vice versâ*, the two latter forms disappear in the shape of a common ague and fever, or a mild remittent. So in yellow fever there is a regular gradation from the exceedingly mild febricula through all stages of severity to the most ferocious black vomit. We also see sudden conversions of grade in yellow fever similar to those mentioned as occurring in miasmatic fevers. The inexperienced practitioner is surprised to find the stomach deluged with black vomit in a case which some hours previously had given him no apprehension; and an invasion of excruciating severity may be succeeded by a state of extreme mildness, with scarcely a symptom of diseased action. To sum up, instead of malarial fever running into yellow fever, the same parallelism from great mildness to extreme severity, with sudden conversions not into each other, but into different grades of the same disease respectively, subsists in the two diseases.

I now proceed to another point. In places subject to epidemic yellow fever, sporadic cases do indeed sometimes occur in healthy seasons; but as a general fact, yellow fever is an epidemic. In Galveston, the disease prevailed epidemically in 1839, 1844, and 1847; nor have I ever known a case of domestic origin occurring in that city, except during seasons of its epidemic existence. When miasmatic diseases exist, they

are endemic and occur more or less every year, no year being wholly exempt from cases.

Bilious or miasmatic fevers pervade broad districts of country, and persons residing anywhere therein are subject to attacks of the same. Yellow fever is confined to the narrow limits of towns, which limits may be easily learned, and the disease avoided. In the State of Texas, throughout the level prairie country, intermittents and remittents are endemic. Few, indeed, are the plantations and farms, if any, that have not been the *habitat* of this endemic; most of them present cases every year. But I never heard it even suspected by any medical gentleman that a case of yellow fever had originated anywhere in the interior out of the towns. If yellow fever were an aggravated or modified form of bilious fever, we should occasionally meet cases of the former in those sections of the interior country where bilious diseases in all their varieties prevail.

In my section of the Union, and I believe it is a general fact, villages and towns are more exempt from miasmatic disease, though not wholly so, than the surrounding country; on the contrary, yellow fever affecting concentrated populations, the farm-house and plantation are perfectly secure from its visits. Indeed, yellow fever is still more circumscribed; it very generally makes its appearance in a particular quarter of a town, in a district consisting of a single street, or a few streets, to which for the time being it is confined, thence extending more or less gradually over adjacent districts, and so regularly that its progress can be pretty accurately marked. No such rule obtains in regard to intermittents and remittents.

It has been alleged that, though miasmatic diseases prevail in regions exempt from the visitation of yellow fever, nevertheless the latter is developed only in a malarious atmosphere; and the conclusion is thence deduced, that common miasmata are essential to the production of yellow fever. What causes, if any, yellow and malarial fevers have in common, I am unable to decide. Nor am I sure that the following fact will be deemed relevant to this part of my subject. Galveston is situated on an island, and has been thrice visited by yellow fever in ten years; yet residents in the city who do not visit the country or interior are, so far as my observation extends, quite exempt from agues, fevers, and all forms of miasmatic disease. There are apparently sources of miasmata within and adjacent to the city; perhaps the level site of the town and the regular trade winds of the warm season may prevent the collection and condensation of miasmata. It seems scarcely necessary to repeat, on the other hand, that yellow fever at times prevails epidemi-

cally in towns which are every year the *habitat* of endemic malarial fevers. In the city of Houston, bilious fevers are frequent and endemic. Yellow fever also has raged epidemically in that city equally as in Galveston, this disease having hitherto occurred in these two cities at the same periods. By comparing, then, the general hygienic state of Galveston and Houston, we have yellow fever epidemics and absence of miasmatic fevers in Galveston—and yellow fever epidemics with annual endemic miasmatic fever in Houston. It hence seems that the two fevers in question are, for their origin and development, governed by laws, and produced by causes, irrespective and independent, to some extent at least, of each other. This position could be further corroborated by appealing to facts collected by other observers; but I limit myself to what has come within my personal observation.

In the yellow fever epidemics which I have witnessed, most of the first cases which occurred were malignant, and proved fatal. This fact is not noted as peculiar to epidemics of this disease; the same was true of the epidemic cholera which I observed in Paris in 1832. Whether the awful mortality in the first period of that epidemic is attributable to treatment, I shall not pretend to determine absolutely. In my opinion the disease was more malignant in its nature and rapid in its course in the earlier than during the later periods of its epidemic prevalence. So in scarlatine epidemics, I have remarked extreme malignity and rapid fatality of the early cases, with comparative mildness of those occurring in subsequent periods. Herein yellow fever is assimilated to other specific epidemics; while no such character is impressed on miasmatic endemics.

Yellow fever requires as an essential condition for its first development a high range of the thermometer. Allow me to make my meaning clear. In a city where cases of yellow fever do not exist, the disease is not excited, the epidemic is not kindled, cases cannot originate unless beneath an ardent sun. According to my observation, the thermometer has marked at 3 P. M., upwards of 80°, more accurately 84° to 88° for several days in succession previously to the first appearance of yellow fever. When, however, the epidemic has been established, it continues to rage, though the thermometer fall permanently to a point at which the disease could not be originally developed. Now bilious fevers occur continually at temperatures too low for the origination of yellow fever, as well as during the extremest heats of summer and autumn. At first blush one might be led to think that the supervention of an ardent sun to the causes of bilious fever would convert this latter disease into yellow fever. But this cause alone is insufficient, for then

we should find such conversions taking place in the country districts; which, as we have seen, never happens.

It may be noted among the general points of difference between the diseases in question, that it was once commonly held, and not a few still entertain the opinion, that yellow fever is contagious and importable, while no suspicion of a similar quality attaches to bilious diseases.

I have made incidental allusion to causes or rather conditions necessary for the production of yellow fever, without attempting to define them. We are fully authorized to declare that several causes or conditions must co-exist; the present state of our knowledge does not authorize us to say that some of these causes may not be common to bilious and yellow fevers.

Having thus set forth some general points concerning yellow and bilious fever, I now proceed to consider those symptoms which strogly and definitely distinguish the two diseases from each other. It would seem at first blush that my purpose would be best accomplished by grouping together the symptoms of these diseases respectively; but as they are well known to all of you, I proceed at once to note their peculiar and discriminating symptoms.

Bilious fever is intermittent, remittent, or continued. The continued type cannot in strictness be so called; for there are marked exacerbations or remissions every day or every other day.* Yellow fever is neither intermittent, remittent, nor continued; there are never two similar recurring paroxysms or exacerbations. It is essentially and uniformly a disease or fever of one single paroxysm. This paroxysm consists of states or stages, as follows: *first*, of depression; *second*, of vascular excitement, which subsides of itself, if not rendered ataxic by injudicious medication, into a state of apyrexia, terminating at once in convalescence or in the *third* stage of prostration with new and peculiar symptoms; this last is the stage of hæmorrhage or black vomit. The state of vascular excitement is never renewed or repeated any more than is the eruptive fever in the succeeding stages of small pox. I have indeed seen cases when the subsidence of the vascular excitement was prevented, and the disease converted into a fierce continued *causus*, obviously the effect of injudicious medication. Frequently also, there is a feverish state continuing to the close of the disease, but it bears no likeness to the stage of vascular

* I cannot recall any case of continued bilious fever with *quartan* exacerbations. The periodical character of this fever sometimes nearly disappears in cases which assume a nervous form towards their termination. But in all such cases which have fallen under my observation, the fever has been irritative, and dependent on some anatomical lesion generally of the mucous coat of the alimentary canal; the lesion being a sequela of the previous periodic fever.

excitement alluded to. On the contrary, if an artificial febricula is not kept up by medication, the patient falls into a condition the most opposite to pyretic. In many hundreds of cases of yellow fever which have fallen under my observation, I have never witnessed a second febrile paroxysm in the same case, like the first. I have indeed seen yellow fever supervene on an intermittent in persons affected with the latter coming into a district where the former prevailed; but in these cases the yellow fever occupied at once the whole ground, if I may so speak, and marched its course to its termination, regardless of the paroxysmal disposition of the disease which it had thrust out. In like manner I have seen the malignant miasmatic or congestive fever seize a person convalescent from yellow fever, who had been previously exposed to concentrated miasmata in the country districts; but the march of the two diseases was distinct, nor could they be confounded. Yellow fever, as has been stated, is sometimes rendered ataxic by injudicious treatment or neglect; but a second paroxysm similar to the first is not reproduced. To sum up: *billious fever consists of similar paroxysms or similar exacerbations; yellow fever never repeats itself, is not paroxysmal, has no character of periodicity.*

Relapses and second attacks are frequent in malarial fevers, and the liability of the system to these forms of fever is increased by previous attacks. *Not unfrequently it is impossible for a patient to become firmly convalescent but by removal from the malarious district.* Herein are to be noted striking differences between the two diseases whose diagnosis I have undertaken to set forth. *Relapses*, properly so called, do not occur at all in yellow fever, and second attacks are very rare, and especially in the same locality; and *the immunity may be regarded as absolutely complete, provided the patient remains permanently and continually in the epidemic district.* In this disease, however, patients imperfectly or seemingly convalescent do by imprudence in eating or exposure fall into the last stage or that of prostration, characterized by black vomit, hæmorrhages, etc. There is a deceitful pause between the subsidence of the vascular excitement and the setting in of the period of prostration which may last even for several days; during this time the patient may eat, walk about, and be apparently convalescent, when black vomit may suddenly supervene and the case terminate fatally. In such cases, however, the vascular excitement, or in one word the whole paroxysm, is not renewed as in periodic fevers, but the fatal symptoms supervene without prelude upon the delusive pause. *The hæmorrhagic state of the system subsists during all the time of the delusive pause, is easily cognisable, and distinguishable from established convalescence by practitioners familiar with the disease.*

As already stated, second attacks of yellow fever are rare, and when they do occur, it is only on exposure in *different* epidemics. I have seen and treated several hundreds of cases without a single relapse, and at most but one ascertained case of second attack.

This is not the place to enter upon the consideration of how far and under what circumstances an attack of yellow fever in one section of country furnishes immunity against the disease on exposure afterwards in other *habits* of this epidemic.

One attack of yellow fever protects a person against a subsequent one, as we have seen; but yellow and bilious fevers afford no *reciprocal* immunity against each other. I am here led to remark that persons born and reared in southern malarious districts, who have never visited a cold climate, are as liable to yellow fever on entering an infected district as any other persons whatever. A pretty numerous family, all born and reared in the vicinity of Mobile, but who had never been exposed to yellow fever in that city or elsewhere, removed to Galveston. They all had attacks during our last epidemic, two of which proved fatal. The disease was as violent as in persons coming from the highest northern latitudes.

Black vomit is the natural and regular termination of fatal cases of yellow fever, when the disease has been allowed to run its course unchecked or unmodified by treatment. I have been called to see a considerable number of persons in the last stage of this disease who had previously taken no medicine whatever. In every instance I found them pouring forth black vomit. And in general, an examination after death of those who ejected black vomit during life, almost always reveals in the stomach this matter, where death took place within ten days after the invasion of the disease.

Several authors mention black vomit as being of not very uncommon occurrence in other diseases than yellow fever. This does not correspond with my own experience. I have indeed many times seen a brownish saburral matter vomited in malarial fevers, but it would be a great error to confound this with black vomit, from which it is easily discriminated. Matters have been twice brought to me, which it was very difficult if not impossible to distinguish from genuine black vomit, ejected from the stomachs of patients laboring under other disease than epidemic yellow fever. But I am bound to declare that during a most extensive practice in the malignant and congestive bilious fevers of the southern country, I never saw genuine black vomit, in my own practice, out of the pale of well characterized yellow fever. Of the extent of my observations in malignant bilious fevers, some notion may be

formed from the fact that in 1837 I prescribed for 119 cases of this disease in one day; most of them being soldiers of the old Texas army. So extensive a practice ought to have furnished at least one case of black vomit, if it is of frequent occurrence in paludal fevers.

Black vomit is so peculiar and distinctive a symptom of yellow fever that I have chosen to consider it separately; it may nevertheless be perhaps regarded as part of or resulting from a general hæmorrhagic disposition which exists in this disease; which general *hæmorrhagic disposition* or character is eminently diagnostic between bilious and yellow fevers. Hæmorrhages are rare, quite rare in bilious fevers; they are *extremely common* in yellow fever, much more common than is usually supposed; often occurring unsuspected either by patient or physician. My attention has been much directed to this characteristic hæmorrhage. Autopsies have revealed to me numerous and copious internal hæmorrhages whose existence was wholly unsuspected during life. I have found them everywhere in the abdominal cavity; in the large and small intestines and stomach; beneath the peritoneum; in the cellular substance investing the kidneys and renal capsules; between the laminæ of the sac of Willis; into the kidneys; into the substance of the liver; into the bladder of urine; from the uterus; from the surface of the scrotum; from the gums, nose and ears; into the subcutaneous cellular substance of nearly every portion of the surface. The internal hæmorrhages alluded to as being revealed only on dissection are not of a few drops or a teaspoonful or two; I found them in quantities of a gill, half pint, pint, and quart. I have stated that these hæmorrhages are often unsuspected; they occur, moreover, in cases which though severe, present no immediately alarming symptom. I have drawn off bloody urine where the patient did not complain of the slightest uneasiness; I have learned, on inquiring the existence of free uterine hæmorrhage which the patient had not mentioned, supposing it to be a return of her catamenia. On examination in cases not regarded as dangerous, I have found extensive infiltrations of blood into the cellular substance of the lower extremities, giving them a marbled appearance, which, nevertheless, had escaped the notice of the patients. Hæmorrhage from the gums is common where not a grain of mercury has been administered. Now, hæmorrhage from any part of the system is extremely rare in miasmatic diseases; I do not remember at this time a case of hæmorrhage from the gums in paludal fevers without mercurial salivation or a strongly marked scorbutic diathesis.

In illustration of the frequency of hæmorrhage in yellow

fever, I will state that at my recent visits to Staten Island, among ten patients I saw in the hospital and village, there were three cases of hæmorrhage from the gums; one from the bowels; one of ulcerated scrotum; five of black vomit. It is proper to state that some of the patients had bleeding from different parts of the system at the same time.

The peculiar, and not easily describable physiognomy of yellow fever is so constant that it ought not to be omitted. To one familiar with the disease, it is as striking and unmistakable as are the ways and physiognomy of a drunken man.

There are several symptoms, which, though not of themselves characteristic of yellow fever, should nevertheless be mentioned in contrasting this disease with malarial fever, from their frequent occurrence in the former, and their infrequency in the latter disease.

In a majority of fatal cases of yellow fever, the pulse in the last stage is preternaturally slow, unless quickened by stimulants, and sometimes in despite of stimulants. On merely looking at the patient in the Quarantine Hospital, I remarked immediately to Dr. Whiting, that man must have a slow pulse, it cannot be over fifty. Being counted, it was thirty-two—it fell in a few hours to eighteen, several hours before his death. An intervenient feeble pulsation was sought for in vain. In bilious fevers it is rare to find the pulse below the healthy standard of frequency, except after the complete abatement of the disease.

In yellow fever, the skin, after the subsidence of the vascular excitement of the setting in of the third stage, is dry, has a peculiar harsh feel, and its temperature sinks below the healthy standard; when cerebral congestion co-exists, there is moisture about the wrists and ancles. In advanced stages of bilious fever, or in severe congestion, if the surface is cool, the extremities are bedewed with sweat, more or less clammy and copious.

In severe cases of yellow fever, the urine is generally of an intense saffron yellow, not unfrequently deposits blood on standing, as heretofore stated, and the bladder is often insensible to distension. In bilious fever the urine is more commonly of a reddish hue, and scanty, or copious and limpid; nor have I remarked insensibility of the bladder to its presence.

There is an occasional symptom in yellow fever which I have not met with in any other febrile affection; it is spontaneous and genuine venereal appetite in the stage of extreme prostration, and indeed of irremediable danger. *Memoriâ teneo meipsum hoc morbo graviter laborantem, memento quidem periculosissimo, ignibus veneris insolitis vehementer arsisse.*

Great serenity of mind, or rather sluggish indifference, marks

the final fatal stage of yellow fever; this, though not peculiar to the disease in question, is not quite observable as contradistinguished from the usual condition of the mental faculties of persons verging to death in bilious fevers.

Among the less characteristic symptoms deserving remark, is the frequency of attacks in the night by yellow fever, and the comparative infrequency of nocturnal attacks of paludal fevers.

Yellowness commencing commonly before death, and becoming more intense after that event, is so frequent as to have given name to the disease; yellow suffusion is, nevertheless, occasionally met with in bilious fevers; but my observations lead me to think that a very noticeable increase in intensity after death is of rare occurrence in the latter disease.

I now proceed to note briefly the pathological distinctions between the two diseases under consideration. These are in yellow fever, the general hæmorrhagic disposition, black vomit, yellow or fawn colored liver, and a pathological condition of the mucous membrane of the stomach. Much that I should have to say on the hæmorrhages and black vomit has been anticipated under symptomatology, and need not be here repeated. From the frequency of black vomit in fatal cases the Spaniards have thence called this disease *vomito prieto*, or *negro*. This matter is seldom found to be wanting in the stomach in such cases, that though some may have been ejected during life, I have met with it in all the autopsies I have made, with perhaps a single exception. This exception was in the autopsy of an individual whom I never saw during life, but who died, I have reason to believe, of yellow fever. In a very few cases which were fatal from the twelfth to the twentieth day after attack, I suspected the stomach would be found to contain no black vomit, but opportunity was not afforded me to ascertain the fact by dissection. So, also, in the body not examined of a person whose case was rendered ataxic, by treatment, and terminated fatally on the eighth day, I did not suspect the presence of black vomit in the stomach. I note these conjectural opinions in order to qualify the general statement of the results of my autopsies. I have never met with black vomit in my dissections in paludal fevers.

I have always found the gastric mucous coat in a pathological condition in yellow fever. In the formation of black vomit, portions of the gastric mucous membrane are congested or gorged with blood, which is poured into the cavity of the stomach possibly by simple effusion, or, as I believe, by an imperfect or diseased vital process. The portions of the coat which furnished the black vomit are softened and generally

thickened ; but in the present state of pathological science and language, these effects cannot be distinguished from the effects of other diseases upon the same tissue. They will not, therefore, serve the purpose of practical diagnosis. I have, however, had opportunity in several cases to observe portions of the mucous coat in the state of sanguineous engorgement previous to the formation of black vomit—other portions of the tissue having furnished this fluid and being in the condition above mentioned—and I do not hesitate to say that I can distinguish the sanguineous engorgement in this disease from the congestion in any other disease I have yet witnessed. Common gastritis can occupy one portion of the mucous coat, while another portion of the same furnishes black vomit. I shall not here pursue this point further, as my present object is practical diagnosis.

The liver in yellow fever is paler than natural, varying from a pale straw to a bright coffee and milk color ; the hue is uniform or mottled ; in a few rare cases it is of a deep dark color. The intensely dark liver, of which I have seen two cases, was owing to a hæmorrhage into its parenchyma. In all my other dissections, the fawn color of Louis has been distinctly noted, with the exception of one liver which was not remarked on. Three autopsies were made at the Quarantine Hospital on Sunday last ; two of yellow fever cases, and one of dysentery. The livers of the first two were of bright coffee and milk hue, and their stomachs contained black vomit ; these livers contrasted strongly with the mahogany colored liver of the dysenteric body which was emaciated and exsanguineous. I beg here to state that this yellow discoloration was noted by me in six out of eight yellow fever autopsies *published before* the publication of M. Louis's book, and before I had heard of his observations. But to him alone belongs, so far as I know, the merit of calling the special attention of medical men to this pathological appearance.

I have not seen black vomit in the stomach nor the yellowish discoloration of the liver, nor the internal hæmorrhages in my dissections of bodies dead of bilious diseases. It does not seem necessary for our present purpose to describe minutely the pathological appearances in bilious fevers. It is possible that in dissections made years ago of fatal cases of bilious fevers, I may have noted as natural, livers which more careful observation should have described as bronze colored. In the autopsy of a case (Capt. Thompson,) of malignant congestive fever coming from Tampico, last year, the bronze colored liver as described by Drs. Stewardson, Swett, and others, was very strongly marked.

I will here state the present year, I have seen a *distinctly marked straw colored liver* in a child that died of cholera infantum with metastasis to the brain.

I have not deemed it necessary to establish a formal diagnosis between yellow and typhoid fevers. The fact that yellow fever requires an ardent sun for its development, while the typhoid affection prevails most in colder temperatures, as well as the universally known symptomatology of the two diseases, sufficiently distinguish them from each other. Still it may be proper to add, under the head of pathology, that in my numerous dissections in yellow fever, I never met with the anatomical lesion of typhoid fever, to wit, ulceration of the elliptic patches of Peyer. Typhoid fever, though extremely rare in the coast district of Texas, is nevertheless sometimes met with. I was invited last August by a medical gentleman to Galveston to see a lad seven years of age, a native of Texas, and always resident there, laboring under fever. The case presented unmistakable symptoms of the typhoid affection; he died the 15th day from the attack; the autopsy made by me exhibited numerous and large ulcerations of Peyer's glands. For many days preceding, the weather had been dry and warm, the thermometer at 3 P. M., ranging from 84° to 90°; the barometer at 10 A. M., from 30.16 to 30.20 inches.

I have thus nearly completed, how imperfectly I am very sensible, the task I imposed on myself of setting forth the distinctive characteristics of bilious and yellow fevers. My main difficulty, as already intimated, has lain in my inability to paint adequately the physiognomy of the assemblage of symptoms which constitute the two diseases respectively. To my mind's eye, they neither blend with nor run into each other; but the prominent features of demarcation can be clearly and fully appreciated only by personal observation. A person must see with his own eyes in an *intermittent*—the chill, the fever, and complete intermission—to be succeeded the following day or some subsequent day, at regular intervals, by a similar chill, fever, and intermission, and this state to continue for days, months, and even years. In a remittent, he will see chilliness, exacerbation of the fever, great remission—and these symptoms repeated with great regularity, with diurnal or double tertian regularly for a number of days. If the bilious fever be of a *continued* type, he will witness exacerbations and remissions occurring with the regularity of remittent, and like it for days in succession. If he encounters a malignant intermittent or remittent, usually called *congestive* fever, the patient is often comatose or delirious, his extremities and part of his body are dank and colder than a corpse, in an atmosphere perhaps of from 80° to 95°. With this deadly coldness of the surface there is a burning heat of the entrails, shown by unquenchable thirst and incessant cries of the sick man for cold drinks, unless

the stupor be so complete as to mask all sensation. In an hour or two, or a few hours, the scene is changed, and if reaction be complete, a burning heat of the surface has succeeded to the algid darkness. This state in its turn gives place in a few hours to an intermission or remission, as in the milder forms of bilious disease, with perhaps scarcely a symptom of urgent danger. But on the following day, or a subsequent one, a paroxysm similar to the first is repeated. After a few repetitions, if the disease be not arrested by art, or converted into a milder form, it terminates fatally in the cold stage.

If a case of *typhoid fever* be presented, he sees a febrile disease coming on slowly and stealthily, requiring days to reach its acme, and days for its subsidence. Changes which take place in hours in bilious or periodical fevers, require as many days in typhoid fever. Exacerbations are wanting or scarcely distinguishable; nervous and other symptoms rare in other diseases are common in this; convalescence is slow; tardiness is impressed on all its stages. If fatal, ulcerations in the lower part of the small intestines are found on examination after death.

In yellow fever, in a case not rendered ataxic, one sees the depression or chill, the fever with the agonizing pains of the forehead and loins; these subside spontaneously within three or four days into a state of apyrexia. This is the only paroxysm; there will be no subsequent vascular excitement. The patient may now become convalescent without further adverse symptom; but he is very liable to sink into a state of mental sluggishness, he kicks off the bed-clothes, sits up on the edge of the bed, or walks about the chamber with a peculiar unconscious indifference; suddenly a turbid dark fluid gushes from his mouth, or the black vomit makes its approach more insidiously, first an occasional slight eructation of air, then a hiccup with the ejection of a teaspoonful or two of a thin whey-looking fluid; the fluid is ejected more frequently, in larger quantities, and darker and more turbid; hæmorrhages have taken place from his gums, or other parts of his system; his pulse become slow and sluggish; his respiration infrequent and protracted, becoming more infrequent, protracted, and feeble, so that you can scarcely tell when he breathes his last; or he dies suddenly with a copious gush of black vomit; or he drops dead while walking about. An examination after death reveals the black vomit, the hæmorrhages, the ravages in the stomach and liver already noted.

I have spoken of yellow fever as a disease of pretty uniform character. Such has been my observation; and the descriptions of those practitioners whose treatment has seemed to me most judicious and least perturbing, present a similar and

pretty uniform symptomatology; quite as much as any disease with which I am acquainted. Cases have been rendered ataxic by injudicious treatment, and nosological varieties have been thus multiplied at the expense of true science—and of patients too. In the commencement of my practice, I made bad ataxic cases. Such cases do, indeed, occur without any fault of the practitioner, or exposure of the patient; sometimes they are produced by the overwhelming violence of some symptoms, marking or ruling out others. Slight modications are, it is true, presented in different epidemics and in different periods of the same epidemic, but it is chiefly in the minor symptoms, or in the comparative severity of the leading symptoms; so that I regard the formal division of yellow fever into different species, types, and forms, as being unphilosophical and attended with no practical advantages.—[*Transactions of the N. Y. Academy of Medicine.*

On the Physiology and Pathology of the Phosphate and Oxalate of Lime, and their relation to the formation of cells. By WILLIAM BENEKE, M. D., Resident-physician at the German Hospital, Dalston.

In a small work, which I have lately published in Germany, entitled “*Der Phosphorsäure Kalk in physiologischer und the rapeutischer Beziehung*,” Goettingen, 1850, I have established the following results:—

1st.—Just as in plants and inferior animals the phosphate of lime is indispensably necessary in man for the formation of cells; this formation does not only depend upon the presence of albumen and fat, but likewise upon the presence of phosphate of lime.

2ndly.—The want of phosphate of lime, either in plants or animals or men, causes a deficient formation of cells; and a great many pathological states of the system really seem to depend upon a deficiency of phosphate of lime.

3rdly.—In accordance with these general laws, we must suppose that we are enabled to cure, or at least to alleviate, by the internal administration of phosphate of lime, diseases marked by emaciation, formation of ulcers, in one word, by a deficient formation of cells.

4thly.—I have shown by my experiments, that such really is the effect of the administration of phosphate of lime,—that is to say, I have produced, by the internal administration of phosphate of lime, an undoubted increase of the cell-formation in diseases evidently showing a diminished formation of cells.

5thly.—As diseases or affections of this kind, which have come under my observation, I have to mention,—

- a. Ulcerations of any part of the system, which are based upon general dyscrasia, such as scrofula, and which are not merely local affections.
- b. Infantile atrophy, especially the well-known atrophic state of children, suffering from rickets, and its accompanying symptoms, as diarrhœa, &c.
- c. Tuberculous disease, more especially of the lungs, in its earliest stages.

6thly. There seems to be a remarkable connexion between scrofula and deficiency of phosphate of lime. But as it generally ought to be mentioned, that we never shall be able to produce an increase of the formation of cells, unless we administer a wholesome, preferably nitrogenous diet; so it must be considered that the deficiency of phosphate of lime is only a constituent part of these diseases; and by its mere use we are very well enabled to remove symptoms, which depend on its deficiency, but by no means shall we cure thereby the dyscrasia *in toto*.

These are the results to which I have drawn the attention of the profession in the paper above alluded to; and as I cannot expect it to be known in this country, before proceeding further I shall give a short extract of the chief points of the facts on which I have founded my conclusions.

It is only a few years since we became acquainted with the great importance of inorganic substances in the formation of organic compounds. Liebig first established the fact, that in plants the formation of vegetable acids, of seeds, &c., does not only depend upon the presence of carbonic acid and water, or carbonic acid, nitrogen and water, but likewise on the presence of the inorganic constituent parts of the soil. This really being a fact, based upon observation and experiment, we must be induced to suppose that a similar relation between organic and inorganic parts exists in animals, and even in men. These inorganic parts of the economy, both of men and the inferior animals, are by no means mere incidental substances,—they are by no means unimportant; on the contrary, the time has come when we are far from admitting the least doubt of their necessity for the chemical processes which take place in these organizations; in one word, the inorganic parts are quite as essential as the organic compounds. This undoubted fact having been hitherto overlooked, we may judge of the correctness of the views applied to organic chemistry until within a few years, and especially of the analysis of blood, which have been performed principally in France and Germany; in nearly

all of them are exclusively considered the organic compounds, the inorganic parts being set aside.

It is not here the place to enter more deeply into these views. I have only to show how far we are acquainted with the relation of phosphate of lime to organic compounds, and even to the formation of cells.

In first considering plants, we have been informed by Liebig that the salts of phosphoric acid are indispensably necessary for the formation of nitrogenous compounds as well as of cells. In his elaborate, "Organic Chemistry in its Application to Agriculture and Physiology," the author affords a great many proofs of this result, out of which I beg to select a few. "None of our plants," as we read at page 100, fifth German edition, "can bear perfect seeds—that is, seeds yielding farina, without a large supply of phosphates and even ammonia, substances which they require for their maturity. In the stems and leaves of plants we find salts with alkaline bases; the azotic compounds we find always accompanied by salts of phosphoric acid, and we therefore must suppose that they are of the greatest importance in the economy of plants. The alkaline bases are principally wanted for producing sugar, gum, pectine, and amyllum—substances which do not contain nitrogen; on the other hand, the salts of phosphoric acid are principally necessary for the formation of azotic compounds." (p. 251.) These interesting remarks are based upon facts which do not admit of any doubt; they apparently prove the remarkable relation between the salts of phosphoric acid and azotic compounds in plants. But there is another point we have to direct our attention to, and this is the most important with respect to my present purpose. At page 138, Professor Liebig says: "We know that the quantity of starch in potatoes increases when the soil contains much humus, but decreases when the soil is manured with strong animal manure, *although in this case the number of cells increases*, the potatoes acquiring in the first place a mealy, in the second, a soapy consistence. Beet-roots taken from a barren, sandy soil, contains a maximum of sugar, and no ammoniacal salts, and the Teltowa turnip loses its mealy state in a manured land, because all the circumstances necessary for the formation of cells are there united." Well, then, we must trace the causes of the peculiar influence of the manure on the formation of cells, and referring to its constituent parts, and the experiments which have been made on their relative power, we shall be far from admitting the least doubt that the salts of phosphoric acid are those materials, upon the presence of which the formation of cells depends. This view is still farther proved to be correct by our finding that for manure of animal ex-

cretions, other substances containing their essential constituents, may be substituted. "In Flanders (Liebig, 'On Agricultural Chemistry,' English translation by Playfair, 1840, page 182) the yearly loss of the necessary matters in the soil is completely restored by covering the fields with ashes of wood or bones, which may or may not have been lixiviated, and of which the greatest part consist of phosphate of lime and magnesia. The great importance of manuring with ashes has long been recognized by agriculturalists as the result of experience. Its use will be at once perceived when it is considered that the ashes, after having been washed with water, contain silicate of potash in the same proportion as in straw, and that their only other constituents are salts of phosphoric acid." Finally, Liebig adds the important remark, (German edition, page 245,) that we may furnish a plant with carbonic acid and all the materials that it requires; that we may supply it with humus and ammonia in the most abundant quantity, but that it will not produce albumen, fibrine, and caseine, unless the salts of sulphuric and phosphoric acid are afforded to it. "We ought to suppose that without their co-operation, the ammonia does not influence in the least the formation of the nitrogenous substances."

This, then, is the result of the foregoing remarks: that the azotic compounds in plants are not produced without the co-operation of the phosphates; that the produce of cells increases proportionally to the power and quantity of the manure afforded to the soil, and that this power particularly depends upon the presence of phosphates, the other salts really being of no great importance in this respect.

But there are different sorts of phosphates contained in the soil, such as phosphate of soda, phosphate of lime, phosphate of magnesia, and phosphate of iron. Is there any reason to believe one of these several substances to be more important for the produce of nitrogenous substances and cells than the others? Facts are not wanting which afford an affirmative answer to this question. The single experience that we may considerably increase the produce of nitrogenous substances and cells by manuring the land with ashes of bones, is quite sufficient to prove that the phosphate of lime is of the greatest importance in this respect. A great many analyses of ashes of bones, communicated in the "*Annalen für Pharmacie und Chemie von Liebig und Wöhler*," by Ederlin, Fresenius and Will, and the analyses of bones by Berzelius, evidently show that the phosphate of lime is always present in ashes in a certain proportion, according to the nutritive power of the plants and the soil from which they are taken; and that, on the other hand, the proportion of phosphate of magnesia and soda

in bones themselves is too small to partake of the influence exerted upon the soil by manuring it with bones. Berzelius found in 1000 parts of dry bones, 53.04 phosphate of lime 1.16 phosphate (carbonate?) of magnesia, and 1.20 soda, with a small quantity of chloride of sodium.

On these facts, then, I have founded the conclusion, that the phosphate of lime is indispensably necessary for the formation of cells in plants.

With regard to the inferior animals, and the part which is performed in their economy by the phosphate of lime, I have only to mention an excellent paper by Dr. Carl Schmidt, "*Zur vergleichenden Physiologie der wirbellosen Thiere*. Braunschweig, 1845." In this paper Dr. Schmidt communicates most interesting experiments, from which it becomes evident, that in inferior animals the phosphate of lime has an intimate relation to the formation of cells. Dr. Schmidt ascertained beyond a doubt, that in the articulata the quantity of phosphate of lime increases or decreases proportionally to the quantity of chitin, a sort of colourless transparent tissue, which is not soluble in water, alcohol, ether, and liquor potassæ, and forms the principal constituent part of the skeleton of all the invertebrata. Now, this tissue is the result of an active formation of cells during the period of changing the integuments in these animals, and so it results that the quantity of cells formed is proportionate to the quantity of phosphate of lime present. This result becomes still more evident from the experiment which Dr. Schmidt describes in the following words: By carefully and gradually scraping off the outside of the hard scales of the thorax and of the claws of crabs to the upper pigment-layer of the subjacent membrane, I produced an exudation of new matter. This quickly took place, and, after the lapse of eight hours, a thickish, viscid, transparent mass (cyto-blastema) was to be found. This mass contained a large quantity of corpuscles, which did not dissolve in water, nor in acetic acid (fat,) and others, which were soluble in water and acetic acid (albumen.) No other corpuscles were present. When this mass was incinerated, a remarkable quantity of phosphate of lime (about eight per cent.) resulted, besides a small quantity of alkaline phosphates and carbonate of lime, the latter being the result of the incineration. The phosphate of lime was held in solution in the mass itself, as was proved by the turbidity resulting from the addition of a drop of ammonia to a drop of the liquid placed under the microscope. After the lapse of fourteen to sixteen hours the soluble molecules (albumen, and very likely phosphate of lime) surrounded the fat-globules, exhibiting in this way globular masses. Some of these masses were covered already

by a membrane, so as to form cells, others not. At the same time numerous rhomboedic crystals (carbonate of lime) appeared. By the addition of liquor potassæ a swelling of the cells and their molecular contents took place: afterwards they became transparent and dissolved; the fat globules were recognized as forming the nuclei of these cells. The cells did not yet present themselves as chitin. But after twenty-four to thirty-six hours several of these cells were extended, spindle-like; they swelled still by the addition of liquor potassæ, but they did not dissolve more in this stage, and appeared to be of the chitinous composition." These experiments are followed by others, which afford a negative proof in favor of Dr. Schmidt's views alluded to. In examining, namely, the calcareous scales of helix, and their interior, transparent, *structureless* membranes, he observed that no phosphate of lime was present, and accordingly no cells were found; nothing could be detected but amorphous, hardened, mucous masses, separated by layers of carbonate of lime. Dr. Schmidt himself says: These observations really are so striking as fully to confirm the opinion before advanced; and he further adds, as his firm belief, that a certain combination of albumen and phosphate of lime, or better, that a solution of albumen, which is saturated with the phosphate of lime, is particularly enabled to coagulate by the contact of heterogenous substances, and to form membranes around them—that is to say, walls of primary cells.

Well, then, having become acquainted with these remarkable facts, I put forward the question, whether the phosphate of lime might not have the same relation to the formation of cells in the higher classes of animals, and even in men, as it has been shown to have in inferior animals and plants? I have been fortunate enough to obtain satisfactory and affirmative results.

The way in which I tried to solve the question was a double one—first, I had to prove that wherever we find a formation of cells, the phosphate of lime is present, and *vice versâ*, that the phosphate of lime is wanting where no formation of cells takes place; secondly, I had to show that the phosphate of lime is indispensably necessary for, and that it really influences, the formation of cells.

With respect to the first point, I examined a great many pathological exudations, such as the serum produced by blisters, the exudations and secretions of wounds and ulcers, &c., &c.; and I have shortly to state that wherever a formation of cells took place, the phosphate of lime was found in considerable quantity, and wherever it was absent, I could scarcely find traces of phosphates. In order to detect the smallest quantities phosphate of lime, I used the microscope, which shows the

most beautiful crystals of sulphate of lime, if a preparation, containing the phosphate, is mixed with a small drop of sulphuric acid, and no crystals at all where no lime is present. However, it is requisite for a microscopical examination of this kind to be well acquainted with all stages of the formation of the sulphate-of-lime crystals, so as to be enabled thereby to judge whether the quantity of phosphate present is a large or small one; the former being the case, we shall observe a rapid and splendid formation of large crystals; the latter taking place, only the former stages of the crystal formation are to be detected.

First, I examined the serum which was drawn by blisters. It is rather difficult to detect the phosphate in a single drop of the unaltered serum in the way described, the quantity of lime really being a very small one. However in a single drop I detected crystals of the smallest size, by the addition of sulphuric acid, and by continued examinations, I found that the crystals presented themselves the more quickly and well marked the sooner the efformation of pus-globules took place in the serum, which was left beneath the skin. But from twelve to twenty-four hours are often required for the formation of crystals, and the preparation should not be judged of until during the lapse of this time repeated observations have been made. If, on the other hand, I slightly evaporated the serum in a hot-water bath, and now mixed a drop of the evaporated serum with sulphuric acid, a rapid formation of crystals generally took place, which undoubtedly showed the phosphate of lime to be present in large quantity. I then examined exudation-matter of wounds and ulcers, and these observations really afforded the greatest interest; they decidedly proved the relation of the phosphate of lime to the formation of cells. It will be well known to every accurate observer, that during the time of cicatrization of wounds and ulcers, two different sorts of exudation generally take place. First, an exudation appears, which I should like to call "spurious exudation," and which really exhibits nothing but a natural cover for the part affected or wounded, being far different from what we call "spurious granulations;" this exudation is subsequently thrown off; afterwards beneath this covering the real blastema is produced, affording the materials for the tissue which is to be formed, and undergoing the well-known changes to cells, tissue, &c. Well, then, if we examine microscopically what I have called the spurious exudation, we shall observe it to consist of amorphous structureless masses; no cells are to be detected; it only seems to consist of molecules; no organization takes place. And even in those masses, by the addition of sulphuric acid, I have never observed the formation of sulphate-of-lime crystals,

and consequently no phosphate of lime could be present. If, on the contrary, I examined in the same way the blastema produced beneath the spurious exudation, after the lapse of about twenty-four hours, I not only met with beautiful exudation-cells and pus-globules, but also, by adding a drop of sulphuric acid, could observe a rapid formation of crystals, so as to be led to the conclusion that the phosphate of lime is present in a large quantity, where cells are produced, and that it is wanting where we find nothing but amorphous masses. Lastly, with respect to this point, I have to draw the attention to the muscular tissue itself; and it will become evident, from my observations, how small a quantity of phosphate of lime we are able to detect by the kind of examination alluded to. The muscular tissue is well known to contain a certain quantity of phosphate of lime; it was the result of Liebig's inquiries, that when the formation of muscular tissue from the constituents of the blood takes place, nearly the whole quantity of alkaline phosphates returns into the blood, and that at the same time a certain quantity of phosphate of lime becomes chemically fixed in the organs themselves. This quantity, then, however small it may be in a muscular fibre, which is so fine, as, by microscopical examination, to show the transverse stripes, I have detected, in the above described way, in a few muscular fibres which I had submitted to the action of sulphuric acid for about twenty-four hours. I observed, after this time, by the microscope, crystals of sulphate of lime—of course only in very small quantity, but beautifully formed. Especially in this kind of preparation the different stages of crystal-formation are to be well observed.

After these experiences, the other question remained whether the phosphate of lime really influences and increases the produce of cells? I have tried to solve this question in a double way, first by experiments, and, on the other hand by practical inquiry.

With respect to the experiments, it is my firm belief that I succeeded in artificially producing cells, which did not show any distinction from pus globules and what we call exudation-cells. For this purpose I mixed together a part of the albumen of a hen-egg, some pure fat, a small quantity of phosphate of lime, and a very few drops of water; put the mixture, which was contained in an evaporating-dish, into a hot sandbath, at a temperature of 104° F., and then microscopically examined the changes which took place in the mixture itself. After the lapse of from four to six hours, I observed that an accumulation of albumen and phosphate had already taken place around the fat-globules; and after another period equally long, I found

those forms of cells which I have figured in the first plate of paper alluded to. In another preparation I distinctly observed the gradual process of the formation itself by the simultaneous appearance of all the stages of formation of cells. Considering as the first stage the pure fat-globules, I observed, as the second, fat-globules, which presented a turbid appearance, in consequence of an accumulation of albumen and of phosphate of lime on their outside; and this accumulation increasing more and more, produced cells, which did not differ in their appearance from real exudation-cells. By the addition of sulphuric acid to these cells, I obtained the same results as by treating exudation-cells or pus-globules with it: there appeared on the outside of the cells very small and dark forms of crystals, which I considered to be crystals of sulphate of lime. I further observed some larger cells, which included a smaller one, so as to exhibit the appearance of what is called the "ovum primitivum," the observations and communications of which are communicated by Rud. Wagner in his splendid work, "*Prodromus Historiæ Generationis*." It appeared to me, that in these cases an accumulation of fat took place on a ready-formed cell and that this fat-accumulation became surrounded again by the combined albumen and phosphate of lime. By the addition of acetic acid the cells became clearer, and dark molecules appeared on their surface.

Now with respect to the natural combination of the albumen of a hen-egg with a small quantity of phosphate of lime, I inquired whether the formation of cells should not take place by the mere addition of fat to it, and its continued exposure to a temperature of 104° F. I have instituted experiments with respect to this point; and I have found, that after the lapse of about eight hours some forms of cells could be detected, but their quantity was much smaller than in the above described experiments—a result which was to have been expected, if the theoretical views upon which I founded my observations were correct. In conclusion, I have to state, that I obtained the same results by repeatedly performing the same experiments, and I only hope that they may soon be repeated by physiologists who are known to be authorities in this kind of observation.

With regard to the above mentioned practical inquiries I equally obtained satisfactory results, which evidently showed that phosphate of lime increases the formation of cells, supposing a sufficient quantity of albumen and fat to be present. I must refer to my former publications with respect to the individual cases; here I only beg to mention the general results.

First I tried the phosphate of lime in patients who suffered from chronic ulcers, resulting from the scrofulous diathesis, and

exhibiting a want of formation of cells in the highest degree. These patients had been for a long time under medical treatment, inclusive of my own, but all remedies had been fruitless, such as cod-liver oil, ointments of lead and zinc, lotions of nitrate of silver, &c. Being myself quite sure, that no influence of the former kind of treatment could be still remaining in operation, and even after having left off all treatment for a long time, I then ordered the phosphate of lime to about four or eight, to twenty grains per diem, and after a few days the ulcers evidently showed themselves in another state. The suppuration improved; instead of an ichorous secretion, a pus bonum et laudabile was produced, and after a few days longer the cicatrization begun. In children, in particular, I obtained very striking results, and there was not the least doubt, that the effect must be ascribed to the phosphate of lime. Generally I ordered it to be taken with the breakfast, dinner, and supper, in order to have it mixed as well as possible with the food taken; it is certain, that the phosphate of lime easily dissolves in albuminous solutions as well as in mineral acids, and in the acid of the stomach. But I have especially to mention, that in several cases, some time after having left off the use of the phosphate of lime, I observed a fresh outbreak of the ulcers, for the real cause of which circumstance I could scarcely account at the time; I only supposed that the bad living of the patients, the food exclusively consisting of potatoes and bread, the abode in a damp unhealthy air, &c., caused the continuance of the general dyscrasia, and the repeated breaking out of the local affection. I am able now to account for it better than I could before, and with respect to this point, I must refer to the following parts of these communications.

As most of these ulcers occurred in patients afflicted with scrofula, the question arose, whether the phosphate of lime really cured the scrofulous dyscrasia, or only a part of it. With regard to this point from many observations, the number of which has much increased since, it resulted that there exists an undoubted intimate relation between scrofula and want of phosphate of lime, but that we are not able to cure the dyscrasia by the mere use of the phosphate. The same is the case with tuberculosis, a disease which is well known to be intimately related to, if not identical with, scrofula. In both kinds of disease, however, we shall promote the cure in the most efficient manner by the administration of the phosphate of lime, and I cannot forbear recommending its use as much as possible. In the following pages I shall give the explanation of these facts, which I hope will at once prove, that the effect of the phosphate ought to be such an one as I imagined and really found

it to be. I have especially to mention, that the waste of tissues, or, in other words, the want of formation of cells, was apparently less in many cases of tuberculosis and scrofula, which were treated with phosphate of lime, besides other remedies, than in those which were treated without the phosphate; that the cure of tuberculous ulcers of the intestines was evidently promoted and even effected by the administration of the phosphate, and this remedy proved most efficient in cases of incipient acute tuberculosis, and even those which are well known to manifest at their commencement nearly all the symptoms of typhus. I need not enter here more deeply upon the special effects of the administration of the phosphate in these cases, if the one general fact is always kept in mind, that it increases the formation of cells, or prevents the rapid and fearful waste of tissues.

In accordance with those facts, the phosphate of lime proved most beneficial in children who suffered from scrofula, diarrhœa, ulcerations and excoriations of the skin and bowels, general waste of the cellular tissue, loss of power, &c. In these cases, the mere use of six to ten grains per diem was often quite sufficient to effect the cure, and I have met with such striking and satisfactory results, as to leave no doubt that the want of the phosphate of lime was the real cause of the symptoms alluded to. These results, too, are confirmed by many physiological observations. Chossat, for instance, observed that pigeons, in consequence of an artificial want of phosphate of lime in their food, became afflicted with diarrhœa and softening of the bones; children, in the period of dentition, or when afflicted with rickets, which disease positively shows a want of phosphate of lime, are especially well known to suffer from diarrhœa, sores, &c. Still I have to mention the interesting observation, that in patients who were afflicted with diseases, in which the phosphate of lime proved beneficial, and who consequently were supposed to suffer from a want of phosphate of lime in their economy, wounds of occasionally applied blisters healed much more slowly than was the case in other patients, and the more so the more distinctly those symptoms appeared which should be referred to a want of phosphate of lime.

In syphilis I also tried the phosphate of lime, and even in persons who had for a long time already suffered from secondary ulcers, ulcers of the bones, &c., and became emaciated and extremely weak during that time. These cases likewise showed a most beneficial effect of the phosphate on the formation of cells. It ought to be mentioned, that besides the phosphates, the iodide of mercury was administered, but I never have met with such a rapid cicatrization of syphilitic ulcers,

as was the case in these persons, and I could not help thinking that the cure was promoted in a remarkable degree by the internal exhibition of the phosphate. With respect to this point, I have drawn attention to the relation between scrofula and syphilis, and I think there are many symptoms in both of these diseases, and many facts besides which evidently show an intimate relationship between them, and require a more accurate study than has been given to them hitherto.

As to other affections, in which I have tried the phosphate of lime, I have to mention rickets, caries, inflammations, and consecutive abundant suppurations of the cellular tissue, and also fracture of the bones. In all these cases the administration proved most beneficial, and I would strongly advocate its further experimental use. With respect to fractures of the bones, I have to state in particular, that the consolidation of the callus took place in a much shorter period than is generally the case; however, too large doses of phosphate of lime must be shunned in these cases, as I have observed an abundant callus, causing a deformity of the bones, produced by the daily administration of twenty-four grains of the phosphate for a fortnight.

With regard to the fact that phosphoric acid is always produced in the economy by the changes of albuminous substances and supposing that this phosphoric acid might combine with the lime, if carbonate of lime has been given, I tried also the carbonate; but the results of its administration were not so favorable as those which followed the administration of the phosphate, unless in some cases where the urine presented a high degree of acidity. This even was the case in many children afflicted with scrofula, and it must be decided by future experiments, whether the carbonate of lime is preferable to the phosphate in these patients.—[*London Lancet*.

Casein in the Blood; Synthetical Proof by Formation of Artificial Milk.

Dr. Panum, of Copenhagen, of whose interesting accounts of the discovery of Casein in the Blood, we gave a translation in the July number of this Journal for 1850, has published two additional articles on the subject in the *Bibliothek for Læger* for April and July 1850.

In his article in the April number, after recapitulating the facts brought forward in his previous paper, and pointing out the difficulty of establishing a chemical or a physiological distinction between casein and coagulated albumen, the author observes:

"If we institute a comparison between the substance precipitated from blood-serum by means of water and diluted acetic acid, and albumen, separated in the same manner from its combination with soda, *we find* truly greater differences than can be distinguished between casein and coagulated albumen. * * *

"Before the publication of my former contribution, I had already performed various experiments with the view of instituting a more accurate comparison between the material under consideration and albumen separated from its combination with soda. To procure albuminate of soda, I separated the albumen from the serum, then added soda, and applied slight warmth. A greater or less quantity of albumen (according to the quantity of soda employed) entered into combination with the alkali as albuminate of soda, and was precipitated by acetic acid. A difference was here observed between the albumen of serum and that of an egg. For while albumen from an egg required no warming to promote its entering into combination with soda, or its precipitation by means of acetic acid, the albumen of serum had to be slightly warmed before it would unite with an alkali, or give a precipitate with acetic acid.

"The albumen which was precipitated from albuminate of soda, as above described, was always *soluble with difficulty, even in a considerable excess of acetic acid*; eight or ten parts of acetic acid to one of albumen produced no solution. On the other hand, the material *originally* present in the serum, and precipitated from it by acetic acid, was soluble in so slight an excess of acetic acid, that it is almost a *finer test of the acid, alkaline, or neutral properties of the liquid, than litmus paper*. This difference seems important; and can scarcely be without essential influence on the organic economy.

"The precipitate thrown down by acetic acid from albuminate of soda, appears as *firm flocks*, which are not changed by shaking, and which resemble what is seen when a not too highly diluted solution of albumen is boiled. The other matter produces a perfectly homogeneous opacity in the liquid, and only after some time forms something like a solid deposit in the bottom of the vessel, which, however, immediately disappears, and is resolved into a general opacity, when the liquid is slightly moved.

"Moreover, if the albumen precipitated from a somewhat diluted solution by boiling, or from a solution of albuminate of soda by acetic acid, be collected in a filter, it forms, when half dry, *an elastic mass, by no means glutinous*. The other substance, on the contrary, when half dry, forms *a very glutinous mass*, almost like turpentine or bird-lime.

"The albumen, when collected in a filter, has, when dry, a

dirty yellow brown colour. In my previous contribution, I referred to the circumstance, that the substance I am describing sometimes assumed a *beautiful green colour* when thoroughly dried. At that time, I attributed it more to accidental circumstances than to any essential difference, that the substance became green or yellow-brown on drying, as from the same serum I sometimes procured a green and sometimes a yellow product. I now find, that the substance is always of a beautiful green colour when the serum is perfectly clean and free from red blood corpuscles. But if serum be used, in which there is found a greater or less number of blood-corpuscles, the green colour is effaced by the accompanying redness of the blood, and the product becomes either dirty yellow or dirty brown, according to the number of blood-corpuscles. The green colour is removed neither by alcohol, ether, nor water, and seems to belong essentially to the casein.

“However lightly these differences may be thought of, they are yet not less important than the distinction already referred to, between the so-called soluble casein and albuminate of soda. The preconceived opinion that the material under consideration must be albuminate of soda can thus no longer serve as a proof against its identity with what is called casein, or the essential nitrogenous constituent of milk.”

Relative Quantity in the Blood.—Dr. Panum states that he engaged in a quantitative analysis of the casein in different individuals. He does not yet attempt to draw conclusions; but states that in 1000 parts of dried serum he has found in men, in 3 cases, from 4 to 7 parts; in 8 cases of women, from 5.5 to 12.5 parts. The greatest quantities (9.9—11.4—12.5) were in lying-in women shortly after parturition. In nurses there was an average of from 6.5 to 7.4 parts.

Formation of Artificial Milk.—In the July number of the *Bibliothek for Læger*, Dr. Panum writes as follows:—“It occurred to me that the question of the identity of the material I have been describing with casein, might be solved by the *synthetical* method, which, though it can seldom be satisfactorily employed in scientific diagnosis, yet in the inability of analytical chemistry with regard to the protein compounds, is not to be despised. If it were possible, by adding the necessary constituents, to form from the substance under consideration some universally known product containing casein, such as milk or cheese, we should have a striking proof of the identity of the substance with casein.

I obtained a quantity of serum from bullock's blood. To this I added acetic acid, in the proportion of six drops of concentrated acid to an ounce of serum, and then a considerable

quantity of water. The substance described in my former contributions sank, on standing, to the bottom of the vessel, so that the superabundant clear fluid could be poured off. On again adding water and letting the substance sink to the bottom, it was rendered almost perfectly free from soluble albumen, etc. After it had settled as much as possible, the water was poured off, so as not to waste the substance, which remained suspended in a greater quantity of water than casein is diluted with in milk. It was now my object to find out, whether the solution of this substance produced by salts or an alkali could be brought to coagulate by contact with the mucous membrane of the stomach of kittens or puppies; and whether it were possible to produce a substance which, in taste and other circumstances, should agree with milk. But as it would probably be difficult to make animals use the solution as milk, unless the other constituents of milk, namely butter and sugar, were mixed with it, and as the taste and peculiarities of cheese might be modified by them, I first tried, by adding these substances, to produce a liquid which should have some resemblance to milk. At a temperature of 30° Raumur, I added to the milky liquid, phosphate of soda, till all the supposed casein was dissolved. I then added butter and sugar in the proportions in which they are usually contained in milk, and, after the butter was melted, I shook the whole mixture in a flask. The liquid, at first greyish or dirty yellow, became of the colour of egg-flip, and, as it cooled, became more and more like milk. The clearer and the less mixed with red blood-corpuscles the serum was, the clearer was the solution and the whiter the milk-fluid. If the serum was perfectly free from blood-corpuscles, the emulsion, when diluted with water, had the same bluish colour as milk and water; but if there was a strong blood-red tinge in the serum, the solution in phosphate of soda had a reddish and the emulsion a weak yellowish tint. The product had a taste which bore a most striking resemblance to that of true milk; only it was a little more sweet, and left a feeble, though recognisable, bitter after-taste.

Under the microscope, I observed in the emulsion innumerable small globules, exactly like those in milk. On comparing them with those found in milk, scarcely any difference could be perceived, except that their average size in the artificial milk was somewhat greater than in the natural. The globules in the artificial milk were surrounded by a covering, and not simple drops of oil; this covering often showed a distinct though very fine folding, especially on the larger globules. The globules (like those of true milk) were not dissolved in ether, until the covering had been removed by treating them with nitric acid. But by the side of these small globules there were some which

were somewhat greater, and presented in their interior appearances which, if they had been furnished from the organised body, would have been certainly considered as nuclei, granular cell-contents. etc.

As the difficulties, which might have been expected to arise in the microscopic comparison of the artificial and the true milk, had so unexpectedly disappeared, I felt myself called on to inquire whether any more perfect imitation could be attained. The most striking difference, which the artificial milk presented on a superficial examination, was that it required a far shorter time than real milk to form a layer of cream on the surface, and that, after standing some time longer, a layer of clear liquid was formed at the bottom of the vessel. I ascribed this failure to the greater average size of the globules in the emulsion, as compared with true milk-globules. When I employed sugar of milk in place of common sugar, the emulsion was uniform, and showed no layer of clear liquid at the bottom of the vessel, even when more water was added. At the same time, the average size of the corpuscles became less, so that they could not be distinguished by their size from the true milk-globules, and they were now free from any appearance of nuclei or granular cell-contents. The cream was longer in rising to the surface; and the great sweetness which the emulsion had, when made with common sugar, was no longer prominent, so that the liquid tasted still more like milk. A slight smack of salt disappeared, when the butter was washed in water; and a slightly harsh taste of the butter was still farther removed when unsalted butter was used, which had been freshly churned, and washed with water. There was still a slightly bitter after-taste, but this was removed by using carbonate instead of phosphate of soda; a much smaller quantity of the former salt being required to produce the required solution. It was now impossible for me to recognise, by the taste, any difference between artificial and real milk. As, however, it might be said that "*de gustibus non est disputandum*," I submitted the liquid to the taste of others, with the same result.

It now only remained, to render the agreement perfect between the artificial and true milk, that the former should coagulate when brought into contact with the mucous membrane of the stomach. When I used common sugar, it was not wonderful that my attempts thus to make the liquid coagulate, failed; for it is well known, that the addition of sugar to milk retards or prevents its coagulation. But, even after I had used sugar of milk, I could not perfectly succeed in making the milk coagulate. Instead of fresh calf's stomach, I used the artificially prepared swine's stomach, which is commonly used in the

dairies, with or without a temperature of 40 R.; and I also gave the liquid to two kittens, who drank it greedily, and whom I then killed. Under these circumstances, as well as by mere standing, the artificial milk turned, and separated into a clear liquid, and a thick curdy mass, which had very strongly the well known smell of sour milk; but I could not produce a consistent coagulum, which could be collected in linen, and pressed out. But when the original substance separated from the serum, after being washed from soluble albumen, was warmed, it formed masses resembling pressed curds, and could be collected on a cloth, while the liquid ran through quite clear.

The identity of artificial with natural milk, and of the substance found in the blood with casein, may seem to be not proved; yet the resemblance is so great as to strengthen the probability of their being identical. I have in vain endeavored to produce from albumen, precipitated from albuminate of soda by acetic acid, and dissolved in phosphate or carbonate of soda, a liquid having the slightest resemblance in taste to milk.

[*London Journal of Medicine. Med. Examiner.*]

Diagnosis between Spasmodic Asthma, and other Affections which are accompanied by Asthmatic Paroxysms. By Dr. J. BERGSON.

I.—BETWEEN IT AND ANGINA PECTORIS.

A. *Symptoms in Common.*—The accessions coming on suddenly, with distress of breathing. The countenance, during the accessions, equally altered and anxious, the eyes prominent, and the cheeks colourless; the pulse weak and small.

B. *Distinguishing Symptoms.*

IN ANGINA PECTORIS.

1. The distress overpowering and accompanied by a feeling as of approaching death. The anxiety indescribable, and the seat of it under the sternum at the pit of the stomach.

2. The distress, however, violent, yet not interfering with respiration.

3. The paroxysms usually occurring in the day time, and coming on while the patient is walking, standing, or speaking.

IN ASTHMA.

1. The distress not so intense and not limited to any one point, but extending throughout the whole chest.

2. The breathing confined, and the constriction producing a sense of approaching suffocation.

3. The paroxysms almost always occurring at night, when the patient is in bed.

4. The respiratory motions never violent, and not visibly disturbed, except inasmuch as it may arise from the general state of distress. No peculiar dyspnœa, and the patient can often, by a strong effort, take a deep inspiration, even while the paroxysm is most severe.

5. The painful sensation which is at first confined to the cardiac region extending in all directions, and especially lodging in the left arm.

6. The heart's action but little changed. No alteration appreciable to auscultation or percussion.

7. The accession terminating with eructations or yawning, but with no vomiting.

4. The respiratory motions irregular and laborious; the thorax impelled up and down, but without having its capacity expanded. Hollows formed above and beneath the clavicles. The patient, even with the strongest efforts, cannot take a deep inspiration. Respiration seems at times to become almost impossible.

5. No such phenomenon occurring.

6. The action of the heart unchanged, but there is a sibilant ronehus, with failure of the respiratory murmur.

7. The accession terminating with copious bronchial secretion.

II.—BETWEEN IT AND SPASM OF THE THORAX (I. E. OF THE EXTERNAL MUSCLES CONCERNED IN RESPIRATION).

A. *Symptoms in Common* between asthma, which is a spasm of the bronchial tubes, and spasm of the external muscles, are, that in both respiration is affected suddenly and in paroxysms; the chest and shoulders are convulsively raised and depressed; and the difficulty of breathing approaches suffocation. The patients in both are unable to speak, but once the paroxysm has terminated, are then completely freed from all pain or obstruction in breathing.

B. *Distinguishing Symptoms.*

IN SPASM OF THE THORAX.

1. Respiratory murmur perfectly natural.

2. Dyspnœa present only in proportion to the degree in which inspiration is impeded by the spasmodic action of the external muscles.

3. The paroxysms mostly during the day.

4. The spasm never occupying more than one-half of the chest.

5. The spasm ceasing without any critical evacuation.

IN ASTHMA.

1. Respiratory murmur almost extinct.

2. Dyspnœa to such a degree that both inspiration and expiration become equally constrained, with a feeling of internal oppression.

3. The paroxysms at night.

4. Asthmatic oppression over the whole chest.

5. The paroxysm terminating with an abundant expectoration.

III.—BETWEEN IT AND PARALYSIS OF THE THORAX.

A. *Symptoms in Common.*—Dyspnœa coming on in a paroxysm at night during the first sleep. In both cases the patient complains of inability to expand the chest in respiration.

B. *Distinguishing Symptoms.*

IN PARALYSIS OF THE THORAX.

1. Some degree of deformity in the breast or back, from defect in the action of the nerves supplying these parts. Hence arise curvatures, inequality in the height of the shoulders, or chicken-breast.

2. Laborious respiration can be produced merely by pressing on the abdomen, which renders evident the deficient action of the muscles of the chest.

3. The most frequent subjects of the disease are delicate young persons before puberty, and it often arises from hooping cough.

IN ASTHMA.

1. No necessary deformity, except that the chest appears distended like a barrel.

2. During the intervals pressure on the abdomen produces no approach to asthma.

3. Asthma generally attacks men of strong constitution, and without any previous disease.

IV.—BETWEEN IT AND INTERCOSTAL NEURALGIA.

IN INTERCOSTAL NEURALGIA.

1. The pain is in the space between the ribs, and follows the course of the intercostal nerves from their origin at the vertebræ to their termination at the sternum.

2. Pressure at the origin of the affected nerve always produces pain.

3. The disease occurs especially in females, and principally in women of sensitive habit.

4. No morbid phenomena detected by auscultation or percussion.

IN ASTHMA.

1. The painful sensation diffused indefinitely over the whole chest, and not confined to the course of any nerve.

2. Pressure on the spine, unless spinal irritation be present, is unaccompanied by any painful sensation.

3. It mostly attacks men of robust constitution.

4. During the accession there is either a mucous or sibilant rouschus.

V.—BETWEEN IT AND HYPERÆSTHESIA OF THE PULMONARY PLEXUS.

Laennec distinguished this kind of asthma, in which, notwithstanding a sensation of inability of adequately breathing, which is felt in the highest degree, yet the expansion of the chest goes on well, and the respiratory murmur is not only undiminished, but rather assumes the character of puerile respi-

ration. This he designated as asthma with respiration. Along with Canstatt and Romberg (Berlin, 1846), our author is inclined to consider this asthma as a hyperæsthesia of the lungs, and as having its seat in the sensitive portion of the par vagum, in the same way as he considers spasmodic asthma to have its seat in the motor portion of the same nerve.

A. *Symptoms in Common*.—Accessions coming on suddenly and characterized by violent dyspnœa, incapacitating the patient from doing anything as long as it lasts. In both there are often catarrhal symptoms.

B. *Distinguishing Symptoms.*

IN HYPERÆSTHESIA OF THE PULMONARY PLEXUS.

1. The accessions occurring mostly in the day, and aggravated by any movement of the body.

2. Respiration goes on well, and on auscultation is discovered to be puerile.

3. The expression of the countenance unaltered.

4. Expectoration during the accession is easy, but yet not attended with any relief.

IN ASTHMA.

1. The accessions coming on at night, and relieved in some measure, by pressing down the arms.

2. The respiratory murmur is oppressed.

3. During the accession the countenance is contracted and anxious.

4. Expectoration not occurring till toward the termination of the accession, and always attended with relief.

VI.—BETWEEN IT AND RESPIRATORY PARALYSIS OF THE PAR VAGUM.

Respiratory paralysis was first pointed out by Romberg,* and he made two divisions of it, viz., the first paralysis of the vagum, which influences the respiratory muscles of the larynx; and the second, paralysis of the spinal nerves supplying the respiratory muscles of the trunk. The last is the paralysis of thorax of No. 3. The disease now before us is occasioned generally by pressure of indurated glands, or other tumours on the par vagum or some of its branches, and presents symptoms which may readily be mistaken for those of asthma.

A. *Symptoms in Common*.—Accessions coming on suddenly, and having intervals perfectly free from oppression. In the paroxysms the distress often amounts to approaching suffocation, with the formation of various bronchial sounds, while the respiratory murmur is enfeebled.

* De Paralyti Respiratoria, Berol, 1845.

B. *Distinguishing Symptoms.*

IN RESPIRATORY PARALYSIS OF THE PAR VAGUM.

1. The attacks come on after any considerable motion of the body, and cease as soon as the patient lies down again.

2. The voice in the course of the disease assumes a whispering or hoarse sound, or is reduced to complete aphonia.

3. The disease mostly attacks scrofulous children, arising from pressure of tumours in the neck.

IN ASTHMA.

1. The paroxysms coming on during sleep and when lying down, and are relieved by sitting up.

2. No change of the voice or of speech.

3. Occurring rarely, except in adult age, and without any traces of lymphatic swellings in the throat or neck.

VII.—BETWEEN IT AND NIGHTMARE (INCUBUS).

A. Floyer had already recognised a resemblance between asthma and nightmare, inasmuch as the latter comes on in sleep, and produces lacorous respiration with moans, the chest being compressed as by a superincumbent weight, and the patient in all the horrors of approaching suffocation; and Romberg, in his late work, has placed it amongst his hyponeuroses.

B. *Distinguishing Symptoms.*

IN NIGHTMARE.

1. As soon as the patient is completely awakened, all the feelings of approaching suffocation are at an end.

2. While in the fit, notwithstanding the greatest difficulty of breathing, yet the patient is unable to move his limbs or to call for help.

3. Auscultation detects no abnormal sound.

1. The asthmatic fit at once wakens the patient out of sleep, who, though completely roused up, yet has to seek the upright posture in order to breathe more freely.

2. The patient gets up, and is able to assume the easiest position.

3. The respiratory murmur is oppressed.

VIII.—BETWEEN IT AND PNEUMOTYPOSIS.

A. *Symptoms in Common.*—Pneumonia, bronchitis, and pleurisy, when occurring in an intermittent form, are included by our author under the one name, pneumotyposis, and in this form they agree in resembling asthma, by reason of the suddenness of their accessions, and from their recurring in paroxysms.

B. Distinguishing Symptoms.

IN PNEUMOTYPOSIS.

1. The paroxysm commencing with a rigor, and the oppression in the chest not decidedly manifest till the hot stage.

2. The paroxysm occurring at regular periods, and thus assuming the character of quotidian, tertian, &c.

3. Auscultation and percussion reveal the existence of pneumonia, bronchitis, or pleuritis, which still continues in some degree during the intervals.

IN ASTHMA.

1. The paroxysm commencing without any change of temperature, and the severest difficulty of breathing coming on from the beginning.

2. A much inferior tendency to regularity in the return of the paroxysms.

3. The symptoms to be detected by auscultation, and especially the mucous sounds coming on chiefly towards the end of the paroxysm.

IX.—BETWEEN IT AND SUBMUCOUS LARYNGITIS OR ŒDEMA OF THE GLOTTIS.

A. Symptoms in Common.—The patient is seized with a fit of suffocation, which soon attains its highest degree. The countenance becomes congested, the eyes starting from the orbits, and at the same time the extremities become cold; in short, presenting the appearance of asthma in its severest form.

B. Distinguishing Symptoms.

IN ŒDEMA OF THE GLOTTIS.

1. Inspiration produces no painful sensation in the chest, but only in the larynx, to which the patient refers all his difficulty of breathing.

2. Every effort to draw in the breath is accompanied by a wheezing sound.

3. Expiration goes on uninterruptedly.

4. The patient thinks there is some large foreign body in his larynx, which he would gladly get rid of.

5. The suffocative attack lasts only from two to four minutes.

6. It returns in from five to ten minutes.

7. The cough is dry and of a croupy sound.

IN ASTHMA.

1. The impediment to inspiration is proved to be not so much in the larynx as in the chest.

2. Inspiration takes place without any peculiar sound in the larynx.

3. Expiration and inspiration are equally difficult.

4. No sensation as of a foreign body in the larynx.

5. The asthmatic attack lasts several hours.

6. It returns at the utmost in about every twenty-four hours.

7. The cough moist and with the usual sound.

[*Dublin Journal.*

Coup de Soleil or Sun-Stroke.

In "a summary of the transactions of the Coll. of Phys. of Philadelphia," published in the last number of the American Journal of Medical Sciences, are some remarks by Dr. Pepper, one of the physicians of the Pennsylvania Hospital, on the above disease. "He considered it a remarkable circumstance that this affection has received so little attention from medical writers." "In consulting the standard authorities, we find but little said in reference, and that generally vague and unsatisfactory." This fact is, in our opinion, easily accounted for. The disease seldom occurs except in crowded communities, as in large cities; and in these, only among a particular class, the common laborers, who earn their daily bread by their daily work, and are consequently compelled, usually, to labor during the intensest heat of the day, when the thermometer, in the shade, ranges from 96 to 100 degrees and over, with not only the direct rays of the sun playing full upon them, but also the reflected rays from pavements and buildings. Added to this, as the efficient and exciting cause, we have fatigue, intemperance, and often insufficient or improper food as predisposing agencies; these causes do not usually exist in the country, and in small communities, where, if labor is performed in the heat of the day, and under exposure to the sun, it is with a supply of fresh, wholesome air, with none of the other predisposing causes; the powers of the constitution will, under such circumstances, generally resist its baleful influence. The members of this class, when any accident befalls them, are almost always conveyed immediately to the hospital; and therefore it is rather rare for a private physician to be called to treat a single case of coup de soleil, and of course nothing can be furnished by him on the subject in the way of practical experience. When brought to the hospital, it is generally at an hour when the attending physician is absent, and the case usually dies before his next visit, or is so far recovered as not to call for his particular notice, so that he knows but little personally of the phenomena of the disease. It is thus only seen by the resident physician, who, in the discharge of his multitudinous duties takes no particular note of the symptoms or history of the case, but sees the patient die in a few hours, perhaps in a few minutes, after his admission, and thinks no more of it.

Private physicians are sometimes called suddenly in the heat of the summer, to a man who has "fallen down in a fit, while at work," and regarding the case as one of apoplexy, he pulls out his lancet, bleeds him and sends him to the hospital. This is almost universally the practice. Dr. Pepper says, of twenty

hospital patients, *all* had been bled previous to admission. This fact is, of itself, a strong indication that some knowledge of pathology of the disease is much needed in the medical community ; for it is well known to those who have had much experience in this disease, that venesection, if it succeeds, is almost certain death.

In this city, the disease is quite common in the months of July and August, commencing sometimes as early as the middle of June and ending as late as the first week in September. In the summer of 1847, if I remember rightly, there were thirty-seven cases in four days. Most of them died so promptly, that there was not time to convey them to the hospital, the coroner being usually the only physician who saw them. Not only were men affected, but animals, omnibus horses especially, it being quite common to see them fall and die in the street. During the last five years, according to our records, forty-two cases were admitted into the hospital. Of these, twenty-four died, and eighteen survived. Fourteen occurred in the month of August, twelve in July, twelve in June, and four in September.

The prognosis in this disease, as in cholera, depends almost entirely upon the stage in which the disease is seen. If in the stage of collapse, the stage is almost hopeless. So that one physician might have ten cases, and all might recover ; another might have the same number, and the treatment be equally judicious, yet nine out of the ten might die.

Nearly half of the cases that have been brought to this hospital, as far as my own experience extends, have been in the stage of collapse, or bordering upon it. They were usually brought in late in the afternoon, and, of course, some hours after the inception of the attack. They have then been comatose, with cold surface, except that of the head, which is often very hot, feeble, frequent and fluttering pulse; scarcely perceptible at the wrist, dilated and inactive pupils, respiration labored, sometimes stertorous. Sometimes they have lain perfectly motionless and paralyzed, sometimes restless, sometimes in convulsions. Often, when in this state, under the application of a powerful stimulus to the surface, as burning alcohol to the legs, a patient has sprung up in bed, stared at those around him for a moment, asked for a drink, taken it, and then fallen back again into his former condition. In a less advanced or less severe stage of the disease, the patient has presented pretty much the same symptoms, but in a less marked degree. The pulse is frequent but not so feeble and irregular, the pupils act feebly, the surface is cool, the head perhaps burning hot ; patient is perhaps in a state of partial coma, from which he can be aroused, however, by addressing him by name, in a loud tone ; the

respiration is quick and labored, but not stertorous; sometimes he has convulsions, quasi epileptic; sometimes he is extremely restless, requiring to be held in bed. In a still earlier or less severe stage, the patient is perhaps able to walk with assistance, complains of intense pain in the head, which is usually hot. The extremities are cool; pulse not much altered, not hard or bounding; no infection of eyes; pupils rather dilated, if altered at all.

Perhaps one or two cases by way of illustration, and briefly stated, would not be amiss here.

C. 1st. A man, name unknown, about forty years of age, was brought to the hospital about noon, July 27th, 1848, and admitted under Dr. H. D. Bulkley. He had fallen in the street a short time previous to admission. Was in a state of complete coma, with labored and irregular respiration, quick and fluttering pulse, head hot, pupils immovably dilated.

Treatment.—Sinapisms to feet, legs and stomach. Ice to head and stimulants. He survived but a few hours.

Autopsy, eighteen hours after death.—Brain normal: lungs slightly congested—crepitant. Other internal organs healthy.

C. 2d. Michael Collyer, native of Ireland, laborer, was admitted under Dr. Griscom, Aug. 11th, 1848, in a state of insensibility, having fallen down in the street, respiration stertorous, pupils dilated, pulse quick, feeble and irregular.

Treatment.—Turpentine enema. Sinapisms to chest and limbs—stimulants freely. Patient survived but a short time.

Autopsy, eighteen hours after death.—Brain slightly congested; lungs emphysematous at some points; other organs healthy.

In these cases, as has been seen, there was no marked congestion of the internal organs. In nearly all the cases that occurred in 1850, there was well-marked congestion of these organs; sometimes of the lungs, sometimes of the brain. Thus, out of eight cases recorded in our books, there was congestion of the lungs in two, and of the brain in four. In one of the remaining two, there was apoplexy, and this man was bled in the hospital, being the only case out of the forty-two in which the lancet was considered admissible; the case proved fatal.

In the remaining case, there were well marked epileptic convulsions; this case, terminated favorably. The congestion of the brain in two of the four cases was inferred from the symptoms as the cases recovered. In the others, it was revealed by a post mortem inspection. In no case was inflammation of the brain or its membrane observed; and in all the cases the same course of treatment was pursued, with the above mentioned exception. Cups were applied to the temples in the

cases suffering from head symptoms, such as heat, dilated pupils, stertorous breathing, pain, but external and internal stimulation of the most active kind was indicated in all, except perhaps in those admitted in the first degree or stage of the disease. Sometimes the patient was placed in the warm bath, and at the same time, the cold douch was applied to the head; this usually seemed to have some effect, though but temporary. In the cases which showed congestion of the brain, at the autopsy, the symptoms were still such as to require prompt stimulation, the only difference in the treatment being the local abstraction of blood from the temples, and the application of ice to the head. I knew of one case in private practice in the year 1847, which occurred in a high liver, of apoplectic build, and showed marked symptoms of apoplexy. The attack yielded with some difficulty to large bleedings.

Insolation is almost uniformly nervous exhaustion, and is to be treated as such. We are not to bleed because the patient is a robust man, and has fallen in a fit at his work, which seems to be the only circumstance taken into consideration usually by the physician who is hurriedly summoned to such a case. The pulse is always a sure guide.—[*New York Journal of Medicine*.

The Treatment of Erysipelas by the Muriated Tincture of Iron.
By G. HAMILTON BELL, Surgeon, F.S.C.S.E.

It will be generally admitted that practical observations on the treatment of diseases of daily occurrence are more valuable to the medical practitioner than the most interesting descriptions of anomalous cases, however extraordinary in their character, or successful in their treatment. I am convinced, indeed, that the publication of the journals of well-employed medical men, giving in detail the treatment of every case occurring in their private practice, would prove most useful to the young practitioner, and be a valuable contribution to our medical literature.

In furtherance of such views, I am anxious to bring to the knowledge of my professional brethren a mode of treating erysipelas, differing from that usually resorted to, but which I have found invariably successful. I have no intention, however, of writing a treatise on a disease so well known, and on which so much has been recently published, because I think my object will be fully attained by reporting a few cases from my own journals, illustrative of my experience.

My purpose, then, being purely practical, it would be out of

place to premise with a disquisition on the nature and causes of inflammation; but in order to explain in some measure the principle by which I have been actuated in employing a powerful tonic in a disease generally occasioning so much fever and cerebral excitement as erysipelas, I consider it necessary to repeat the opinion I have elsewhere expressed—viz., that “in inflammation, the capillary vessels having apparently lost the power of separating or electing the component parts of the blood which are necessary for functional purposes, and become to a certain extent inert tubes, a stream of blood is admitted, for the circulation of which they are not calculated.” In other words, I consider that in erysipelas the capillary vessels are in an atonic state.

This hypothesis appears to me to be supported by the effect of the treatment I have adopted in erysipelas—the cases demonstrating that when an extensive portion of the surface of the body is violently inflamed, producing a high degree of fever and cerebral excitement; on the system being rapidly surcharged with, or brought under the influence of, the muriated tincture of iron, while the cerebral affection and other symptoms of fever subside, the local pain is relieved, and the redness and swelling gradually disappear; and, so far as the tonic medicine appears to be concerned, all this is effected without any appreciable evacuation from the emunctories of the system.

But refraining from theory or speculation, and in the hope that I shall lead to the general adoption of the practice I have suggested, and which I shall illustrate by a few cases, I shall proceed at once to state shortly the mode of treatment I have resorted to, in every case of erysipelas I have attended for upwards of a quarter of a century, without having in a single instance failed of success. I have found that my remedy is not only effectual in removing erysipelas, but that it generally renders the patient more healthy and robust than before the attack of the disease; and in no instance in which I have had the charge from the commencement of the disease, has suppuration taken place. I have for a series of years pressed privately on the attention of the senior members of the profession the value of this remedy in the treatment of this always troublesome and often fatal disease; and I have taught many of my junior brethren successfully to combat it. But my prolonged experience of the invariable success of my practice, justifies me, I think, in thus bringing it to the notice of the profession generally, trusting that, in the hands of others, it may prove as great a blessing as it has in mine.

Mode of administering the Remedy.—Of course the first object is to have the bowels freely acted on. If the erysipelas

be mild, fifteen drops of the muriated tincture of iron are administered in water every two hours until the disease is completely removed. When the attack threatens to be more severe, the dose of the tincture is increased to twenty-five drops every two hours, and persevered in night and day, however high the fever and delirium. The only local applications I ever find necessary, are hair powder and cotton wadding. While I depend for the removal of the disease on the chalybeate, it is necessary that the bowels should be attended to throughout the treatment.

I have in my journals several cases in which erysipelas was combined with gout. Two of these occurred in gouty subjects, and in both the health was restored by the chalybeate, as in the above case of Mr. C. This gentleman had, two years ago, an attack of erysipelas in his thigh, and was cured by the steel drops. He never had gout before, but I understand his father had suffered from it.

With regard to the diet of the patient under erysipelas, unless when there is much fever, I always recommend it to be generous, and of easy digestion. In a case at present convalescent, in which the attack was very similar to Mr. C.'s, while the great toe and foot were violently inflamed, the knee of the other leg was also affected. From the habits of the patient—a constitution broken from intemperance—I was obliged to allow, along with the drops, nearly a bottle of port a-day. In this case the podagral erysipelas has disappeared, and my patient is in better health than he has had for many months.—*[Edinburgh Monthly Jour. of Med. Science. Boston Med. and Sur. Jour.]*

Æthereal Solution of Iodine.

I beg to offer to your notice a preparation of iodine, which is as yet unknown to the profession, except to a few in this locality whose attention I have directed to its efficiency as a counter-irritant. I have employed it in my practice for upwards of ten years, and generally with the most satisfactory results, in the most of those cases where the use of the tincture is commonly indicated. It is applied in the same way as the tincture, by means of a camel-hair pencil rubbed over the part, until it begins to produce a burning sensation in the part; then cover it with a pledget of wadding, so as to prevent evaporation. For the first fifteen minutes the burning sensation is pretty severe, so as to alarm some patients. Yet it soon becomes tolerable, but usually continues to be felt for several hours. The next day the cuticle has a dry hardened feel, having the iodine color;

and great relief to deep-seated pain is obtained. In the course of two, three or four days, vesication will be observed around the edges of the superficial eschar which has now commenced to suppurate; and as the destroyed cuticle cleans off, a very copious discharge of purulent matter takes place, and may be kept up for two or three weeks under the popular application of a cabbage leaf, or oiled silk, which I usually apply on the second day. The surface of the sore assumes a fine granular appearance, and heals without leaving a cicatrix. I have often thought that, in cases of chronic inflammation of the joints, this application is more efficient than the caustic issue, relieves pain quicker, and can sooner be repeated.

I have frequently derived great benefit from keeping up a discharge from the chest in chronic affections of the lungs, making a sore the size of a quarter or half dollar at a time, and opening a new sore as the other heals.

This solution is very simply prepared. I commonly use the sulphuric ether of the shops; but the stronger the ether, the more efficient is the preparation. Hence the importance of obtaining a good article and in full strength.

I commonly put a quantity of pure iodine into a phial, and add sulphuric ether until dissolved; that is, the ether must be perfectly saturated. To make the solution as strong as possible, I have added a few grains of the iodide of potassium, which furthers the capability of the ether to take up more of the iodine. There are different modes by which this can be prepared, that will be readily suggested to your several readers. All of them, however, will tend to the same result.

In some cases it may be used at a reduced strength, according to the amount of counter-irritation or stimulation which individual cases may seem to require.

I am yours truly,

ROBERT THOMPSON.

Dover, N. H., June 27, 1851. [Bos. Med. and Sur. Jour.

Creasote in Diarrhœa.

M. Kesteven extols (London Med. Gaz., Feb. 1851) the efficacy of creasote in the cure of diarrhœa. The form in which he used it was: R.—Creasoti \mathfrak{m} j to \mathfrak{m} v; Spt. ammon. arom. \mathfrak{m} xv to \mathfrak{z} j; Aq. \mathfrak{z} j to \mathfrak{z} iss. Where pain has been severe, Tinc. camph. co. has been added.

In no single case, Mr. K. says, has creasote failed to be of signal benefit; in most cases one single dose was sufficient to arrest the course of the disease; in very few instances has it been requisite to administer more than the second dose.

[Amer. Jour. Med. Sciences.

Miscellany.

Value of Vaccination.—Dr. GUY, in his interesting lectures on public health, now in course of publication in the *Medical Times*, has brought forward the following striking statements illustrative of the great value of vaccination, and of the vast saving of human life and human suffering it has been the means of effecting :

In the history of small-pox, we have three different periods to compare with each other: a period anterior to the introduction of inoculation ; a period during which inoculation was practiced · and a period during which inoculation was superseded by vaccination. As inoculation was introduced into England in 1721, the deaths from small-pox during the ten years ending 1719 will fairly represent the mortality occasioned by small-pox, unmodified by the practice of inoculation. Now, as I have already stated, the practice of inoculation did not gain much ground till towards the middle of the 18th century, and did not become general in England till the last quarter of that century. Hence the ten years from 1740 to 1749, inclusive, will correspond to the introduction of inoculation into partial use ; and the ten years from 1790 to 1799, inclusive, will mark the time when it was largely and generally practiced. In like manner, the ten years from 1810 to 1819, inclusive, will represent a period during which inoculation was to a great extent, though by no means altogether, superseded by vaccination. The ten years from 1840 to 1849, inclusive, will represent a period when small-pox inoculation, having fallen into entire disuse, and been even declared an illegal practice, vaccination has come to be the only preventive of small-pox employed or permitted. It must not, however, be forgotten, that large numbers of persons still remain unvaccinated, though the greatest possible facilities are offered for the performance of the operation, and that without charge to the poor.

I have arranged the deaths by small-pox for the five periods of ten years each to which I have just referred, in a tabular form, so that the numbers may admit of easy comparison.

	Deaths from Small-pox.	Estimated Population within the Bills of Mortality (limits in 1726.)	Deaths from Small-pox in a million inhabitants.
1. Ten years ending 1719, before inoculation	21,228	675,691	31,416
2. Ten years ending 1749, inoculation partially practiced	20,029	708,188	28,282
3. Ten years ending 1799, inoculation in general use	17,685	773,344	22,863
4. Ten years ending 1819, vaccination in general use	8,334	1,035,865	8,045
5. Ten years ending 1849, inoculation superseded by vaccination (whole me- tropolis).....	9,174	1,912,172	4,798

A small decrease in the number of deaths from small-pox, coincident with the partial practice of inoculation; a still more marked decrease under the more general use of that palliative; but a far more remarkable falling off in the number of deaths from small-pox, concurrently with the introduction and extension of vaccination: such are the results stamped on the very face of the table which you have before you,—results fully borne out by Mr. Farr, who says that “In 1771 to 1780, not less than 5 in 1000 died annually from small-pox; while in 1801 to 1810, the mortality sank to 2; and in 1831 to 1835, to 0.83,” or less than 1 in 1000.

The decrease in the number of deaths from small-pox in the second and third periods of ten years, is a circumstance in favor of the views of the supporters of inoculation, who affirm that that practice, though objectionable in one point of view, was on the whole beneficial.

That inoculation may really have been the means of saving life to some extent does not seem altogether improbable, when we bear in mind that in place of the mortality of *one in four*, which attended the small-pox caught in the usual way, the loss under inoculation, when skilfully and carefully practiced, did not exceed *one in 500*. So that it is quite possible that the mischief which inoculation wrought by spreading the disease among those who might never have caught the natural small-pox, was counteracted by the good it effected in reducing the danger to a very insignificant amount. But even admitting the justice of this compensation, the benefit really due to inoculation does not exceed the saving of 8500 lives in 31,500; whereas the boon conferred by vaccination is represented by a reduction of the mortality from nearly 23,000 in the ten years ending 1799 (the very heyday of inoculation) to 8000, and then to less than 5000, being a saving of life more than twice as great as that effected by inoculation.

But the superiority of vaccination over inoculation is only partially and imperfectly represented by the greater saving of life which it effects. It has the great additional merit of requiring no preparation or confinement, of producing very trifling indisposition, and of entailing no danger. Above all, it is free from the great objection to inoculation, that, while it mitigates the disease in the individual, it brings the infection home to those who may happen to be unprotected, and who might never have been exposed to the contagion of the natural small-pox.

Taking all these considerations into the account, there can be no doubt that the legislature was perfectly justified in rendering the performance of inoculation a legal offence, and in giving, by the appointment of public vaccinators, to be remunerated by a moderate fixed charge, large facilities for carrying the blessing of vaccination to the very doors of the poor.

But, undisputed as is the superiority of vaccination, and undoubted as are the benefits it has conferred, they are small indeed compared to those which it was calculated to bestow if the practice of it had been as universal as it ought to have been. It was the cherished opinion of Jenner himself that we possess in vaccination a means of

entirely eradicating the small-pox, and some facts are on record which seem fully to bear him out in that view of the case. Thus, Sir Gilbert Blane tells us that he was informed, on good authority, "that vaccination had been practiced with so much energy and success in Lima that for the last twelve months there had occurred not only no deaths from, but no cases of small-pox; that the new-born children of all ranks are carried as regularly to the vaccination house as to the font of baptism; that the small-pox is entirely extinguished all over Peru, nearly so in Chili, and that there has been no compulsory interference on the part of the Government to promote vaccination." Sir Gilbert Blane goes on to say, "It is now matter of irrefragable historical evidence, that vaccination possesses powers adequate to the great end proposed by its meritorious discoverer in his first promulgation of it in 1798, namely, the total extirpation of small-pox. The first proof of this was at Vienna, where, in 1804, no cases occurred except two strangers, who came into the city with the disease upon them. In 1805, there did not occur a single death from it in Copenhagen."

I quote this more detailed account of the progress of the small-pox in Denmark from a MS. of Jenner's, published by Dr. Baron in his *Life of Jenner*. "From the year 1762 to 1792, the number that died of small-pox in the Danish dominions amounted to 9728. About the year 1802, vaccination was first introduced, and the practice became general, but not universal; however, fifty-eight persons only died of the small pox to the year 1810. Vaccination, by command of the king, was now universally adopted, and small-pox inoculation prohibited. And from the year 1810 to the year 1819, not a single case of small-pox has occurred." From the same MS. I quote the following passage: "From Bombay, I learn the small-pox is there completely subdued, not a single case having occurred for the last two years."

"Dr. Sacco, the indefatigable superintendent of vaccination, in Lombardy (I resume my quotation from Sir Gilbert Blane), stated in his annual report, 3d of January, 1808, that small-pox had entirely disappeared in all the large towns in that country; and that in the great city of Milan it had not appeared for several years. Dr. Odier, of Geneva, so favorably known for his high professional, scientific, and literary acquirements, testifies that, after a vigorous perseverance in vaccination for six years, the small pox had disappeared in that city and the whole surrounding districts, and that, when casually introduced by strangers, it did not spread, the inhabitants not being susceptible. The central committee in Paris testify, in their report of 1809, that the small pox had been extinguished at Lyons and other districts of France." "These (I still quote from Sir Gilbert Blane) are selected as some of the earliest and most remarkable proofs of the extirpating power. But it is demonstrable that if, at the first moment of this singular discovery, at any moment since, at the present or any future moment, mankind were sufficiently wise and decided to vaccinate the whole of the human species who have not gone through the small-pox, from that moment would this most loathsome

and afflicting of all the scourges of humanity be instantaneously and forever banished from the earth." If for this somewhat too enthusiastic view of the case, we substitute (what Sir Gilbert Blane probably intended to affirm) that if in any way it could be brought about that every man, woman, and child, for a term of years, could be vaccinated, small-pox would gradually die out, and ultimately disappear, like a fire extinguished for want of fuel, we should be stating what all experience and analogy seem to warrant. In our own country, we have not, that I am aware, had any proof afforded us of the extirpating power of vaccination. But we have had some striking and encouraging illustrations of the power of vaccination when systematically practiced. The army affords us one such instance. In Her Majesty's dragoon guards and dragoon regiments, which, in common with the rest of the army, are submitted to inspection, and vaccinated without exception, if that operation is found to have been omitted, "the deaths from small-pox during a period of seven and a quarter years, in an average annual strength of 6165 men, were only *three*—a proportion which would represent an annual mortality of 1 in 14,900 adult males!"

The value of vaccination is shown in a very remarkable manner by the experience of certain severe epidemics of small-pox, which have taken place since vaccination came into general use in this country. There was such an epidemic in Scotland in 1818-1819, when a careful analysis of the cases showed that, out of 205 cases occurring in persons unprotected either by vaccination or previous small pox, 50 died, being one death in every *four* cases; out of 71 cases in which small-pox had occurred a second time, there were *three* deaths, or one in 23; while out of 310 cases, occurring after vaccination, one only died! In this instance, then, the protecting power of vaccination proved to be more than 13 times as great as the protecting power of small-pox itself. A similar result was established in the epidemic of small-pox which occurred at Marseilles in 1828. It was estimated that the population consisted of 8000 *unprotected* persons; of 2000 who had been *inoculated*, and of 30,000 who had been *vaccinated*. Now, of these 8000 unprotected persons, 4000, or one-half were attacked by small pox; of the 2000 who had been inoculated, 20 were attacked, being 1 in 100; and of the 30,000 who had been vaccinated, 2000, or 1 in 15, were attacked. The mortality of the several classes was as follows: Of the 4000 unprotected persons, 1000, or 1 in 4, died; of the 20 inoculated persons, 4, or 1 in 5; of the 2000 vaccinated persons, 20, or 1 in 100! It would seem, then, that the practice of vaccination is less effectual in preventing small-pox than that of inoculation; but that at the same time, the disease, when it does occur after vaccination, is of so mild a character as to destroy only 1 life for every 20 sacrificed by small pox occurring after inoculation.

The relative value of vaccination and inoculation, and the proportional risk of the protected and unprotected, will be best seen by supposing 15,000 unprotected, inoculated, and vaccinated, persons, respectively, to be exposed to the danger of catching small-pox. The

result as regards these several parties are shown in the annexed table :—

	Unprotected.	Inoculated.	Vaccinated.
	15000	15000	15000
Attacks.....	7500	150	1000
Deaths.....	1875	30	10
Ratio of attacks.....	1 in 2	1 in 100	1 in 15
Ratio of death to attacks	1 in 4	1 in 5	1 in 100

In order to form a just idea of the superiority of vaccination, we must not forget to add to the lives saved by it, the blindness, deformity and lingering debility which it is the means of preventing in those cases which do not prove fatal; for it may safely be assumed that these untoward consequences bear a certain relation to the fatality of the disease, being of more frequent occurrence where the mortality is highest.—[*American Journal of Medical Sciences*.

Congenital Variola in Twins. By JAMES AYER, M. D., Boston.—I was called, in haste, last week, to Mrs. P., and found her lying on the bed, in great pain. On examination, the head of a small fœtus was found born. The uterine contractions were active, and its full delivery effected in a moment, attended by a feeble cry. The pains continued, a bag of fluid was felt protruding, and soon a second fœtus was expelled dead. Two separate placentæ were afterwards removed, and the patient made comfortable.

The infants were found of the size and developement of six months. The living one had a dozen or more of pustules, on the face, head and breast; one or two were noticed on the abdomen, but none on the limbs. Three or four were good sized, plump and well-defined pustules of small-pox. The remainder were not so full, but evidently of the same character. This one survived its birth two hours.

The dead child had no offensive odor; the abdomen was dark purple, and the cuticle quite loose. Its whole body, especially the abdomen, was marked with depressions, similar to those of variola in infants, after death. No elevations or pustules were noticed; these marks only remained.

Three weeks before the abortion, the mother, I was informed, had broken out with varioloid, after the usual premonitory symptoms, and had just recovered when I saw her. The disease was so mild that a physician was not called. She could not trace her miscarriage to any over exertion, or any cause, except the attack of varioloid. Whether the mother infected the two at the same period, and the death of one caused the expulsion of both: or one had the disease first, and the second received it from him, are questions of some interest, but difficult, from the evidence, to decide.—[*Boston Med. and Surg. Journ.*

Result of the Use of Chloroform in 9000 Cases at Bartholomew's Hospital. By Mr. SKEY.—One of the most interesting questions connected with the subject of operative surgery relates to the use of anesthetic agents employed for the purpose of suspending the function

of sensation. This question has assumed a moral as well as a medical type. It has been urged, that sensation is a natural function of the living organism, and that to suspend it by artificial agency, is to set at nought the ordinances of nature ; and that man is born to suffering, as evidenced by the sensibilities of his body. If the soundness of this argument be admitted, it would be difficult to draw a line which would define the boundary at which moral and *immoral* suffering meet ; or to say, in what form of suffering our remedial agents may be justifiably resorted to. The sensibilities of our frame are not given us by nature to the end of promoting pain, but to enable us to avoid it. Corporal suffering is no part of the discipline of the mind ; nor can it even be generally asserted that its excess exercises a salutary influence on the character. Every movement of our body instinctively points to the avoidance of bodily suffering ; why, therefore, should we not as readily and unobjectionably employ the agency of anesthetic medicines for the purpose of suspending bodily pain, under the circumstances of an otherwise painful operation, as we endeavor to mitigate the bodily suffering of any other patient cast down on a bed of sickness ? Will not the objection to the anesthetic action of opium to a region affected by a neuralgic pain, or to the system generally, hold as strongly as that of another agent of the same principle given to avert the pain of an operation ?

The medical arguments against the use of anesthetic agents have a somewhat better foundation. That great and sudden determination to the brain, and an unnatural circulation of venous blood, result from their employment, is undeniable.

It is undeniable, if the quantity administered be large, and long continued, that symptoms resembling those of apoplexy present themselves, in the form of extreme congestion of the vessels of the face, stertorous respiration, and total insensibility ; and it cannot be denied, that occasionally its full administration leads to headache, vertigo, and languor of some days duration ; and cases are recorded in which death itself has followed in the course of an hour or more after its employment. It must be observed, however, in pursuing this question in strict accordance with the laws of evidence, that we have no *proof*, in the cases above referred to, that death was the direct effect of the supposed cause. The parties administering it were not fully experienced in the mode of its application. They entertain the *opinion* that death was referable to it, while it cannot be disputed that the fatal issue may be attributable to other causes ; and, in one example, it appears more reasonable to refer the death of the individual to a suspension of the function of respiration by violence, than to any obnoxious agent circulating through the lungs or brain. On the other hand, the records of St. Bartholomew's Hospital point to its successful administration in upwards of 9000 cases ; in not one of which, including the aged and the young, the healthy, the infirm, and the asthmatic, has its employment left a stain on its character, as an innocuous agent of good. Under all circumstances, its careful employment may be unhesitatingly resorted to in all cases, excepting only such as are marked by

determination to the brain of an apoplectic type; secondly, under circumstances of great and serious exhaustion from loss of blood; and, thirdly, in diseases of the heart. In these conditions of the system, it is perhaps better avoided.

The agent in general use is chloroform and one word may be added as to its administration. It appears indisputable, that its influence on sensation precedes that on consciousness. I have employed it on several occasions, in which a patient has been conscious of all that has been passing around, and yet who has declared himself to have been totally insensible to pain. This state of his system has arisen from the moderate use of the agent, ample, indeed, for all purposes of utility, though somewhat difficult to regulate in quantity sufficient for the required object.

I prefer its gradual administration. I do not think it desirable to exclude atmospheric air, employed as a diluent during the process of inhalation. Its influence should be gradual, not sudden. I consider its application through the medium of a cambric handkerchief laid on the face, preferable to the use of instruments made for the purpose of excluding atmospheric air, and food should be rigidly avoided before its administration, otherwise sickness will frequently follow.

Against the occasional convictions or objections of others to its employment, I place the strong, and to my own mind the unanswerable fact, that it has been successfully used in so large a number of cases in St. Bartholomew's Hospital, since the period of its introduction; that these cases have been indiscriminately taken, and that its objections have not yet made their appearance before the observant eyes of the medical staff of that institution, either by promoting danger during the operation, or protracting the recovery of the patient after it. In one class of cases its employment is especially applicable, viz., in that form of disease in which the pain of an operation is the chief warrant for its non-performance, and in which the recovery from a chronic disease is left to nature, that might be greatly hastened by the hand of art; such, for example, as the removal of a piece of dead bone.

Up to the period of the introduction of chloroform, a surgeon was very unwilling to subject a patient to the painful process of sawing and chipping away portions of dead bone, with a view to reach the medullary cavity, because the operation was both a painful and protracted one. The consequence was, that an hospital bed was occupied by a patient thus affected, for many months, to the exclusion, perhaps, of three or more claimants, who would have successively occupied it. But by the aid of chloroform the operation is now performed unconsciously to the patient, and the period of his recovery greatly abridged. With the three exceptions above mentioned, I cannot hesitate in strongly recommending its administration in all cases of large surgical operations; believing its discovery to be the greatest blessing conferred on the profession of surgery during the last century; and although I have seen its employment pushed, on many occasions, apparently to the verge of apoplexy, I cannot say, even in such examples, that the good has not largely predominated.—[*Operative Surgery. Buffalo Medical Journal.*]

On a Successful Case of Transfusion. By G. BELLASIS MASFEN, Esq., House Surgeon to the Staffordshire Infirmary.—On the 30th of July, 1848, at one P.M., I was called in to attend Mrs. B——, a lady of particularly delicate appearance, in her thirty-eighth year. It appears that on the evening of the 29th she had perceived some slight sanguineous discharge from the vagina, and had consulted my father, to whom she described herself as being four months advanced in her tenth pregnancy, but thought that the child had not grown for the last month or two. He ordered a mixture containing diluted sulphuric acid with Batley's sedative; but the discharge continued to increase until about seven o'clock this morning, when it became quite alarming. Plugging and injections of oak-bark were tried, but with no effect, and a dose of ergot was administered, which produced a severe pain, and the expulsion of a two-months' fœtus; but the hæmorrhage continued to increase till one P.M., when I first saw her.

I found her excessively weak, from loss of blood; not the slightest pulse was to be felt at the wrist; and she became at last insensible. The stomach rejected everything, and though the hæmorrhage had in a great measure stopped, there was every symptom of sinking and speedy dissolution.

About three o'clock, it being the opinion of every one present that it was the only possible means of saving her life, the operation of transfusion was decided upon, which I performed in the presence of Dr. Knight and my father. I immersed a four-ounce brass syringe in water at the temperature of 110° Fahr., and drew a full stream of blood into it from the arm of a stout buxom-looking servant-maid. This I injected into a vein on the left arm, taking every precaution to prevent the admission of any air bubbles. As the operation was going on consciousness appeared to be somewhat roused, and the pulse became slightly perceptible at the other arm, but in the course of half an hour the pulse had again disappeared, and she remained still unconscious. I then a second time injected three ounces of blood into the right arm, (the veins were so small and empty that there was difficulty in finding the same opening twice;) this was again attended with a return of pulse and sensibility, which, however, gradually disappeared as before. After an interval of nearly an hour, I injected a third three ounces of blood, which produced more permanent good effects: the pulse gradually rose as the injection went on, color made its appearance in her face, and she inquired if we had been bleeding her. During the evening she complained much of thirst and she had occasionally a teaspoonful of wine-and water.—Eight P.M.: The pulse was slightly perceptible, but was not to be counted; she attempted to take a cup of tea, but it was immediately rejected, as was also even a teaspoonful of water, and she remained all night awake and thirsty, but afraid to drink even a little water.

31st.—Six A.M.: the pulse was 150, and very much increased in strength; the tongue dark brown, hard, and dry. Ordered three drops of creosote in form of a pill. She vomited almost immediately after taking it but did not throw up the pill, which from that time appear-

ed to allay the sickness. She then took a table-spoonful of brandy-mixture every hour. In the evening she still complained of thirst, and was ordered the following mixture ;—Sesquicarbonate of soda, two and a half drachms ; sesquicarbonate of ammonia, half a drachm ; compound tincture of cardamoms, two drachms ; oil of lemon, six drops ; distilled-water to six ounces.—Two table-spoonfuls to be taken every three or four hours in a state of effervescence, with twelve grains of citric acid. There was a great extravasation of blood for six or eight inches above and below the elbow in both arms, probably the effect of the injection. Ordered warm-water dressing.

Aug. 1st & 2d —She continued gradually improving in appearance ; her pulse was slower, and she was better able to take slight nourishment. The arms were becoming more ecchymosed, and she complained of great pain in them. The warm-water dressing was continued.

3rd.—Her health is gradually improving, and she is taking no medicine ; complains of great pain in the right arm, which was much inflamed, and very hard just below the elbow, and seemed likely to suppurate. Ordered castor-oil and the water-dressing.

4th.—The arms rather better ; the swelling abated.

5th.—Continues to improve, both in health and as regards her arms. Ordered tincture of sesquichloride of iron, one drachm ; infusion of quassia and camphor mixture, of each three ounces ; to take two table-spoonfuls three times a day.

14th.—The arms have been gradually improving, and the discoloration is nearly gone, but they remain very weak, and she is not able to write.

28th.—She has now quite recovered the use of her arms, and is in general good health. From this time I discontinued attendance.

In June, 1849, she miscarried again, but otherwise she has remained perfectly well up to the present time.—[*London Lancet*.]

Presence of Iodine in Sarsaparilla. By M. A. GUILLIERMOND, Apothecary.—The experiments upon the presence of iodine in sundry plants, as recently published by Prof. Chatin, have induced me to inquire whether sarsaparilla did not owe its anti-syphilitic qualities to the presence of this substance among its constituents. The peculiar odor of the decoction, also, having frequently struck me, I conceived the idea, which has been confirmed by chemical analysis. My mode of procedure was as follows :

Five hundred grammes of Honduras sarsaparilla were incinerated and washed with water, which liquid was then evaporated to dryness, giving an alkaline product which was digested in alcohol. Upon the application of the usual tests for iodine, its presence, in considerable quantity in the state of iodide of potassium was evident. I found no iodine in the root, after it had been exhausted with water, although I found it present in the extract ; thus proving that it passes into the aqueous preparations of sarsaparilla in the state of a soluble salt.

It is reasonable to suppose that these results are not unimportant in a therapeutic point of view. They confirm the opinion offered by M.

Chatin upon the presence of iodine in plants employed as anti-scorfulous remedies.

This fact being acknowledged it would prove interesting to ascertain the connection existing between the amount of iodine contained in these plants, and the greater or less activity attributed to them. Sarsaparilla may yet become a valuable auxiliary, and in certain cases even a succedaneum of iodide of potassium—*L'Abeille Médicale*, Jan. 15, 1851, from *Gaz. Méd. de Lyons*. *American Journ. Pharm.*

Eau de Cologne. By Professor VARRENTRAPP.—This well-known perfume is a solution of different volatile oils in pure strong spirit. The principal condition for the preparation of a fine water, is the employment of a spirit quite devoid of fusel-oil (oil of grain) and of all foreign odor.

In respect to the proportion and kind of oils employed, we have numerous formulæ. It is of importance that these oils, which are usually purchased of the druggists of the South of France, should be of the finest quality, and that no oil should be used in sufficient quantity to allow of its peculiar odor being recognisable in the mixture. The oils are to be dissolved in spirit, and the mixture allowed to stand for some weeks (or still better for some months) to improve its odor. Distillation does not effect this; on the contrary, a fresh distilled water requires to be kept a much longer time. Distillation is indeed objectionable, for on account of the greater volatility of the spirit, the oils in part remain behind in the still. Distillation can improve the odor only when the less volatile oil has been used in too large a quantity, and we wish to obtain a better proportion. Before all things, we should employ a pure, old, strong spirit, and not too much of, nor a too strongly smelling, oil.

The different sorts of volatile oil which are obtained from varieties of citrons, oranges, and lemons, in different states of maturity, are the most important, and, therefore, it is most important to ascertain their purity and goodness.

Förster gives the following formula for the preparation of a fine Eau de Cologne: Take of rectified spirit of 82 per cent. of Tralles (= sp. gr. 0.855) 6 [wine] quarts; essence of oranges, essence of bergamot, essence of citron, essence of limette, and essence of petits grains, of each 3j; essence of cedro, essence of cedrat, essence de Portugal, and essence de neroli, of each 2ss; oil of rosemary, 3ij; and oil of thyme, 3j.

Otto gives the following formula for a good Eau de Cologne; Rectified spirit of 86 per cent. of Tralles (= 0.846 sp. gr.) 200 [wine] quarts; oil of citrons, lb. iv; oil of bergamot lb. ij; oil of neroli $\frac{1}{2}$ lb.; oil of lavender lb. ss; oil of rosemary, $\frac{1}{4}$ lb.; and spirit of ammonia, 3ss. Mix.—*Pharm. Journal March*, 1851. *Ibid.*

Curious effects of Fire arms held close to the Body.—From a very interesting communication of M. Rigal (de Gaillac) to the Surgical Society of Paris it appears that when the muzzle of a pistol was held

tightly to the chest, so as to prevent the ingress of air into the barrel, the ball could not penetrate the walls of the thorax, but be deflected at a considerable angle. In a case of attempted suicide, related by M. Rigal, the ball rebounded from the chest, and struck the ceiling. Another strange fact connected with gunshot wounds, was mentioned by the same surgeon—viz., two distinct shots fired by an insane man upon his wife, with two distinct perforations of the clothes, and two tracks of the charge within the chest, and *only one* aperture of entrance.—[*London Lancet*.]

Sixth Meeting of the Association of Medical Superintendents of American Institutions for the Insane.—Dr. Kirkbride, from the standing committee on the construction of Hospitals for the Insane, in compliance with the resolution adopted last year, read a report, containing a “series of Resolutions or Propositions, affirming the well ascertained opinions of this body in reference to the fundamental principles which should regulate the erection and internal arrangements of American Hospitals for the insane.”

I. Every Hospital for the Insane should be in the country, not within less than two miles of a large town, and easily accessible at all seasons.

II. No Hospital for the Insane, however limited its capacity, should have less than fifty acres of land, devoted to gardens and pleasure grounds for its patients. At least one hundred acres should be possessed by every State Hospital, or other Institution for 200 patients. to which number these propositions apply, unless otherwise mentioned.

III. Means should be provided to raise ten thousand gallons of water, daily, to reservoirs that will supply the highest parts of the building.

IV. No Hospital for the Insane should be built, without the plan having been first submitted to some Physician or Physicians, who have had charge of a similar establishment, or are practically acquainted with all the details of their arrangements, and received his or their full approbation.

V. The highest number that can with propriety be treated in one building is two hundred and fifty, while two hundred is a preferable maximum.

VI. All such buildings should be constructed of stone or brick, have slate or metallic roofs, and as far as possible be made secure from accidents by fire.

VII. Every Hospital, having provision for two hundred or more patients, should have in it at least eight distinct wards for each sex,—making sixteen classes in the entire establishment.

VIII. Each ward should have in it a parlour, a corridor, single lodgings for patients, an associate dormitory, communicating with a chamber for two attendants; a clothes room, a bath room, a water closet, a dining room, a dumb waiter, and a speaking tube leading to the kitchen or other central part of the building.

IX. No apartments should ever be provided for the confinement of patients, or as their lodging rooms, that are not entirely above ground.

X. No class of rooms should ever be constructed, without some kind of window in each, communicating direct'y with the external atmosphere.

XI. No chamber for the use of a single patient should ever be less than eight by ten feet, nor should the ceiling of any story occupied by patients be less than twelve in height.

XII. The floors of patients' apartments should always be of wood.

XIII. The stairways should always be of iron, stone, or other indestructible material, ample in size and number, and easy of ascent, to afford convenient egress in case of accident from fire.

XIV. A large Hospital should consist of a main central building with wings.

XV. The main central building should contain the offices, receiving rooms for company, and apartments entirely private, for the Superintending Physician and his family, in case that officer resides in the Hospital building.

XVI. The wings should be so arranged, that if rooms are placed on both sides of a corridor, the corridors should be furnished at both ends with moveable glazed sashes for the free admission of both light and air.

XVII. The lighting should be by gas, on account of its convenience, cleanliness, safety, and economy.

XVIII. The apartments for washing clothing, &c., should be detached from the Hospital building.

XIX. The drainage should be under ground, and all the inlets to the sewers should be properly secured to prevent offensive emanations.

XX. All Hospitals should be warmed by passing an abundance of pure fresh air from the external atmosphere, over pipes or plates, containing steam under low pressure, or hot water, the temperature of which at the boiler does not exceed 212 degrees F., and placed in the basement or cellar of the building to be heated.

XXI. A complete system of forced ventilation, in connection with the heating is indispensable to give purity to the air of a hospital for the Insane, and no expense that is required to effect this object thoroughly, can be deemed either misplaced or injudicious.

XXII. The boilers for generating steam for warming the building should be in a detached structure, connected with which may be the engine for pumping water, driving the washing apparatus, and other machinery.

XXIII. All water closets should as far as possible be made of indestructible materials—be simple in their arrangement, and have a strong downward ventilation connected with them.

XXIV. The floors of bath rooms, water closets, and basement stories should as far as possible be made of materials that will not absorb moisture.

XXV. The wards for the most excited class should be constructed with rooms on but one side of a corridor, not less than ten feet wide, the external windows of which should be large, and having pleasant views from them.

XXVI. Wherever practicable, the pleasure grounds of a Hospital

for the Insane should be surrounded by a substantial wall so placed as not to be unpleasantly visible from the building.

Which propositions having been duly read and maturely considered, were adopted by the Association.

On motion of Dr. Hanbury Smith, it was

Resolved, That the Secretary be instructed to cause the propositions now adopted, in reference to the construction and arrangements of Hospitals for the Insane to be published in the Medical Journals of this Continent, as the sentiments of this Association, on the subject referred to.

To the Medical Profession of the United States :

The undersigned having been appointed, at the last meeting of the American Medical Association, Chairman of the Committee on the "Results of Surgical Operations in Malignant Diseases," respectfully solicits contributions to the subject, founded upon personal observation. To place the subject in as tangible a form as possible, he begs leave to direct attention to the following points :

1. The difference between cancerous and canceroid diseases, or those affections which are truly malignant, and those which are only partially so. In the former category are comprised scirrhus, encephaloid, and melanosis ; in the latter, certain maladies of the skin and mucous tissues, as lupus, cheloid, eiloid, and cancer of the lip.

2. The precise seat of the disease, as the skin and subcutaneous cellular tissue ; the eye, ears, nose, face, lips, tongue, salivary glands, jaws, and gums ; the lymphatic ganglions of the neck, axilla, groin, and other regions ; the mammary gland, uterus, ovary, vulva and vagina, penis and testis ; the anus and rectum ; and, finally, the extremities.

3. The age, sex, temperament, residence, and occupation of the patient.

4. The cause of the disease, its progress, and the state of the part and of the system at the time of the operation.

5. Mode of operation ; whether by the knife, caustic or ligature.

6. Time of death, or relapse, after operation.

7. Examination of the morbid product ; how conducted—whether by the unassisted eye alone, or by means of the microscope, and chemical tests.

The undersigned hopes that the importance of the subject confided to him, as chairman of the committee above referred to, will be sufficiently appreciated by his professional brethren to induce them to aid him in carrying out the wishes of the American Medical Association. The subject is one of absorbing interest, and cannot fail, if properly treated, to elicit matter of the greatest benefit. It is very necessary that all communications upon the subject should be sent to the chairman of the committee by the 1st of January, 1852.

Medical journals and newspapers friendly to the interest of medical science, will confer a favor upon the undersigned by inserting the above notice.

S. D. GROSS, M. D.

University of Louisville, June 29, 1851.

SOUTHERN
MEDICAL AND SURGICAL
JOURNAL.

Vol. 7.]

NEW SERIES.—SEPTEMBER, 1851.

[No. 9.

PART FIRST.

Original Communications.

ARTICLE XXVII.

Typhoid Fever. By JOHN S. WILSON, M. D., of Muscogee County, Georgia.

The extensive prevalence of this disease, together with the unsettled state of opinion in the profession, in reference to its pathology and treatment, will, I hope, be a sufficient apology for introducing it again, so soon after the "Remarks" of Dr. Long, contained in the June No. of this Journal. I shall, in this communication, endeavor to follow his excellent example, and shall make my remarks mostly practical. But, before recording my experience in the treatment of this disease, I will make a few general remarks in reference to its pathology, and some other points, which have so much divided the profession and given rise to so much discussion.

A great many of the difficulties involved in the consideration of continued fevers, originate in a confusion of terms, and in the attempts of writers on this side of the Atlantic "to give a European coloring to the pathology of our fever." Almost all our treatises on Continued fever are but transcripts from European writers, with their endless discussions as to the identity of typhoid and typhus fevers, or of typhoid fever and follicular enteritis. The history of continued fevers in the United States is yet to be written, (unless this has been done by Dr. Bartlett,) and it is to be hoped that our future investigations will be conducted with more independence; for I feel assured

that the solution of the difficult problems presented to us, will be much facilitated by recording our *own* facts, and drawing from them our *own* conclusions, instead of trying to find a community of symptoms and organic lesion, in fevers originating under such dissimilar circumstances as the fevers of this country and those of Europe. We are much more interested in a correct pathology than in names; still it is very unfortunate that writers have used the same terms so differently, and thus rendered it almost impossible for them to understand each other; for if this were not so, a great many of the dissensions which now divide the medical world, and perplex the student, would cease. To say nothing of the discussions as to the proper application of the terms typhus and typhoid, it seems that European writers are not even agreed as to the use of the prefix continued, when used in a generic sense—"some taking one of its types, some another, from which to draw a description." The best means of obviating such difficulties is, to make continued fever a generic term, and apply it to all fevers not periodical; and I think it would be sufficient to embrace under this head only three varieties, viz: Synocha or inflammatory fever, typhus and typhoid; with subdivisions founded on the prominence of the particular symptoms, or, if possible, on the organic lesions. I have included the Synocha of Cullen, not because I think it will be often met with in practice, but because it may serve as a type or standard of comparison, as the antipode of typhus, or the typhus gravior, of systematic writers: the term Synochus, as used by Cullen, is certainly useless, if the word typhoid is retained; for he defines it to be—"Febris ex Synochû et typho composita," which means a fever partaking of the nature of typhus, or in other words, a *typhoid* fever; or a synochus in the beginning, and a typhus in the end.

It is not my design to enter into a discussion of the identity or non-identity of typhus and typhoid fevers; nor shall I attempt to say whether the latter shall be considered primarily, as an idiopathic fever, with consecutive intestinal inflammation, or essentially a follicular enteritis. On the first point—the identity of typhoid and typhus—I will quote from Dr. Bell, (Bell & Stokes' Lectures, vol. ii. p. 669,) merely remarking

that I fully coincide in the conclusion to which he has arrived. He says, after quoting numerous authors, and patiently investigating the whole subject, "If we were to institute a comparison between the two fevers, typhus and typhoid, we should find many more features of resemblance than of difference," &c. The second point—the symptomatic or idiopathic nature of typhoid fever—is one of vast importance, one worthy of the most serious consideration and the most studious investigation; for the settlement of this important question would have an important bearing not only on the treatment of this fever, but also on this whole class of fevers. It is generally admitted, even by those who do not adopt the French pathology, that inflammation and ulceration of the intestinal mucous membrane, and especially of the glands of Peyer and Brunner, are frequent concomitants of typhoid and typhus fevers: still we are not prepared to admit that this is an *essential* feature of the disease, for Andral has shown, that patients have perished with all the symptoms of typhoid fever, without any lesion whatever in this membrane. In the present state of our knowledge, I think it safest to assume the following positions—viz:

1st. There is no fixed anatomical lesion either in typhoid or typhus fever.

2d. Inflammation and ulceration are frequently found in the intestinal mucous membrane (Peyerian glands)* of typhus.

3d. Typhoid fever is only a milder form of typhus.

4th. That inflammation and ulceration of the intestinal mucous membrane, and especially of the glands of Peyer and Brunner, occur in the vast majority of cases, in typhoid fever, either as an *essential* feature or as a consecutive phenomenon.

5th. That typhoid, like typhus fever, is, under favorable circumstances, contagious.

I do not intend to discuss these propositions in extenso: I will only make a few remarks on the two last, and refer the reader to the work already quoted (Bell & Stokes, p. 659, et seq.) for a full exposition of the whole subject. One of the earliest

* Dr. Tweedie found these to exist in 24 out of 54 cases of typhus fever. Dr. Anderson reports 68 cases of inflammation and ulceration, in 74 fatal cases occurring at Glasgow; and a greater proportion than this even has been reported in Edinburg

and most characteristic symptoms of typhoid fever is abdominal tenderness, (on pressure,) together with increased force in the pulsation of the abdominal aorta, and other symptoms of inflammation in the viscera of this cavity. Post-mortem examinations made by Louis, Chomel, Gerhard, and others, show that this inflammation does exist, in the vast majority of fatal cases, in the fever, and that its seat is the follicular glands of Peyer and Brunner. Now, are we justified in drawing the conclusion from these facts, that this inflammation is *pre-existent*, and that the fever is only symptomatic?—that the term typhoid fever should be discarded, and that of follicular enteritis substituted? I think not; because it has already been shown from high authority, (Andral,) that all the symptoms of typhoid fever may exist without the lesion referred to; and it is declared by other observers that there is no uniformity in the violence of the abdominal lesions; and still less correspondence between the symptoms of the disease and the extent of the Peyerian lesion. But while I oppose the exclusion of the term “typhoid,” and the substitution of follicular enteritis, I would not by any means under-rate the importance of the latter; for though we may not be prepared to admit that it is absolutely *essential* to the existence of the group of symptoms termed typhoid—that it is the *ipse morbus*, the proximate cause of those symptoms—still we know that it is one of the earliest and most constant companions of typhoid fever; and whether we admit it to be *the essential* feature of this fever, or not, we should regard it as one of the most *dangerous* and *uniform* features, and treat it accordingly. So far as my experience goes, I can assert that I have never seen a case of typhoid fever in which there was not more or less evidence of intestinal inflammation, *early* in the disease; and I have always treated it accordingly, *acting* on the presumption that *this is the* disease; and while we do this, paying due attention, in the mean time, to *all* the symptoms as they arise, I think it matters but little whether we consider the fever idiopathic or symptomatic—typhus or typhoid.

The contagiousness of this disease, though it be one of the most interesting questions connected with it, I shall pass over slightly, because it involves the argument as to the identity of

typhus and typhoid fever, which I do not intend to discuss. It could be proved, if necessary, by quotations from numerous authors, that even typhus fever is not contagious, except under such circumstances as are found to exist in camps, jails, hospitals, and crowded, illy ventilated and filthy apartments. Now if this be true, in reference to typhus fever, it will apply *a fortiori* to typhoid fever, admitting that this is only a mild or modified form of the former disease. I will dismiss this subject, then, by admitting that typhoid fever *may* be communicated by human effluvia under very favorable circumstances, such as those mentioned in connexion with typhus fever; but that it is not contagious, in the absence of such circumstances. With a brief record of my own observations and experience in this disease, I conclude.

My observation in this fever fully confirms the declaration of Dr. Long and others, that the disease "generally comes on slowly and gradually." I also coincide with him in the value of this circumstance as a diagnostic; and not only as a diagnostic, but likewise as one of the elements of prognosis, for I have generally found that the gravity and obstinacy of the disease, bore some relation, or were in direct ratio with the duration of the premonitory symptoms. But while this is generally true, the invasion of this fever is *sometimes* sudden, and its progress to a fatal termination rapid. I have seldom been able to trace the onset of the disease to a distinct chill—headache I have found invariably in its beginning, but this symptom generally yielded either spontaneously, or to the treatment, during the first week, notwithstanding the gravity of the other symptoms might have increased.—(Vide Watson's Practice, p. 938.) The skin is hot and dry, with some moisture frequently about the head; the tongue is coated with a dark brown fur, sometimes black, and in some of the worst cases it is red and clean, having a glazed appearance, as if it had been varnished—sometimes it is furred and cracked. Sordes are sometimes seen on the teeth, in the very inception of the disease, but generally this symptom does not appear until it has continued several days. I consider the early appearance of sordes an unfavorable symptom. I have found great variation in the pulse, so much, in *well-marked* cases of typhoid fever, that I

cannot assent to the declaration that it is "peculiar and characteristic"—I have frequently found it "small, quick and frequent," and I have sometimes observed the "double beat," mentioned by Dr. Long, but I have often found it slow and full, running from 75 to 100, and in some cases I have even found it *below* the natural standard in frequency—in others, it varies but little from the standard of health in frequency, having nothing morbid about it, except a little sharpness. These slow-pulse cases I do not consider so dangerous as the others, but I have always found them remarkably tedious—they will not die, nor will they get well soon. In conclusion, I would remark that I believe the pulse described by Dr. Long is the most common in this fever, while I cannot admit that it "is peculiar and characteristic."

Diarrhœa I have not found to be a constant symptom, and never an early one: it is occasionally absent during the whole progress of the disease, but there is generally diarrhœa, or a tendency to it during the second or third week. This will be explained when I come to the treatment.

The next and most important symptom belonging to this fever, which I shall review, is "abdominal soreness." This, so far as my observation extends, is an *invariable* symptom, so much so that I look for it as a matter of course, as one of the *essentials** of the disease. I have generally discovered this soreness first in the epigastric region, even in the absence of nausea and vomiting; which latter symptoms I have most frequently found absent, as well as other direct symptoms of gastric irritation, save this soreness—this I have *always* found, in cases of all grades, from the mildest to the gravest. I think that this soreness has oftener extended towards the right hypochondrium than the left, and as this corresponds with the course of the duodenum, (the seat of the glands of Brunner,) I have been induced from this circumstance, and from the absence of gastric symptoms, to conclude that this was the real seat of disease, instead of the stomach; and that the sensibility of this organ was increased by sympathy with the duodenum. But it is my design to record facts, leaving theories to others. I do not wish to be understood as saying that I have never disco-

* I do not use this term in a pathological sense.

vered this soreness any where besides in the epigastrium, because I *have* traced it to the right ileac region, but I have always found it existing first in the epigastrium. This tenderness is accompanied frequently by the gurgling sound mentioned by all writers, and I have been able to locate it by this sign alone, without any expression of pain on the part of the patient. As before observed, this soreness is one of the earliest and most constant symptoms—it is among the first to appear and sometimes one of the very last to leave; and I have noticed that the patient will not convalesce so long as it remains—he may stand still, but he *will not* advance. This soreness is seldom complained of except on pressure—sometimes it is felt on making a full inspiration. The eruption mentioned by authors, I have seen only in one case; this was accompanied also with anginose symptoms, and was rapidly fatal.

Pulmonary symptoms, according to my observations, have been equally rare. I have seen a few cases complicated with slight bronchitis. My experience has taught me, that our prognosis in this disease should be very guarded; and it has also taught me the sad lesson, that this fever will sometimes prove fatal under the most favorable circumstances of age, constitution, &c., even when the treatment is not “*aggravating*.” I know of no disease more fallacious than the one under consideration: “grave and prolonged cases” will frequently terminate favorably, contrary to all expectation; on the other hand, the disease may commence without any untoward symptoms, and continue thus for several days, when the pulse will suddenly rise to 150 or 160, delirium and coma will supervene, and death soon close the scene.

In other cases all the graver symptoms may yield, but still they will not improve—they remain stationary for days, and even weeks, and may finally die. Even in these cases the chances are in favor of the patient, if we can keep the disease in abeyance without having resort to very active remedies. It is in such cases as these that we should act on the aphorism of Celsus—“*Multi morbi curantur*,” &c. To conclude on this point: If the pulse rise to 150, and creeping or jerky—if the skin and tongue become more dry, or if the former be bedewed with a viscid sweat—if the delirium becomes furious, or

the stupor merges into coma—if the bowels are evacuated involuntarily, the case is almost hopeless.

I cannot leave this subject, without repeating my warning against a *decided* prognosis, for my experience fully confirms the declaration made by Dr. Watson, (op. cit., p. 957,) that the disease is “liable to sudden and remarkable *change in the symptoms.*”

TREATMENT.—It has been remarked, with much truth, by the distinguished physician just quoted, that the treatment of continued fever has always been “a stumbling block to young practitioners,” and I think I might add, with equal truth, to old practitioners too; for certainly there is no disease in which the practice is more unsettled; and it is equally true, that the most judicious treatment is too often unavailing. Before proceeding to the detail of treatment, I assume the two following positions, which I think will be granted by all, and therefore need not be defended—they are these:—1. This disease cannot be materially abbreviated by treatment, after it is fully formed: it “can be conducted safely through its different stages—it cannot be cured.” 2. We have no specific remedy, nor have we any specific and established mode of treatment; this must be modified by the peculiar circumstances of each case, and the predominance of certain symptoms.

It will be readily inferred from the above that I have but little faith in *emetics* and other active measures recommended by some, for the cure of this fever; still I believe that emetics might be used with great advantage, in the *very beginning*, while the nervous system alone is, as yet, implicated; but unfortunately for the patient, the time for their use is generally past, when the physician is called, the disease being then “*rooted and grounded.*”

Bleeding.—No remedy has given rise to more discussion than this, while the Armstrong and McIntosh school extol it too highly and use it perhaps too freely, their opponents go into the opposite extreme; and not satisfied with withholding it, they commit as great an error by the too early and free use of stimulants. In this, as in all other disputed points in medicine, we should ever bear in mind that sound maxim, “*in medio tutissimus ibis.*”

As with emetics, the bleeding stage has frequently passed when the physician is called, but I think the remedy should never be withheld, when the pain in the head is severe, and the fever high, with evidence of actual or imminent inflammation in any organ, for fear of subsequent debility; for I contend that we can remedy debility attended with a *subdued* inflammation, with much more ease than we can *subdue* an inflammation after it has prostrated the vital powers. My plan is to bleed moderately and *very cautiously* under the circumstances mentioned; and if I have any thing to regret in this connexion, it is because I have not done so oftener, and a little more freely; for I confess that I have been *afraid* of the remedy; so much so that I have only of late adopted the plan of bleeding under *any* circumstances, in this fever. I will conclude on this point, by saying that no remedy requires more caution and sound discretion, than the use of the lancet in typhoid fever—that it may, as a general rule, be dispensed with, but in some cases, *early* in the disease, it is decidedly indicated, and should be used in such cases; for I believe where it is indicated, the salvation of the patient often depends upon it.

Local Bleeding.—Of the advantages of local bleeding in typhoid fever, I can speak in terms of *decided* commendation: the application of scarified cups to the epigastrium, or where ever abdominal soreness predominates, is one of my first remedies; and I repeat them again and again, until this soreness yields, or until the blistering period arrives. I have used this remedy as many as twelve times, in one case, with marked advantage, the patient expressing relief from the “fulness about the abdomen” and “shortness of breath,” before the application was concluded. I *never* treat a case of this fever without applying cups to the abdomen; for they are *always* indicated by the soreness in this region. If there be much headache or cerebral disturbance, I also apply cups to the temples—in this way, I think they are more indicated, in active delirium than in coma.

Cathartics.—Some writers of great eminence recommend the use of active cathartics in the beginning of this disease, in decided terms, and I am acquainted with physicians who adopt

this plan ; but I consider this more dangerous and objectionable than even the indiscriminate use of the lancet ; because we cannot hope to cut short the disease by such measures, and they tend only to increase the intestinal irritation and set up an uncontrollable diarrhœa. So well am I convinced of this, that I withhold all active cathartics from the very beginning, and thus *avoid* the diarrhœa—it is to this plan I attribute the fact that I have seldom been annoyed by this troublesome complication. I commence generally by giving a small dose of calomel combined with Dover's powder, with the intention of having it to remain from 24 to 48 hours—I then follow it by castor oil, giving directions to check the bowels if the evacuations are thin and watery.

Opium.—The use of this remedy requires much caution, on account of the tendency to coma ; but it may be used with the happiest effect, where the excitement is more nervous than vascular, with more vigilance than coma. In doubtful cases it should be commenced in small doses, and increased *pro re nata*. I generally withhold it, unless there be some decided indications for it, and when I use it, I prefer the Pulv. Dov. It is best to give this at night, and let the patient rest undisturbed, if he will. I think the plan of arousing patients from a quiet sleep, to give them medicine, very objectionable ; for this is a better remedy to the exhausted powers than any we can apply.

I did intend to review several other remedies, and conclude with a general summary of my experience in their use, but as the limits of this article will not admit of it, this design must be postponed to some future time.

ARTICLE XXVIII.

Surgical Cases. By C. T. QUINTARD, M. D., of Roswell, Ga.

The following case is reported, as illustrative of the principles, adduced in a paper on "Injuries of the Cranium," read before the Medical Society of the State of Georgia, at its last session, by Henry F. Campbell, M. D., of Augusta, and subsequently published in the Southern Med. and Surg. Journal :

Wm. H., æt 38 years, was, during an affray on the 1st Jan. 1850, struck by a stone (weighing two pounds) on the left

frontal region, near the coronal suture ; which produced a fracture, with depression of a portion of the bone, attended by considerable hemorrhage. Coma immediately supervened, in which condition he lay several days ; at the end of which time the coma subsided, and of his own accord he got up and walked about ; seemed conscious of surrounding objects and events, but had lost the power to articulate. At the end of eight weeks the wound of the scalp had healed, with the exception of a fistulous opening through which a profuse discharge of pus was flowing, and which gave passage at successive intervals to nine spiculæ of bone, varying from a line to a half inch in length. At the time of our seeing him, there was a fistulous opening through the scalp, communicating with a triangular opening in the skull, having its base above and on a line with the suture, its apex downwards, through which the pulsations of the brain were discernable. To the left of this opening the probe detected a large spicula of bone, and also an ossific deposit pressing upon the dura mater. The patient had, to a certain extent, lost his memory and hearing—which last defect was marked by a “constant roaring in the head.” His general health was tolerably good. The operation (April, 1851) consisted of a crucial incision over the depressed bone, and the flaps of integument being freely dissected up, the trephine was applied, and the bone, which had partially re-united, was removed. At the point of bony union there were numbers of ossific points protruding downwards into the substance of the brain, and also a loose piece of carious bone floating loosely in the pus with which all the parts were suffused. Immediately—*instantly*—on the removal of the bone, the roaring in the head ceased and all disagreeable symptoms subsided. The wound was dressed by adhesive plasters, and a simple bandage applied—adhesion went on rapidly, and the patient in a few days returned to his home.

The phenomena presented in the above case are somewhat remarkable. The long continued coma—the aphonia and loss of memory, together with the decided aberration in the function of audition—are symptoms which fully establish the rationale of these effects, developed and commented upon by my friend Dr. Campbell.

Dr. C. accounts for similar results of pressure on the brain by a destruction of equilibrium of pressure.

Osteo-sarcoma of the Inferior Maxillary—Exsection and Recovery. Miss D. S., æt about 21 years, had had a slight enlargement on the base of the lower-jaw, from the period of her second dentition, which had remained nearly stationary till she arrived at the age of 14, when on the appearance of the menstrual phenomena, this tumour suddenly evinced a disposition to an increase of activity, which was attended by periods of pain and uneasiness. Two years subsequently, its growth became more rapid and the tumor continued to extend towards the symphysis, though not at all in the direction of the ascending ramus. At the time of our seeing her, she was very much disfigured by a large tumor over the left side, extending from the angle of the lower jaw, quite to the symphysis. Protruding from the mouth, was a large, red sarcomatous tumor, about an inch in diameter, which prevented the closure of the lips. On examining within the mouth there was discovered a considerable enlargement in the form of a firm fleshy mass, with a reticulated structure of bone on its posterior portion. This was easily crushed by pressure with the finger.

Notwithstanding her general health was unpromising, and bore the usual features of carcinomatous cachexia, we determined upon the operation of exsection as the only possible method of relief.

The patient was subjected to the influence of chloroform, and the operation performed in the ordinary mode, viz., an incision was made along the base, commencing a little above the angle and terminating about half an inch to the right of the symphysis. This was met by a perpendicular incision from near the vermilion border of the lower lip to the termination of the first incision. The flap was raised and the bone divided with Hey's saw, first above the angle, and afterwards at a point to the right of the symphysis. It was then dissected from the muscles and other attachments, and removed. Besides the facial, but one small artery required the ligature, and withal, the hemorrhage was but trivial. A piece of sponge was placed in the position of the removed bone—the lips of

the wound brought together and retained by three sutures, and adhesive straps, and rendered secure by the application of the bandage. The wound healed rapidly in every respect, and the case progressed finely towards recovery. Eight months have elapsed since the operation, and as yet there is no disposition to a return exhibited. The deformity is very slight, and the patient enjoys excellent health and spirits. In relation to this case, there is perhaps but one practical remark to be suggested, and it is from the low condition of the patient at the time of the operation, and also the advanced stage of the disease. Should this terrible affection return, we have the satisfaction of having secured to our patient a considerable period of actual comfort and health.

ARTICLE XXIX.

CLINICAL REPORTS, No. 1.

On a Case of Obscure Uterine Tumor terminating fatally—with remarks. By D. C. O'KEEFFE, M. D., of Penfield, Ga.

Under the above caption it is proposed to give, from time to time, a series of articles comprising details of cases as they occur in the sick room. The term "*Clinical Reports*," has been usually applied to the practical instruction imparted to students at Medical Schools when the subject of the disease lectured on is before them, while clinical instruction is used to designate the hospital *clinique*. Should the clinical reports appear in print, as they frequently do, they would be to the reader in the same light as a record made at the bed-side of the patient and communicated to him through the journals. There can be no impropriety then in extending the use of this term to reports made in the sick room by a physician in private practice, since the literal and etymological meaning of it is bed-side report. As regards the necessity or utility of making such reports I would introduce the series with Surgeon Bell's opinion :

"It will be generally admitted that practical observations on the treatment of diseases of daily occurrence are more valuable to the medical practitioner than the most interesting descriptions of anomalous cases, however extraordinary in their character, or successful in their treatment. I am convinced, indeed,

that the publications of the journals of well-employed medical men, giving in detail the treatment of every case occurring in their private practice, would prove most useful to the young practitioner, and be a valuable contribution to our medical literature."

To promote such views, the appended report is humbly submitted :

CASE 1. June 1st, 1850.—The subject of the following notes was a negro woman *æt.* 35, of full habit and plethoric temperament. Up to the invasion of her last illness, she enjoyed excellent health and had given birth to twelve children without any impediment in parturition.

After the birth of her last child, a period of eight months, she has suffered from occasional uterine hemorrhage, sometimes very profuse, at others not so much. These hemorrhages in conjunction with a tumor, she distinctly felt in her left side, made serious inroads upon her constitution, and caused her a few months ago to relinquish her usual employment of field labor. A physician who was called upon to arrest the hemorrhage, under the impression of its being a case of menorrhagia, prescribed vaginal injections of alum water and some astringent tea. This course being persisted in for a week or longer without any mitigation of the hemorrhage, and the patient very reasonably becoming more enfeebled every day, I was requested by her owner to see her, and inform him of the true nature of her disease, for my predecessor had made neither digital nor instrumental examination, therefore he was not prepared to give an opinion of any diagnostic value.

I found the patient lying upon her back in a great deal of pain and considerably emaciated; the whole train of symptoms that usually characterize the anemic state was presented. She complained of constant pain in the uterine region, and could but illy bear pressure on the hypogastric region. She could get no rest at night, her bowels were usually constipated and her appetite was well nigh gone.

A vaginal digital examination revealed to me a soft tumor, the size of a hen's egg, of an oval form, filling up the vaginal cul-de-sac to the left, and leaving scarcely room to pass up two

fingers to the os uteri along the right side of it. Its consistence was not very dense, being soft, somewhat spongy and easily broken down between the fingers. By exercising slight pressure on it with the extremities of the fingers, I could bring away large portions of the morbid mass without inflicting any pain or exciting any sensation whatever in the patient. This heterologous mass apparently consisted of coagulated blood interspersed here and there with pieces the size of a pea (some larger) of a dingy white color, and soft, resembling adipose matter. Considerable quantities of this mass could be extracted with the fingers, thus infringing on the spheroidal form of the tumor; but in a day or two it would reproduce itself and again assume its former shape. It grew from the left half of the circumference of the uterine lip, its peduncle embracing from half an inch to an inch of the external surface of the cervix of the same side. About four-fifths of the capacity of the vaginal cul-de-sac was blocked up with the tumor, impeding the movement of the uterus in any direction. The os uteri was sufficiently patulous to admit the first phalanx of the middle finger; the lips, hard and rendered uneven by several notches, presented a cartilaginous feel. There was more sensation manifested when the lips were touched, or the finger introduced within the cervical cavity, than by any manipulations with the tumor itself. Its peduncle (if its attachment to the os and cervix could be so called) was many degrees larger than that usually assigned to polypus; indeed the plain of the tumor on its upper surface was inclined to an angle of about 23° , the inclination commencing at its attachment to the cervix and extending to its convexity, while inferiorly its neck was as well defined as the neck of the ordinary brass knob of a door. The tumor bled, though not freely, when touched, so that when examined the hand was smeared with blood. The surface of the tumor was neither rough nor granulated, but smooth; and the parts detached differed in no respect from the uniform appearance of coagulated blood. The parts torn off had a slight unpleasant odor, but none of that which usually attends a malignant growth in these parts.

Without coming to a definite conclusion as to its nature, I prescribed vaginal injections of sulph. of zinc, opiates at night,

and quinine and iron to retrieve her lost strength. These means arrested the hemorrhage to a very great extent, and her health recruited considerably for two weeks, when she suffered from another attack of hemorrhage that counteracted what improvement she had made.

In the mean time, I had satisfied myself, as I thought, of the nature of the tumor by repeated vaginal examinations, digital and with the speculum; and was corroborated in my opinion by two gentlemen of extensive experience in the diseases of females, who were consulted by letter.* Regarding the tumor to be a polypus, Gooch's canu'a was procured, and several attempts were made to throw the ligature round the neck of the tumor, to no effect. Efforts were made with ligatures of different materials, viz: cat-gut, whip-cord and silk, wrapped round with fine metallic wire. The greatest difficulty in the way was the retention of the ligature on the neck of the tumor in consequence of the thickness of the stalk and the inclined plane of its superior surface; for as soon as the slightest force was used in tightening the ligature, it would slip from the sloping surface aforementioned over the cavity of the tumor. After three or four unsuccessful attempts in this way, I was convinced that at least *I* could not remove it with the ligature; so I requested three neighboring physicians to see the case with me. Each one examined for himself, and it was determined in consultation to make a final effort for its removal with the ligature, which was attended with no better success than the others. In this event, it was agreed to put her on a palliative course of treatment, and wait for further developments of the tumor, in the hope that its form might become more favorable for removal. It was also frankly announced to the owner, that if the tumor could not be removed the issue would prove an unfavorable one.

A short time after the last effort for removal was made, her master, harrassed by the patient's importunities, and as a placebo to her sufferings, put her under the charge of a certain non-descript practitioner, who continued to treat her until her death.

In April of the year (1851) I was in attendance on another

* Drs. J. A. Eve and H. F. Campbell, of Augusta. Vide seq.

case on the plantation where the subject of these notes resided, still taking medicine from the "*root doctor*," as the non-descript is popularly denominated. In view of the lively interest I felt in the case, I was induced to see it; and lo, what a loathsome spectacle of human suffering was before me! As soon as I entered the cabin where she lay, I was instantly assailed with the most offensive odor I ever had perception of. She lay on her back in the utmost agony and could not move but with the greatest caution on account of the pain she endured. She was not as much emaciated as she is described at the head of these remarks; but her round features and plump appearance were owing to anasarca. The left extremity was the part most affected, being nearly one half larger than its natural size. About the middle of the left leg was a sore that continually discharged a thin serum.

I made but a very unsatisfactory vaginal examination in consequence of the foul odor emitted; and from the same cause my desire of examining with the speculum was frustrated. The condition of the morbid region was far different from what I have already described it. The spheroidal tumor was replaced by a hard cartilaginous mass of irregular surface; or rather, the tumor had disappeared and somewhat of an irregular cavity was left in its place. Much of the uterine substance was lost by ulceration; the os uteri had lost its outlines, and as well as I could judge, the morbid action had taken possession of the uterine body. The pulse at this time was 120 per m., and her nights were sleepless. In about three weeks death closed the suffering scene.

REMARKS.—The first enquiry that occurs to the reader's mind after the perusal of the foregoing details is: what is the nature of the tumor in question? I am not certain that I can answer this enquiry satisfactorily, inasmuch as the nature of the tumor is involved in some obscurity. Instead, therefore, of endeavoring to point out what it is, I will proceed to its investigation negatively, ("*par voie d'exclusion*," as the French term it,) in order to arrive at its real character.

The tumor under notice exhibits many features common to polypus in this region, which it may not be amiss to enumerate.

1stly. Its somewhat pedunculated form and smoothness. 2dly. The absence of ulceration at the time the diagnosis was made out, and insensibility to the touch. 3rdly. Hemorrhage and a correspondence of their places of attachment. 4thly. The manner of its connection to the uterus. Thus it is said that polypus, instead of being imbedded in the substance of the uterus, is attached to some part of it by a neck or pedicle, of a less diameter than the body of the polypus; so it was with the tumor under consideration. In this connection I will take the liberty of quoting the opinions of my esteemed friends, Drs. J. A. Eve and H. F. Campbell, of Augusta, Ga., in relation to the diagnosis of the present case. And it is but justice to these gentlemen to state that the data from which they drew their conclusions were meagre in the extreme, from the circumstance that the written description I gave them was necessarily scanty and unsatisfactory.

Dr. Campbell says: "I have examined the portion of tumor as well as it would admit of, not however with the microscope; it is too dry to ascertain any thing certain about it. It is in all probability a polypous or encephaloid growth. If cancerous, I would be inclined to the opinion of its being of encephaloid rather than scirrhus form, from the youth of the patient and the softness of the tumor. But the strong probability is that it is a *polypus*, from its pediculated form, and also from the fact that malignant growths seldom form *upon* the healthy tissues, but *among* them; while the non-malignant, to which class polypus belongs, are found invariably upon them, or in an isolated space by themselves, but not having *their* tissue mixed with *healthy* tissue, as in malignant growths."*

Prof. J. A. Eve says: "With respect to the nature of the tumor, I believe it is a polypus. I gave your specimen and letter to Dr. Campbell, that he might examine by microscope; his reply expresses my opinion. I have not the slightest hesitation in advising the removal of the tumor by ligature and cauterization of the point from which it sprung, by nitrate of silver."†

Again: In a note in Churchill's *Diseases of Females*, p. 177, Dr. Lee describes a variety of polypus strongly resembling in

* Extract of Dr. C.'s letter to writer.

† Extract of Dr. Eve's letter to writer.

substance the one under notice. He says, "a fourth variety of tumor of the uterus, to which the term polypus has also been applied by writers, is produced by a morbid enlargement of the glandulæ or ovulæ nabothi. One of these two bodies is sometimes converted into a cyst, as large as a walnut, or even a hen's egg, and hangs by a slender peduncle from the cervix or lip of the os uteri. It is smooth, and vascular, and contains in some instances a *curdly matter, or yellowish colored viscid fluid*. The tumor produces great irritation, and gives rise to copious sanguineous and mucous discharges from the vagina."*

Another case from the same authority extracted from a paper by M. Langstaff, in the 17th vol. of the *Medico-Chirug. Trans.* p. 63., and the affirmative evidence is at an end. "Mrs. —, aged 59, in whom, a few days previous to death, there was a large polypus in the uterus projecting into the vagina, died of hemorrhage, before a ligature was applied.

"*Dissection.*—The body of the uterus and its parietes were much larger than natural, yet there were not any signs of scirrhus or fungus hematodes.

"A polypus had formed at the superior part of the fundus of the uterus, which seemed to have had its origin in the muscular coat, it had projected into the mucous surface, and proceeded along the cavity in the form of a large pedicle, nearly equal in size to its base; and the growth had passed through the os uteri into the vagina, where it had acquired the magnitude of a large peach, and assumed the appearance of a fungoid tumor.

"The mucous surface of the tumor, in the vagina had been destroyed by ulcerative absorption; *it was coated with coagulated blood*, which appearance induced me to suppose that the hemorrhage had proceeded principally from this part, and not found the vessels belonging to the internal surface of the uterus. On cutting through the whole extent of the polypus, I found the cervix of a dense structure, exactly similar to that of the uterus; but to my astonishment, when the incision was extended through that part of it which had entered the vagina, I found in its centre grumous blood contained in a dense cyst, surrounded by coagulated blood."†

* The italics in this report are the writer's. † The italics are the writer's.

The substance of the tumor described in the former case as polypus corresponds somewhat with the description I have given of the tumor in the present instance. Mark the record: "This heterologous mass apparently consisted of coagulated blood interspersed here and there with pieces the size of a pea, of a dingy white color, soft, resembling adipose matter." Dr. Lee's tumor is "smooth and vascular, and contains, in some instances, a curdly matter, or yellow colored viscid fluid." Thus the fourth variety of polypus described in Dr. Lee's paper furnishes a parallel to the present in some of the particulars that present a disparity from the ordinary appearances of polypus viz: the heterogenous nature of the tumor. And M. Langstaff's case exhibits a polypus coated with coagulated blood—a condition that existed in our tumor, as has been stated in the fact that by the pressure of the fingers considerable quantities of the tumor could be detached, differing in no respect from coagulated blood, save in the interspersion here and there of fatty matter.

Thus we see that the tumor we have been considering has many symptoms in common with polypus: we will next give the negative facts. Dr. Churchill assures us that polypi are seldom or never attacked by inflammation or ulceration, *and they never degenerate into malignant disease*.^{*} Our tumor, therefore, was not a polypus, for the fatal result certainly proves that it was not otherwise than malignant; the patient died, not of hemorrhage, nor from mechanical inconvenience, but from constitutional irritation. Dr. Hamilton, speaking of Dr. Gooch's opinion concerning the source of hemorrhage in polypus, says: "But the experience of the author leads him to entertain a very different opinion on this subject; for, in the *first place*, in no instance to which he has been called has there been any bloody discharge from the surface of the polypus, notwithstanding any liberty he might have taken in pressing upon it, and in attempting to twirl it round."

* The italics are the writers. "The nature of non-malignant tumors consists essentially in this, that they are formed of the persistent elements of the body, and as such maintain their existence and participate in the general metamorphosis of the tissues. They may indeed be destroyed by softening and ulceration, but this is effected through the agency of causes which are not inherent in their nature, but are only accidental and exoteric."—(Vogel, Path. Anat., p. 240.

It will be remembered, that whenever the tumor of the case we have been noticing was touched, it bled some, though not freely, and a vaginal examination was always attended with more or less of a sanguineous discharge. According to Dr. Hamilton's experience, then, this could not have been a polypus. It differs from *simple induration* in being better defined, more distinct, less dense, more disposed to bleed and more easily broken down. From *fibrous tumor*—in being less dense, and ultimately by the ulceration and foetid discharge. From *simple ulceration*, attended with chronic enlargement and induration—in the well defined form of the tumor, its size and softness, and the termination of the two diseases. From *prolapsus uteri*—in its distinct attachment to the os and cervix; the repeated hemorrhages and the presence of the os alongside of the tumor. From *inversion of the uterus* in the absence of sensibility when touched, and the presence of the os *in situ*.

There are other morbid changes which may occur in the region where this tumor was developed, but of such distinctive characters that it is deemed unnecessary to touch on them. The pain enumerated among the symptoms present when I first saw the patient is somewhat difficult to reconcile. The surface of the tumor was insensible to the touch, while she suffered considerably from pain in the uterine region which was amenable to opiates.

The diagnostician will, I hope, profit by the gross neglect of the physician who treated this case one week without making a vaginal examination; and it will add value to Dr. Churchill's maxim that no case of uterine hemorrhage should be managed without making a vaginal examination. In conclusion I must express regret that I am unable to determine with what variety of malignant disease to class this case. I had determined at the beginning of my notes to trace it to its variety, but I cannot find a parallel, so I will leave the subject *sub judice*.

[TO BE CONTINUED.]

PART II.

Eclectic Department.

Clinical Lectures on Diseases of the Chest. Delivered at the Hospital for Consumption and Diseases of the Chest. By THEOPHILUS THOMPSON, M. D., F. R. S., Physician to the Hospital.

LECTURE I.

Importance of general symptoms. Physical signs. Hooke, in the seventeenth century, the proposer of auscultation. Laennec. Importance of visible signs. Hydatids expectorated. Cracked-pipkin sound. Modifications of movement of Chest by Phthisis. Pleurisy. Emphysema. Causes and treatment of asthma.

The hospital in which I have the honour of addressing you was founded for the relief of individuals suffering from a class of diseases, to which more than one-sixth of the annual mortality in this country is attributable. The opportunity for studying this class of diseases has hitherto been lamentably deficient, and no argument can be requisite to prove the desirableness of rendering this institution available for the communication of knowledge on the subject which it is specially adapted to illustrate. With this impression, it has been determined to deliver a course of clinical lectures; and whilst conscious of my own deficiency for the share of the task which I have undertaken, I am encouraged by the reflection, that my duty is not so much to communicate opinions as to assist you in the observation of facts, and by the conviction that knowledge acquired by your own attention, and made to fructify by the energy of your own mind, will take root with more certainty, and prove more productive, than any opinions which I might attempt to transfer to you complete and mature.

Let me remind you, at the onset, that lectures on the diseases of the chest involve far wider considerations than those connected with the science of auscultation alone. The probable duration of the disease, and the chances of its relief, have no exclusive relation to the extent of the local physical conditions. The various circumstances which characterize or modify the general constitutional state of the individual patient must be carefully considered before we can form a reasonable prognosis, or adopt a judicious treatment. Nevertheless, the various modes of physical investigation will, with propriety, engage a large proportion of attention, for it will well repay your careful study, and will lose its seeming complication, only by the devotion of patient care.

The systematic application of the ear to the investigation of the thoracic diseases is of modern introduction; but it is not generally known that the suggestion of such a method of inquiry was really made by one of our own countrymen, about two centuries ago.

Robert Hooke, surveyor to the city of London in the middle of the seventeenth century, who is said to have been the inventor of spring watches, and even in his boyhood to have exhibited great ingenuity in the construction of clocks, records his opinion that some modes of ascertaining the condition of the works of machinery might be extended with advantage to the investigation of the mechanism of animal life. The passage is worth your attention, and is at once so philosophical and definite, that you will probably feel some surprise that such a suggestion should have remained so long unnoticed.

"There may be a possibility," says Hooke, "of discovering the internal motions and actions of bodies by the sound they make. Who knows but that as in a watch we may hear the beating of the balance, and the running of the wheels, and the striking of the hammers, and the grating of the teeth, and a multitude of other noises—who knows, I say, but that it may be possible to discover the motions of internal parts of bodies, whether animal, vegetable or mineral, by the sounds they make; that one may discover the works performed in the several offices and shops of a man's body, and thereby discover what engine is out of order, what works are going on at several times, and lie still at others, and the like. I have this encouragement, not to think all these things impossible, though never so much derided by the generality of men, and never so seemingly mad, foolish, and fantastic, that as the thinking them impossible cannot much improve my knowledge, so the believing them possible may perhaps be an occasion for taking notice of such things as another would pass by without regard as useless. And somewhat more of encouragement I have also from experience, that I have been able to hear very plainly the beating of a man's heart; and 'tis common to hear the motion of the wind to and fro in the guts and other small vessels; the stopping of the lungs is easily discovered by the wheezing. As to the motions of the parts, one among the other, to their becoming sensible, they require either that their motions be increased, or that the organ be made more nice and powerful, to sensate and distinguish them as they are; for the doing of both which, I think it is not impossible but that in many cases there may be helps found."

It may be interesting to you to hear a few particulars of this ingenious philosopher, a short notice of whom may not be thought inappropriate to the occasion of our meeting.

Robert Hooke was born in 1635, and died in 1702. He was intended for the church, but a liability to headache interrupted his studies; and on attempting to become a painter, under the instruction of Sir Peter Lely, the same affliction, aggravated by the smell of paint, again changed his destination. He was for a time pupil to Dr. Busby. In 1655, he assisted Dr. Willis in his chemical illustrations at Oxford, and probably took a part in constructing the first air-pump, introduced by that eminent philosopher, Mr. Boyle. He stated some ideas regarding gravitation, approaching so nearly to those which immortalized Sir Isaac Newton, that this great man, with the ingenuousness of true philosophy, gave Hooke full credit for an approximation to that great discovery.

Robert Hooke, in his suggestions for an improved plan of building London, may be said to have anticipated some important measures of sanitary reform. He obtained through Cornelius Drebbel the first microscope used in England, and in 1629 discovered the cellular structure of plants, applying to the separate cells the name of utriculi. It is remarkable how little additional knowledge of importance was attained in this direction until 1833, when Robert Brown, in his work on *Orchideæ*, described the cytoblastema, or cell nucleus.

You will agree with me that Dr. Tilotson, the Archbishop of Canterbury, exercised a sound discretion in making Hooke, by patent, Doctor of Medicine.

This short account of the observations and intellectual character of Hooke, will not be unseasonable, if it serve to impress you with the conviction, appreciating as we do the ingenuity of his suggestions of helps to auscultation of the living body, that the rules for physical investigation are not arbitrary and mystical, but distinctly deducible from scientific laws.

The credit fairly due to Hooke in no measure detracts from the merit of Laenec. That distinguished physician,—with scarcely a hint to help him from previous observers, excepting an erroneous statement of Hippocrates, that a sound like boiling water could be heard in the chest of patients with watery effusion, and also a general remark by Double, the author of the *Semeiologie*,—in two short years of observation produced the first edition of that immortal work on *Diseases of the Chest*, which constitutes the solid groundwork of our knowledge of thoracic diagnosis. Laennec, however, was fallible, and made one important error: engrossed too much with the instruction derived through the ear, he disregarded the information obvious to the eye. To the great value of indications afforded by inspection, let me now make it the object of this lecture to call your special attention. The two sides of the chest, when in

the healthy condition, appear symmetrical in form, and similar in movement. A change in these respects, obvious to the practised eye, is usually induced by any serious disease of the lungs or pleura. Let me show you proofs of this statement.

The patient now before you was sent into this hospital under the idea that "one lung was gone from consumption." But although he had rapidly become thin, suffered from profuse night perspirations, and expectorated great quantities of "purulent matter," the aspect of the chest made me doubt the correctness of the opinion. You see that during inspiration the expansion on the two sides is equal, and that it is quite free even in the sub-clavicular regions. Your eye witnesses truly.

This patient, George S——, aged eighteen, a musician, is of phlegmatic temperament and orderly habits, but obliged, by his profession to keep late hours. He was admitted Nov. 5th, 1850. At the age of six, and several times subsequently, he is said to have had considerable enlargement in the region of the liver, which subsided, but seventeen months since recurred to an unusual extent, and did not yield to remedies. About eight months ago, he had violent attacks of cough, attended with expectoration of frothy yellow matter; in about three days the humour subsided, but he still continued to expectorate; the expectoration containing pieces of skin which he compares to gooseberry husks. Five months ago, fresh exasperation of his cough occurred, with expectoration of bags of humour "the size of a chestnut. When the cough was tolerably easy, these bags came up whole; when the cough was hard, they broke, their contents being first ejected, and their skins afterwards." He asserts that he could feel the bags come from the region of the liver. About two months since, fresh exasperation of his cough occurred, with imminent danger of suffocation. He says he felt a tearing sensation about the liver, as if something required to be ejected.

He soon began to throw up skins and a piece of what he called gut, about a foot long, and was obliged to tear these from his mouth to prevent suffocation. A week afterwards he had a fresh attack of cough, with expectoration of matter like yellow jelly covered with blood. He then brought up several pieces of shrivelled reddish skin, which, when stretched out, would each have covered the first. There was more blood mixed with them this time than before. For three or four days after, he continued spitting up a reddish humour containing small fragments of skin. Five weeks ago, during a period of two days, he spat up a large quantity of black congealed blood. Three days previously, and two days subsequently, to his admission, he expectorated a peculiar peach-coloured matter, which,

when submitted to the microscope, was found to contain pus and mucous globules, but no ecchinococci. He has always eaten much vegetable food, and been particularly fond of cabbages; has never had jaundice. His father was asthmatical; one of his brothers died of phthisis, another brother died at the age of four, three days after the bursting of a tumour, immediately below the ear, which in the course of a month had gradually acquired the dimensions of a goose's egg, and on bursting gave exit to a fluid resembling water. A fortnight before death, "a film formed over the sight of both eyes," and four days before his death he became "stone blind."

S—— is five feet seven inches high, his weight nine stone four pounds and a half. He sleeps well, but still has nightly perspirations. Expectorates about an ounce and a half daily, more easily on stooping or lying down.

There can be little doubt that this case is one of hydatids of the liver, which have by absorption found their way into the lungs, and been expectorated. Every circumstance, particularly the improvement in his strength and the absence of ecchinococci in the expectoration, may encourage the expectation of his perfect recovery. In the cases of hydatids which have come under my observation, I have generally ascertained one or more of the following circumstances—hereditary liability, vegetable diet, or blows. This patient cannot recollect receiving any blow on the side. He has, however, taken much vegetable food, and the brother's history is remarkable in relation to hereditary tendency. Two inquiries present themselves in reference to the origin of these hydatid cases: First, what is the mode of introduction of the ova? Secondly, is there impaired vitality of some organ, rendering it apt to harbour these animalcules? The influence of moist food on sheep and rabbits, as increasing their liability to hydatids, is a matter of observation, and from analogy we should recommend in the human subject, when affected with these parasites, a nourishing animal diet. The promotion of the hepatic secretion is also desirable; the introduction of bile into acephalocysts seeming to be destructive to these parasites, and favourable to their elimination. The treatment of S—— has consisted chiefly in a generous diet, the administration of tonics, with taraxacum, and the application to the hypochondriac region, of an ointment of iodide of potassium. His general strength is much improved, and the fulness in the hepatic region is subsiding.

The next patient whom I place before you has a very different aspect of chest. Even those of my audience who are at a distance can tell me at once that the lower half of the right side of the chest is flattened, and scarcely moves in inspiration.

You see also that the apex of the heart beats close to the left nipple, instead of its proper situation, namely, two inches below, and an inch within that part. You form a surmise that the cause of these conditions is contraction from pleurisy, and further observation confirms the correctness of the suspicion conveyed through the eye. If the heart were displaced by existing effusion, you would probably see bulging rather than depression. You put your hand on the flattened portion of the chest as the man speaks, and the vibration of the voice is distinctly perceived; effusion therefore does not exist, for that would interrupt the communication of the vocal thrill. You make percussion, and find dulness greater than false membrane on the pleura alone would produce, but the dulness lessens as you proceed upwards to the apex. The cardiac dulness is displaced, but not extended; that from the liver extends too much on the left. Accompanying the first sound of the heart, a loud murmur is heard near the apex, but not in the epigastrium or to the right of the sternum above the cartilage of the fourth rib.

This patient, Thomas C—, a sign-painter, aged nineteen, was admitted on the 23d of December; height, five feet, two inches, and three-quarters; weight, seven stone, thirteen pounds; vital capacity by spirometer, 114. He has always lived well till lately, and has usually resided in London; has no hereditary tendency to phthisis; his father died of dropsy at the age of sixty-two, and his mother of jaundice at sixty. He enjoyed good health till three and a half years ago, whilst on a voyage, when he received a severe blow on the right side of the chest, after which he expectorated about four ounces of blood. Six months afterwards he had rheumatic fever and inflammation of the lungs, and his sputa were streaked with blood. About eighteen months since he had a recurrence of hæmoptysis, he thinks to the amount of two quarts in a week, and since that time his breathing has been oppressed, and some degree of hæmoptysis has occurred whenever he has attempted to resume his occupation. He complains of a sensation of tightness of the chest on the right side; relieved by making pressure on the left; cough slight; expectoration frothy; respirations 32; pulse 88, not strong; digestive organs natural; enjoys exercise, but sleeps badly, and perspires at night.

In this case the cardiac murmur obviously indicates regurgitation from the left ventricle into the corresponding auricle. This regurgitation, however, is probably not considerable, for the circulation in the radial artery is not disturbed or irregular. There are various circumstances in this man's condition, encouraging us to hope that he will escape phthisis. The mitral disease is probably due to rheumatism, and such disease is ra-

ther inapt to occur in phthisical subjects. The dulness on percussion is least at the apex of the lungs, where tubercular disease is most common, and greatest at the part corresponding to the blow, which, by inducing inflammation and occasioning consolidation of the pulmonary structure, may have rendered the thrill of the voice more than usually observable. The frequent occurrence of hæmoptysis might excite suspicions of a tubercular tendency, but any such apprehension is qualified by the fact that this occurrence was preceded and probably induced by a blow. Some friction sound at the border of the contracted part of the chest announces that some inflammation of the pleura still exists. A blister has been in consequence applied, and the patient is improving under the use of iodide of potassium.

The next patient, George S——, you observe, moves the right side of the chest moderately during inspiration, especially at the upper part, but the left side scarcely at all; you suspect tubercular disease from this fact, and further examination confirms that opinion. Percussion yields a dull sound over the whole of the left side, and in the sub-clavicular region a sound is elicited which some of you will recognise as amphoric, like that produced by filliping the distended cheek, and doubtless arising from the proximity of a considerable cavity almost full of air. If a smart stroke be given whilst the patient's mouth is open, you hear the sound denominated by the French, *bruit de pot fêlé*, resembling, as the designation implies, the noise produced by striking a cracked-pipkin. You may imitate the cracked-pipkin sound by doubling the hands together rather loosely, and striking the back of one of them against the knee in such a manner as to allow some escape of air. The production of this particular sound by percussion of the chest, is doubtless owing to the proximity of a considerable cavity, having yielding walls and free communication with one or more large bronchial tubes. If, whilst the patient's mouth is open, you strike smartly over such a cavity, air escapes freely and suddenly from it into the bronchus, and thus the peculiar sound in question is produced. S—— has no hereditary tendency to phthisis, but for fifteen years has been scarcely free from either syphilis or gonorrhœa. The free use of mercury, an unsettled life, and, we may suppose, an uneasy conscience, may have proved adequate for inducing the disease. He began to cough three years since, and a year afterwards to lose flesh; ten months ago he expectorated about four ounces of blood; his sputa green and viscid, average in quantity ten ounces in the twenty-four hours. Occasionally the cracked-metal sound is suspended, probably in consequence of plugging of the bron-

chial tube by the viscid secretion. Pulse 84; respirations 32. There is a chronic ulcer on the leg, which we shall not attempt to heal, for he always feels better when it discharges freely. He has fluctuated considerably, but on the whole improved in strength under the use of cod-liver oil. Belladonna pills and various measures have been adopted for the cough, but the only medicine which relieves him is the following linctus:—Hydrochlorate of morphia, one grain; diluted hydrochloric acid, fine minims; diluted hydrocyanic acid, half a fluid drachm; syrup of squills, one fluid ounce. Water, one fluid ounce. Mix. One drachm to be taken when the cough is troublesome.

The last patient I have to introduce to you to-day is Charles B——. As you watch the movement of his chest, let me remind you that the advance of either of the five upper or “thoracic ribs” in ordinary inspiration varies from .02 to .07 of an inch, and in an extraordinary effort may extend to about two inches; whilst the four or five inferior ribs, which obey the influence of the diaphragm, move outwards ordinarily from .25 to .3 of an inch, or in an extreme inspiration about an inch and a half. You will observe that the upper part of B——’s chest moves in correspondence with this rule; but that the lower part, instead of advancing according to this rule, absolutely recedes. By means of Dr. Sibson’s chest-measurer, (*Medico-Chirurgical Transactions*, vol. xxxi.) we may determine the exact amount of this deficiency of movement; or if you have not had sufficient practice for the dexterous management of this instrument, try that of Dr. Quain, in which the sliding joints are dispensed with. Even this instrument, however, excepting to those practised in its use, is not more trustworthy than the eye. In practice, the attentive eye soon detects the difference of form and movement associated with the diseases to which I have referred; but it is doubtless a great advantage to be able to state to others, by the aid of a graduated instrument, the exact amount of difference. If you strike this patient’s chest, the sound elicited is clear; and you have already recognised the characteristic movement of emphysema. The diaphragm in contracting affords space for the expansion of the lungs, but the pulmonary cells already filled cannot admit more air; atmospheric pressure, therefore, takes effect, and the ribs are forced inwards. This patient is a tailor. He has been subject to colds for ten years, owing, he thinks, to transitions from hot workshops lighted with gas into the cold air. During the last three years these colds have become almost incessant. He has suffered from shortness of breath and cough, and occasional paroxysms of dyspnoea. For the last ten months he has kept his room; sleeps in the sitting posture, with the body

bent forwards: expectoration partly viscid, partly frothy; sonorous and sibilous rhonchi heard all over the chest; pulse 88; respirations 26. He has no hereditary tendency to phthisis or to gout, and has never had hæmoptysis. Asthma would seem in this case to be a consequence of chronic bronchitis and emphysema. It is a disorder depending probably on a peculiar susceptibility of that part of the nervous system which supplies the bronchial tubes and pulmonary cells, but a susceptibility which is rarely manifested in a decided manner until some additional influence is exerted, such as indigestion, gout, bronchitis, or cardiac disturbance. The emphysematous condition of the lungs, which frequently accompanies asthma, is well entitled to your careful study. It is connected with an atrophied condition of the pulmonary cells. If at any early period of the disease you make a microscopical examination of the affected lungs, you will observe a number of little bright dots, like oil-globules, on the membrane which sustains the vessels. At a later period you will find the membrane cribriform, and its meshes of vessels more widely separated than in the natural condition. Partly in consequence of the deficient supply of blood, and partly, it may be, from venosity of the circulating fluid, diminished liability to phthisis is characteristic of emphysema. When cicatrized cavities are found in any part of the lungs, it is common to discover emphysematous portions in the adjoining structure, probably because the obliteration of vessels which attends such cicatrization tends to diminish the supply of blood to the neighbouring cells. Even when you have reason to suspect the existence of phthisis in emphysematous subjects, you have ground to hope that the tubercular disease will be slow in its progress.

There is something very capricious in the asthmatic susceptibility. Some individuals have paroxysms only in town, some only in the country; others indifferently in town or country, but experience relief from the attack by immediate change either to one or the other. The late duke of Sussex, who became asthmatical at a very early age, always escaped the paroxysms when at the foot of Vesuvius, but suffered repeatedly when residing in Naples. Measures which immediately relieve the fit in one individual are useless in another. Sometimes, indeed, in the same subject a remedy at one time efficacious will at another time utterly fail. Thus, in our patient B—, smoking stramonium at first afforded instantaneous relief, but subsequently failed. The inhalation of chloroform almost invariably relieves the asthmatical paroxysm, but its use is not to be indiscriminately recommended. In B— the pulsation of the heart can be seen at the epigastrium, indicating,

what is very common in such cases, some dilatation of the right side of the heart, as a consequence of the obstruction in the pulmonary circulation; and what you know of the influence of chloroform on the heart should make you cautious of its administration under such a complication.

It is therefore satisfactory to have other measures in reserve, and in many instances I have found remarkable relief experienced from the use of blotting-paper dipped in a saturated solution of nitrate of potash, dried, and then set fire to on a plate so placed as to expose the patient to the vapour. Whether the efficacy of this measure be owing to empyreumatic bituminous vapour or to nitrogenous fumes, I cannot tell. I can neither explain its remarkable efficacy, in some instances, nor its failure in others; but it is a remedy of real value; and I think I owe the suggestion to Mr. Harrison, of Broughton, near Manchester.

But it is not enough to relieve the paroxysms: you must aim to accomplish permanent amendment, and with this view must not be satisfied till you have rectified every disordered function, and improved to the greatest possible extent the nervous tone of your patient. Above all, endeavour to subdue any accompanying bronchitis. In many instances grain doses of mercurial pill with antimony, when watchfully regulated so as to avoid salivation, and gradually suspended, as the rhonchus moistens, are with this view singularly efficacious. When, however, there is the least threatening of consumption, it is desirable to find a substitute for the mercurial pill, and the combination most similar in effect is probably iodide of potassium mixed with antimony, which latter medicine would seem to have a tendency to direct the influence of the iodide of potassium to the bronchial tubes. B—— has therefore taken a mixture according to the following prescription:—iodide of potassium, three grains; potassio-tartrate of antimonial wine, ten minims; tincture of henbane, fifteen minims; decoction of sarsaparilla, one fluid ounce; make a draught, to be taken three times a day; and the benefit has been obvious; the breathing having become easier, the paroxysms no longer recurring, and the sibilant rhonchus, which was formerly audible during both inspiration and expiration, being confined to the latter, thus showing that the more minute tubes, into which the air was noisily forced by inspiration, have recovered their natural condition.

Is it probable that alum might prove useful as a remedy in asthma? It is almost a specific in lead colic and in whooping-cough—diseases not without analogy to asthma, in the paroxysmal character of the attacks, and perhaps in the correspond-

ence of the nervous fibres through which the morbid influence is conveyed.

When a medicine is proved to have remarkable efficacy in any one disease, we may often extend its use to the treatment of other affections, if we can succeed in discovering what particular condition the remedy modifies, and in 'classifying together such other disorders as present a distinct analogy in reference to this condition.—[*London Lancet*.

On the Employment of Mercurials in the Treatment of Typhoid Fever, (Black Sulphuret of Mercury and frictions of Mercurial Ointment,) after M. Serres' method. By M. BECQUEREL. Translated from the French, with additions, by JAMES BYRAN, M. D., Prof. of Institutes of Medicine in the Philadelphia College of Medicine.

In 1847, Dr. Serres presented to the Institute a series of memoirs designated to elucidate the nature and treatment of typhoid fever. It is on these two points of the history of this disease that the learned member of the Institute desires again to call the attention of the medical profession, to point out a new method of treatment, destined to prevent the most formidable accidents, to moderate the most dangerous symptoms, and, in fine, to change the malignant typhoid fever into a mild form of disease.

Instructed by the administration of the hospitals, to replace, temporarily, Dr. Serres in the Hospital de la Pitié, I have thought that I could not do better than continue his treatment of this fever and to submit it to new experiments. The following is the result :

It is proper to state Dr. Serres' treatment, and the effects obtained. The mercurial treatment consists in the use of the black sulphuret of mercury, internally, (*Æthiop's mineral*), in doses which vary from sixty grains to two drachms; and in the external use of mercurial friction on the abdomen, (*onguent Napolitain*), in doses which vary from fifteen to sixty grains per diem. This treatment was continued eight, ten, or twelve days, or until the symptoms of the disease ceased.

The following are the results as stated by Dr. Serres in his communication to the Institute: The employment of the black sulphuret of mercury in the above doses, may nearly always be continued eight, ten, or twelve days, before salivation will occur; and when it does take place, it never is so severe as to be dangerous. Mercurial frictions on the abdomen always disperse the sore patches, and rapidly diminish tympanitis of the abdo-

men. Under the combined influence of these two means, the diarrhœa is ameliorated, the frequency of the pulse is diminished, the fever subsides, the headache and delirium are much lessened.

The duration of the disease is not always abridged, it generally continues its usual course, that is to say, it lasts three or four weeks: but it comes without any serious complications, and especially without that adynamic condition which is so dangerous. Such, are, in a few words, the conclusions of the learned physician of La Petié.

Before stating the results which I have obtained in the treatment of fifteen cases of typhoid fever, all very bad cases, and subjected to this treatment, I must add that I have followed closely the above formulary entirely, so that to no other causes can the results obtained by me, more fortunate perhaps than even those of Serres, be attributed. The black sulphuret was used as soon as the patients entered. I commenced with one gramme (about $15\frac{1}{2}$ grains) per day, in powder or in pills made with gum. This amount was given in five or six doses. If the improvement did not commence at the second or third day, I increased the amount up to one and a half grammes, or even to two grammes. I have never exceeded the latter quantity. I suspended its use as soon as convalescence commenced, nor did the intensity of the salivation ever make it necessary to remit the use of the medicine.

Frictions on the abdomen, with the Napolitain ointment, were made of the strength of one gramme of sulphuret to 16 grammes of mercurial ointment in two frictions, with the application of a poultice to favor absorption, one gramme of sulphuret to 24 grammes of mercurial ointment, used in three applications, and, finally, two grammes of black sulphuret to thirty grammes of ointment, also in three applications.

Every two days the abdomen was washed with soap, in order to favor the process of absorption.

The rest of the treatment was, 1st. Ice seltzer water, or lemonade, for drink. 2d. Laxatives or mild clysters where the bowels were constipated. 3d. In adynamic cases, of which there were five, I added to the use of black sulphuret that of musk, in doses of from one to twenty-five or thirty grammes per day. This medicine was discontinued when the delirium and general agitation diminished. Fifteen patients, affected with low typhoid fever, were fully subjected to the effects of the mercurials. There were ten males and five females. The ten males were aged: two of sixteen, two of seventeen, two of eighteen, one of twenty, two of twenty-two, and one of thirty-six years of age. The ages of the five females were as

follows: one of fifteen, one of eighteen, one of twenty, and two of twenty-one years of age.

The period of life at which this disease usually appears, is a very striking feature in its character. According to Louis and Chomel, of 255 cases, 78 were from fifteen to twenty years, 95 from twenty to twenty-five, 54 from twenty-five to thirty, 22 from thirty to forty, and five only from forty to fifty years of age, while but one was above fifty. [As to the cause of the disease, may it not be connected with certain evolutions of the system at this time? May not the mucous diathesis which so commonly exists under the circumstances in which this fever is developed, be, if not a cause of the disease, an aggravating circumstance? May not the general depravation of the nutritive functions, the blood and the tissues, be traced more or less to this depraved condition of the mucous membranes, thus developed? Does not both the prophylactic and curative mode of treatment, usually found effective, add strength to this belief, viz. change of air, diet, and occupation, with the use of nitrate of silver, oil of turpentine, or, as in the above cases, mercury?—J. B.]

The ten males, continues our author, affected with typhoid fever, presented the following forms:—four, the ordinary abdominal form, with stupor, cephalalgia, &c.; five, the adynamic form, of the lowest kind; one, the ataxic form, with delirium and general agitation. The five females presented twice the ataxic form, once the ataxo-adynamic; once, the adynamic; and once, the ordinary abdominal form. These fifteen cases entered the hospital some time after the first attack, having been probably without any treatment at their homes. In all, the treatment was commenced on the day after they entered: the following are the effects it produced on the principal symptoms, and the general disease.

The Fever.—Under the influence of the first doses of the black sulphur and of the frictions, the skin always became cooler, less dry, and in some cases, it became moist, or perspired. At the same time, the pulse diminished in force and frequency; this occurred even in the only case of death that took place, which was from perforation of the intestine.

The Tongue.—At first dry, rough, and the gums and lips dark and sooty—continued thus until the advent of salivation. Of the fifteen cases, salivation took place in twelve; in two it did not occur, and the patients recovered very well: in these two cases, the tongue did not become moist until the cessation of the fever. In the case of perforation of the intestine, the tongue remained dry to the end. [This, however, is not always the case, as we well know. We well remember a case that

was pronounced, from the general improvement of the symptoms, the moisture of the tongue among them, convalescent, by a distinguished clinical teacher of our city, which, however, was brought in on the next clinic day *dead*. A perforation having taken place in the small intestine, which caused death very suddenly. This, in fact, is the great danger in the disease, and it occurs not unfrequently when least expected.—J. B.]

In the twelve cases where salivation was developed, it took place twice on the sixth day of the treatment; three times on the seventh day; four times on the eighth day; once on the twelfth; and once on the thirteenth. Except one case, in which the salivation and swelling of the gums were intense, and continued for twelve days, it was in all the rest very light and unimportant. In no other case did it continue beyond four or five days, and required no medical treatment. We may, in reference to the salivation, establish the following propositions:

1. In cases of typhoid fever in its ordinary form of medium intensity, salivation takes place rapidly, it is decided, and continues during a considerable portion of the time of convalescence; in general, it announces the occurrence of the latter.

2. In more serious cases, salivation occurs at a later period; it is in general less severe, and continues only during the first days of convalescence; it precedes by a few days the cessation of the fever, and constitutes a sign which indicates an early cure. Sometimes, however, it does not occur until the instant that the fever ceases.

3. In extremely low cases, salivation is induced with great difficulty, and until it takes place, the case must be considered as dangerous. It is in these cases that we must insist for a long time on large doses of the black sulphuret and mercurial frictions.

4. In some cases of medium intensity, salivation is not induced at all. The treatment was continued in each of the fifteen cases, not only until the supervention of salivation, but to the cessation of the fever, and the amelioration of all the symptoms.

The Tympanitic Abdomen, except in the case of perforation of the intestine, always diminished rapidly, even from the commencement of the treatment; this result must be attributed to the combined influence of the black sulphuret.

In reference to the *stools*, in the fifteen patients, there were two affected with constipation, which clysters alone removed. The black sulphuret was ineffectual in reliving the system. In two other cases, the black sulphuret produced a diarrhœa, which did not exist before, and which since its employment produced

one or two stools per day. The diarrhœa, which was always ushered in by five or six liquid stools, diminished at once when the black sulphuret was taken. In eight cases, finally, the diarrhœa was neither augmented or diminished; it continued its course, and diminished only as the other symptoms subsided.

Whenever the mercurial frictions were used on a part where the *sore spots* existed (abdomen,) they disappeared in from twenty-four to thirty-six hours; none remained but those on the lower part of the thorax, on the chest, or elsewhere.

In relation to the *cephalalgia*, no appreciable effect was produced in those cases; it did present a very severe symptom.

Where delirium existed pretty severe (four patients,) musk, in doses of 25 to 30 grains, with black sulphuret, was given, until this symptom ceased. In these four cases the improvement was rapid, and five days was the longest period of its existence. In one patient, in which cold water on the head was applied for three days, the delirium was very violent, and we were obliged to resort to the straight waistcoat.

The *expression of the face* did not disappear until the other symptoms had diminished; the black sulphuret acted upon it only secondarily.

The same may be said of the *cough*, the *sibilant râle*; there were, for the rest, no serious symptoms connected with the chest. The patient who died on account of intestinal perforation must be excepted from this list, having had, when he entered, symptoms of severe bronchial engorgement. The mercurials did not affect these symptoms, except as they affected the general disease. In no case was there serious hæmorrhage.

The *duration of the treatment* was, in four cases seven days, in three eight, in one nine, in three ten, in one twelve, in one fifteen, one sixteen, and one seventeen. The medium duration was ten days.

The minimum of the black sulphuret used in the treatment was seven grammes (a little over 15 grs. each), the maximum was 24 grammes. The minimum of mercurial ointment used was 112 grammes, and the maximum 360 grammes; the medium was 12 gr., 30 in each case, and 200 grammes of Neapolitan ointment.

The *whole duration of the disease* varied somewhat. The table added to my memoir gives the following results:—one case twelve days, two thirteen, three fourteen, three fifteen, one sixteen, one eighteen, one twenty, and one twenty-three days. The medium duration of the fever was sixteen days.

The patient that died of the perforation was a male of 36 years, strong and robust, who was admitted on the eighteenth day of the disease; the symptoms of the typhoid fever were

marked by those of a very severe bronchitis, which for the first few days entirely concealed the principal disease; he was treated during five days, from the 8th to the 12th, by two general bleedings, ipecacuanha, and a cathartic. The bronchial engorgement, improved by this energetic treatment, precluded the use of the mercurials, either internal or external, before the twelfth day; it produced happy results as usual. The symptoms improved, and he was getting better, when, without known cause, the patient, on the nineteenth day, was seized with symptoms of acute peritonitis, and died on the twentieth day. The autopsy detected the intestinal perforation.

Duration of the convalescence.—In all the cases it was simple, without complication or accident. In one female there was an escar on the sacrum, which required a month to get well. With this exception, the patients remained in the hospital from eight to twenty-three days.

[In reference to the duration of the disease, we quote the following from Dr. Wood's "Practice:": "Even the mildest cases run on to the fourteenth and fifteenth day: those of a severer character seldom become convalescent before the end of the third or fourth week: and not unfrequently we witness recoveries even after the sixth week. The average duration of cases may be stated at from *twenty* to *thirty* days. The disease seldom lasts longer than sixty days, though it has no fixed limits." This statement does not agree with that of Becquerel, who places the average at sixteen days. Doubtless, there is much difference in this respect between hospital and private practice. The latter can never be altogether safe for the general practitioner to follow, for several reasons. 1. The patients are frequently paupers and broken-down constitutions, either from their poverty or their vices. 2. The atmospheric and other conditions of a general or fever hospital can never be equal in salubrity to those of a respectable private residence. Unfortunately for the general practitioner, especially in our country, the writers on these and kindred diseases are mostly hospital physicians. It is much to be regretted that our country practitioners do not more frequently write out the result of their ample experience. We quote with pleasure the following remarks made by a practitioner of the latter class. He is not alone in recommending opium for these cases of disease.

"For the last twelve years, opium, in *four* and *five* grain doses, has been my main remedy in all forms of typhoid fever. In fact, when I use it at all in fever it is in four or five grain doses. I claim to have demonstrated beyond all reasonable doubt, by a long and careful observation and experience, that while the maximum doses of the schools are of doubtful utility,

and often prove injurious in fever, by increasing the dryness of the skin, aggravating the pain in the head, &c., &c., a *five grain dose* will, nineteen times in twenty, produce free perspiration and relieve every unpleasant symptom. The notion that so generally prevails among the profession that opium cannot be used to advantage in fever while there is determination to the brain, is certainly erroneous, if it is given in the doses which I recommend, unless there is *actual* inflammation of the membranes, and cases of this kind are extremely rare, in my opinion, Dr. Clutterbuck to the contrary notwithstanding." Dr. A. G. Henry, Boston Med. and Surg. Journal, vol. xii., p. 13.

Professor J. K. Mitchell, of this city, relies chiefly on nitrate of silver, which he uses until the stools assume the "metallic lustre" produced by this remedy. Those who are familiar with Dr. M.'s practice know that he is very successful in his treatment of typhoid fever by the nitrate. On the other hand, a respectable *country* practitioner, Dr. John L. Altee, of Lancaster, Pa., says: "Should the fever be prolonged beyond the third or fourth day, and its character become fairly established, I then resort to the *acetate of lead*, in doses of from one to three grains, carefully and perfectly dissolved in a few drops of vinegar and half an ounce of river or distilled water, and given every two, three, or four hours, according to the urgency of the symptoms; and the treatment is steadily persevered in as long as the enteric symptoms continue. He adds: "I do not know that I have ever lost a patient where the case was treated from the commencement as above indicated." (Wood's Practice.) It must not be forgotten that in all these cases the bowels are first moved, and their contents more or less evacuated. My late friend, Dr. Parish, depended almost entirely on the use of the oil of turpentine, together with carbonate of ammonia and neutral mixture. He generally formed what he denominated his "Turpentine Julep," by rubbing it up with gum acacia, loaf sugar, and water, adding sometimes tincture of opium, especially when the oil disturbed the bowels. This practice is followed pretty generally by his pupils, who are now the prominent writers and practitioners of our city. For ourselves, we have resorted to all these measures, and find that what Hippocrates calls the "temperament" of the year or season, or some other cause, influences very much the effects of these as well as other remedies. The last cases of severe typhoid fever which we treated, nothing was so effectual, under proper restrictions, as good *port wine*, of which the patients consumed about a pint daily, sometimes more.—J. B.]—*N. Y. Journal of Medicine.*

Pathological Appearances in cases of Strangulation or Hanging, Apoplexy, &c. (from Professor J. H. BENNET's Clinical Lectures.)—*The Pathological Laws which Regulate Diseased Functions of the Nervous System.*—For the purpose of diagnosis and treatment, it is a matter of great importance to attend to the following generalizations :

(1.) *The amount of fluids within the cranium must always be the same so long as its osseous walls are capable of resisting the pressure of the atmosphere.* There are few principles in medicine of greater practical importance than the one we are about to consider—the more so, as many able practitioners have lately abandoned their former opinions on this head, and on what I consider to be very insufficient grounds. On this point, therefore, I cannot do better than condense and endeavour to put clearly before you the forcible arguments of the late Dr. John Reid, with such other considerations as have occurred to myself.

That the circulation within the cranium is different from that in other parts of the body, was first pointed out by the second Monro. It was tested experimentally by Dr. Kellie of Leith, ably illustrated by Dr. Abercrombie, and successfully defended by Dr. John Reid. The views adopted by these distinguished men were, that the cranium forms a spherical bony case, capable of resisting the atmospheric pressure, the only openings into it being the different foramina by which the vessels, nerves, and spinal cord pass. The encephalon, its membranes and blood-vessels, with perhaps a small portion of the cerebro-spinal fluid, completely fill up the interior of the cranium, so that no substance can be dislodged from it without some equivalent in bulk taking its place. Dr. Monro used to point out that a jar, or any other vessel similar to the cranium, with unyielding walls, if filled with any substance, cannot be emptied without air or some other substance taking its place. To use the illustration of Dr. Watson, the contents of the cranium are like beer in a barrel, which will not flow out of one opening, unless provision be made at the same time that air rushes in. The same kind of reasoning applies to the spinal canal, which, with the interior of the cranium, may be said to constitute one large cavity, incompressible by the atmospheric air.

Before proceeding further, we must draw a distinction between pressure on, and compression of, an organ. Many bodies are capable of undergoing a great amount of pressure without undergoing any sensible decrease in bulk. By compression must be understood, that a substance occupies less space from the application of external force, as when we squeeze a sponge,

or compress a bladder filled with air. Fluids generally are not absolutely incompressible, yet it requires the weight of one atmosphere, or fifteen pounds on the square inch, to produce a diminution equal to $\frac{1}{20,000}$ th part of the whole. Now this is so exceedingly small a charge upon a mass equal in bulk to the brain, as not to be appreciable to our senses. Besides, the pressure on the internal surface of the blood-vessels never exceeds ten or twelve pounds on the square inch, during the most violent exertion, so that, under no possible circumstances, can the contents of the cranium be diminished even the $\frac{1}{20,000}$ th part. When the brain is taken out of the cranium, it may, like a sponge, be compressed, by squeezing fluid out of the blood-vessels; but during life, surrounded, as it is, by unyielding walls, this is impossible. For let us, with Abercrombie, say that the whole quantity of blood circulating within the cranium is equal to 10; 5 in the veins, and 5 in the arteries: if one of these be increased to 6, the other must be diminished to 4, so that the same amount, 10, is always preserved. It follows that, when fluids, are effused, blood extravasated, or tumours grow, a corresponding amount of fluid must be pressed out, or of brain absorbed, from the physical impossibility of the cranium holding more matter. At the same time, it must be evident that an increased or diminished amount of pressure may be exerted *on* the brain, proportioned to the power of the heart's contraction, the effect of which will be, not to alter the amount of fluids within the cranium, but to cause, using the words of Abercrombie, "a change of circulation" there.

Dr. Kellie performed numerous experiments on cats and dogs, in order to elucidate this subject. Some of these animals were bled to death by opening the carotid or femoral arteries; others by opening the jugular veins. In some the carotids were first tied, to diminish the quantity of blood sent to the brain, and the jugulars were then opened, with the view of emptying the vessels of the brain to the greatest possible extent; while, in others, the jugulars were first secured, to prevent as much as possible the return of the blood from the brain, and one of the carotids was then opened. He inferred, from the whole inquiry, which was conducted with extreme care, "That we cannot, in fact, lessen, to any considerable extent, the quantity of blood within the cranium by arteriotomy or venesection; and that when, by profuse hemorrhages destructive of life, we do succeed in draining the vessels within the cranium of any sensible portion of red blood, there is commonly found an equivalent to this spoliation in the increased circulation or effusion of serum, serving to maintain the plenitude of the cranium."

Dr. Kellie made other experiments upon the effects of position immediately after death from strangulation or hanging. He also removed a portion of the unyielding walls of the cranium in some animals, by means of a trephine, and then bled them to death; and the differences between the appearances of the brain in these cases, and in those where the cranium was entire, were very great. One of the most remarkable of these differences was its shrunken appearance in those animals in which a portion of the skull was removed, and the air allowed to gravitate upon its inner surface. He says: "The brain was sensibly depressed below the cranium, and a space left, which was found capable of containing a teaspoonful of water."

It results, from these inquiries, that there must always be the same amount of fluids within the cranium so long as it is uninjured. In morbid conditions these fluids may be blood, serum, or pus; but in health, as blood is almost the only fluid present (the cerebro-spinal fluid being very trifling), its quantity can undergo only very slight alterations. There are many circumstances, however, which occasion local congestions in the brain, and consequently unequal pressure on its structure, in which case another portion of its substance must contain less blood, so that the amount of the whole, as to quantity, is always preserved. These circumstances are mental emotions, hemorrhages, effusions of serum, and morbid growths. Some congestions, or local hyperæmias, in themselves constitute morbid conditions; and nature has, to a great extent, provided against their occurrence, under ordinary circumstances, by the tortuosity of the arteries and the cerebro-spinal fluid, described by Magendie.

The views now detailed had been very extensively admitted into pathology, when Dr. Burrows, of St. Bartholomew's Hospital, endeavoured to controvert them, first in the Lumleian Lectures of 1843, and subsequently in a work published in 1846, entitled, "On Disorders of the Cerebral Circulation, and on the Connection between Affections of the Brain and Diseases of the Heart." Dr. Burrows, however, evidently formed the most confused notions of the doctrine we are advocating; for, instead of stating it as propounded by its authors, he *actually misrepresented it*, as Dr. Reid pointed out. Thus, he is always combatting the idea that blood-letting, position, strangulation, &c., cannot affect the *blood in the brain*; whereas the real proposition is, that they cannot alter the *fluids within the cranium*. By thus confounding blood with fluid, and brain with cranium, he has only contrived to overthrow a theory of his own creation.

Dr. Burrows has brought forward several observations and experiments, which he considers opposed to the theory now

advocated. His facts are perfectly correct. I myself have repeated his experiments on rabbits, and can confirm his descriptions. It is the inferences he draws from them that are erroneous. For the paleness which results from hemorrhage, and the difference observable in the color of the brain, when animals, immediately after death, are suspended by their ears or by their heels, is explicable by the diminished number of coloured blood particles in the one case, and by their gravitation downwards in the other. That the amount of fluid within the cranium was in no way affected, is proved by the plump appearance of the brains figured by Dr. Burrows, and the total absence of that shrunken appearance so well described by Dr. Kellie.

Neither does our observation of what occurs in asphyxia or apnœa oppose the doctrine in question, as Dr. Burrows imagines, but rather confirms it. On this point the following observations by Dr. John Reid are valuable. He says: "If any circumstance could produce congestion of the vessels within the cranium, it would be that of death by hanging; for then the vessels going to and coming from the brain are, with the exception of the vertebral arteries, compressed and then obstructed. These two arteries, which are protected by the peculiarity of their course through the foramina of the transverse processes of the cervical vertebræ, must continue for a time to force their blood upon the brain, while a comparatively small quantity only can escape by the veins. Indeed, the greater quantity of blood carried to the encephalon by the vertebrals returns by the internal jugulars, and not by the vertebral veins, which are supplied from the occipital veins of the spinal cord; and the anastomoses, between the cranial and vertebral sinuses, could carry off a small quantity of the blood only, transmitted along such large arteries as the vertebrals. And yet it is well known that there is no congestion of the vessels within the cranium after death by hanging, however gorged the external parts of the head may be by blood and serum." This is admitted by Dr. Burrows, although he endeavors to get rid of so troublesome a fact by a gratuitous hypothesis, which will not bear a moment's examination, but for the refutation of which I must refer to the works of Dr. Reid.*

On the whole, whether we adopt the expressions of local congestion, of change of circulation within the cranium (Abercrombie), or of unequal pressure (Burrows), our explanation of the *pathological* phenomena may be made equally correct, because each term implies pretty much the same thing. But if

* Monthly Journal, August, 1846: Physiological, Anatomical, and Pathological Researches, No. XXV.

we imagine that venesection will enable us to diminish the amount of blood in the cerebral vessels, the theory points out that this is impossible, and that the effects of bleeding are explained by the influence produced on the heart, the altered pressure on the brain, exercised by its diminished contractions, and the change of circulation within the cranium thereby occasioned.

I have entered somewhat fully into this theory, because, independent of its vast importance in a practical point of view, it is one which originated in, and has always been maintained by, the Edinburgh School of Medicine. Singular to say, notwithstanding the obvious errors and fallacies in Dr. Burrows' work, no sooner did it appear than the whole medical press of England and Ireland adopted its conclusions, and even Dr. Watson, in the last edition of his excellent work, also abandoned the theory of Monro, Kelly, and Abercrombie. But so far is this theory, concerning the circulation within the cranium, from being shaken by the attack of Dr. Burrows, that it may be said now to stand on a firmer basis than ever, owing to that attack having drawn forth the convincing reasoning and unanswerable arguments of so sound an anatomist, physiologist, and pathologist as the late Dr. John Reid.

(2.) *All the functions of the nervous system may be increased, perverted, or destroyed, according to the degree of stimulus or disease operating on its various parts.* Thus, as a general rule, it may be said that a slight stimulus produces increased or perverted action; whilst the same stimulus, long continued or much augmented, causes loss of function. All the various stimuli, whether mechanical, chemical, electrical, or psychical, produce the same effects, and in different degrees. Circumstances influencing the heart's action, stimulating drinks or food, act in like manner. Thus, if we take the effects of alcoholic drink, for the purpose of illustration, we observe that, as regards combined movements, a slight amount causes increased vigour and activity in the muscular system. As the stimulus augments in intensity, we see irregular movements occasioned, staggering, and inability of directing the limbs. Lastly, when the stimulus is excessive, there is complete inability to move, and the power of doing so is temporarily annihilated. With regard to sensibility and sensation, we observe cephalalgia, tingling, and heat of skin, tinnitus aurium, confusion of vision, muscæ volitantes, double sight, and lastly, complete insensibility and coma. As regards intelligence, we observe at first rapid flow of ideas, then confusion of mind, delirium, and lastly, sopor and perfect unconsciousness. In the same manner pressure, mechanical irritation, and the various organic diseases produce

augmented, perverted, or diminished function, according to the intensity of the stimulus applied, or amount of structure destroyed.

Thus it has been shown, that excess or diminution of stimulus, too much or too little blood, very violent or very weak cardiac contractions, and inflammation or extreme exhaustion, will, so far as the nervous functions are concerned, produce similar alterations of motion, sensation, and intelligence. Excessive hemorrhage causes muscular weakness, convulsions, and loss of motor power, perversions of all the sensations, and lastly, unconsciousness from syncope. Hence the general strength of the frame cannot be judged of by the nervous symptoms, although the treatment of these will be altogether different, according as the individual is robust or weak, has a full or small pulse, &c. These similar effects on the nervous centres from apparently such opposite exciting causes, can, it seems to me, only be explained by the peculiarity of the circulation previously noticed. A change of circulation within the cranium takes place, and whether arterial or venous congestion occurs, pressure on the organ is equally the result. The importance of paying attention to this point in the treatment must be obvious.

(3.) *The seat of the disease in the nervous system influences the nature of the phenomena or symptoms produced.* It is a matter of very great importance to ascertain how far certitude in diagnosis may be arrived at, and the seat of the disease ascertained. On this subject it may be affirmed that, although clinical observation combined with pathology has done much, more requires to be accomplished. As a general rule, it may be stated that disease or injury of one side of the encephalon, above the decussation in the medulla oblongata, especially influences the opposite side of the body; whilst, if the spinal cord be affected below the decussation, the influence produced is not crossed, but direct. It is said that some very striking exceptions have occurred to this rule, but these at any rate are remarkably rare. Besides, it has always appeared to me probable that, inasmuch as extensive organic disease, if occurring slowly, may exist without producing symptoms, whilst it is certain most important symptoms may be occasioned without organic disease, even these few exceptional cases are really not opposed to the general law. Then, as a general rule, it may be said that diseases of the brain proper are more especially connected with perversion and alteration of the intelligence; whilst disease of the cranial portion of the spinal cord and base of the cranium is more particularly evinced by alterations of sensation and motion. In the vertebral portion of the

cord, the intensity of pain and of spasm, or want of conducting power, necessary to sensation and voluntary motion, indicates the amount to which the motor and sensitive columns are affected. Further than this we can scarcely generalize with prudence, although there are some cases, as we shall subsequently see, where careful observation has enabled us to arrive at more positive results.

The fatality of lesions affecting various parts of the nervous centres varies greatly. Thus the hemispheres may be extensively diseased, often without injury to life, or even permanent alteration of function. Convulsions and paralysis are the common results of disease of the ganglia, in the cranial portion of the cord. The same results from lesion of the pons Varolii. But this, if it affect the medulla oblongata, where the eighth pair originates, or injury to this centre itself, is almost always immediately fatal.

(4.) *The rapidity or slowness with which the lesion occurs influences the phenomenon or symptoms produced.* It may be said, as a general rule, that a small lesion, for instance a small hemorrhagic extravasation, occurring suddenly, and with force, produces, even in the same situation, more violent effects than a very extensive organic disease which comes on slowly. Here, however, much will depend upon the seat of the lesion. Very extraordinary cases are on record, where large portions of the nervous centres have been much disorganized, without producing anything like such violent symptoms as have been occasioned at other times by a small extravasation in the same place. Here again the nature of the circulation within the cranium offers the only explanation, for the encephalon must undergo a certain amount of pressure, if no time be allowed for it to adapt itself to a foreign body; whereas any lesion coming on slowly enables the amount of blood in the vessels to be diminished according to circumstances, whereby pressure is avoided.

(5.) *The various lesions and injuries of the nervous system produce phenomena similar in kind.* The injuries which may be inflicted on the nervous system, as well as the morbid appearances discovered after death, are various. For instance, there may be an extravasation of blood, exudation of lymph, a softening, a cancerous tumour, or tubercular deposit, and yet they give rise to the same phenomena, and are modified only by the circumstances formerly mentioned, of degree, seat, suddenness, &c. Certain nervous phenomena also are of a paroxysmal character, whilst the lesions supposed to occasion them are stationary or slowly increasing. It follows that the effects cannot be explained by the nature of the lesions, but to

something which they all have in common ; and this, it appears to me, may consist of—1st, Pressure with or without organic change ; 2d, More or less destruction or disorganization of nervous texture. Further, when we consider that the same nervous symptoms arise from irregularities in the circulation from increased as well as diminished action, sometimes when no appreciable change is found, as well as when disorganization has occurred, the theory of local congestions in the nervous centres seems to me the most consistent with known facts. That such local congestions do frequently occur during life, without leaving traces detectable after death, is certain ; whilst the occurrence of molecular changes, or other hypothetical conditions which have been supposed to exist, have never yet been shown to take place under any circumstances.—[*Amer. Journ. of Med. Sciences.* T. R. B.

As many of our readers have doubtless met with cases, to a certain degree similar, we place before them the following from the Medical Examiner.—ED.

Account of a man who lives upon large quantities of raw flesh. In a letter from Dr. Johnston, Commissioner of Sick and Wounded Seamen, to Dr. Blane.

SOMERSET PLACE, Oct. 18, 1798.

My Dear Sir,—Having in August and September last been engaged in a tour of public duty, for the purpose of selecting from among the prisoners of war such men as, from their infirmities, were fit objects for being released without equivalent, I heard, upon my arrival at Liverpool, an account of one of those prisoners being endowed with an appetite and digestion so far beyond anything that had ever occurred to me, either in my observation, reading, or by report, that I was desirous of ascertaining the particulars of it by ocular proof or undeniable testimony. Dr. Cochrane, Fellow of the College of Physicians of Edinburgh, and our medical agent at Liverpool, is fortunately a gentleman upon whose fidelity and accuracy I could perfectly depend, and I requested him to institute an inquiry upon this subject during my stay at that place. I enclose you an attested copy of the result of this—and as it may probably appear to you, as it does to me, a document containing facts extremely interesting, both in a natural and medical view, I will beg you to procure its insertion in some respectable periodical work.

Some farther points of inquiry respecting this extraordinary person having occurred to me since my arrival in town, I sent them in the form of queries to Dr. Cochrane, who has obligingly

returned satisfactory answers. These I send along with the above mentioned attested statements, to which I beg you to subjoin such reflections as may occur to you on this subject.

I am, my dear Sir, your most obedient humble servant,

J. JOHNSTON,

To Gilbert Blane, M. D., F. R. S., and one of the Commissioners of Sick and Wounded Seamen.

Charles Demery, a native of Benche, on the frontiers of Poland, aged 21, was brought to the prison of Liverpool, in February, 1799, having been a soldier in the French service on board the *Hoche*, captured by the squadron under the command of Sir John B. Warren, off Ireland.

He is one of nine brothers, who, with their father, have been remarkable for the voraciousness of their appetites. They were all placed early in the army—and the peculiar craving for food with this young man, began at thirteen years of age.

He was allowed two rations in the army, and by his earnings or the indulgence of his comrades, procured an additional supply.

When in the camp, if bread or meat was scarce, he made up the deficiency by eating four or five pounds of grass daily—and in one year, devoured 174 cats (not their skins) dead or alive—and says he had several severe conflicts in the act of destroying them, by feeling the effects of their torments on his face and hands—sometimes he killed them before eating, but when very hungry he did not wait to perform this humane office.

Dogs and rats equally suffered from his merciless jaws—and if much pinched by famine, the entrails of animals indiscriminately become his prey. The above facts are attested by Picard, a respectable man, who was his comrade in the same regiment on board the *Hoche*, and is now present—and who assures me, he has often seen him feed on those animals.

When the ship on board of which he was, had surrendered after an obstinate action, finding himself, as usual, hungry, and nothing else in his way but a man's leg, which was shot off, lying before him, he attacked it greedily, and was feeding heartily, when a sailor snatched it from him, and threw it overboard.

Since he came to this prison, he has eat one dead cat and about twenty rats. But what he delights most in, is raw meat, beef or mutton, of which though plentifully supplied by eating the rations of ten men daily,* he complains he has not the same

* The French prisoners of war are at this time, maintained at the expense of their own nation, and are each allowed the following daily ration—twenty-six ounces of bread, half a pound of beef, half a pound of greens, two ounces of butter, or six ounces of cheese.

quantity, nor indulged in eating so much as he used to do, when in France.

He often devours a bullock's liver, raw, three pounds of candles and a few pounds of raw beef in one day, without tasting bread or vegetables, washing it down with water, if his allowance of beer is expended.

His subsistence at present, independent of his own rations, arises from the generosity of the prisoners, who give him a share of their allowance. Nor is his stomach confined to meat, for when in the hospital, where some of the patients refused to take their medicines, Demery had no objection to perform this for them—and his stomach never rejected anything, as he never vomits, whatever be the contents, or however large.

Wishing fairly to try how much he actually could eat in one day; on the 7th of September, 1799, at 4 o'clock in the morning, he breakfasted on four pounds of raw cow's udder—at half past 9, in presence of Dr. Johnston, commissioner of sick and wounded seamen, Admiral Child and his son, Mr. Foster agent for prisoners, and several respectable gentlemen, he exhibited his powers as follows: There was set before him five pounds of raw beef, and twelve tallow candles of a pound weight, and one bottle of porter—these he finished by half past 10 o'clock. At 1 o'clock there was again put before him, five pounds of beef, and one pound of candles, with three bottles of porter, at which time he was locked up in the room, and sentries placed at the windows to prevent his throwing away any of his provisions. At 2 o'clock, when I again saw him with two friends, he had nearly finished the whole of the candles, and great part of the beef, but had neither evacuation by vomiting, stool, or urine; his skin was cool and pulse regular, and in good spirits. At a quarter past 6, when he was to be returned to his prison, he had devoured the whole, and declared he could have eat more, but from the prisoners without telling him we wished to make some experiments on him he began to be alarmed. It is also to be observed, that the day was hot, and not having his usual exercise in the yard, it may be presumed he would otherwise have had a better appetite. On recapitulating the whole consumption of this day, it stands thus:—raw cow's udder, 4 lbs.; raw beef, 10; candles, 2. Total, 16 lbs. besides 5 bottles of porter.

The eagerness with which he attacks his beef when his stomach is not gorged, resembles the voracity of a hungry wolf, tearing off and swallowing them with canine greediness. When his throat is dry from continued exercise, he lubricates it by stripping the grease off the candle between his teeth, which he generally finishes at three mouthfuls, and wrapping the wick

like a ball (string and all) sends it after at a swallow. He can, when no choice is left, make shift to dine on immense quantities of raw potatoes or turnips; but from choice would never desire to taste bread or vegetables. He is in every respect healthy, his tongue clean, and his eyes lively.

After he went to the prison, he danced, smoked his pipe, and drank a bottle of porter—and, by four next morning, he awoke with his usual ravenous appetite—which he quieted by a few pounds of raw beef.

He is six feet three inches high, pale complexion, grey eyes, long brown hair, well made but thin, his countenance rather pleasant, and is good tempered.

DESTAUBAN, French Surgeon.

LE FOURNIER, Steward of the Hospital.

REJET, Commissaire de la Prison.

LE FLEM, Soldat de la ser Demi Brigade.

THOMAS COCHRANE, M. D., Inspector and Surgeon of the Prison, and Agent, &c., for sick and wounded Seamen.

Liverpool, Sept. 9th, 1799.

A true copy,

JOHN BYNON,

Clerk in the office for sick and wounded Seamen.

Queries and Answers.

1. What are the circumstances of his sleep and perspiration?

He gets to bed about 8 o'clock at night, immediately after which he begins to sweat, and that so profusely as to be obliged to throw off his shirt. He feels extremely hot, and in an hour or two after he goes to sleep, which lasts until one in the morning, after which he always feels himself hungry, even though he had laid down with a full stomach. He then eats bread or beef, or whatever provision he may have reserved through the day; and if he has none, he beguiles the time in smoking tobacco. About 2 o'clock he goes to sleep again, and awakes at 5 or 6 o'clock in the morning in a violent perspiration, with great heat. This quits him on getting up; and when he has laid in a fresh cargo of raw meat (to use his own expression) he feels his body in a good state. He sweats while he is eating; and it is probable owing to this constant propensity to exhalation from the surface of the body, that his skin is commonly found to be cool.

2. What is his heat by the thermometer?

I have often tried it, and found it to be of the standard temperature of the human body. His pulse is now 84, full and regular.

3. Can this ravenous appetite be traced higher than his father?

He knows nothing of his ancestors beyond his father. When he left the country, eleven years ago, his father was alive, aged about 50, a tall, stout man, always healthy ; and he can remember he was a great eater, but was too young to recollect the quantity, but that he eat his meat half boiled. He does not recollect that either himself, or his brothers, had any ailment, excepting the small pox, which ended favorably with them all. He was then an infant. His face is perfectly smooth.

4. Is his muscular strength greater or less than that of other men of his time of life ?

Though his muscles are pretty firm, I do not think they are so full or plump as those of most other men. He has, however, by his own declaration, carried a load of 300 weight of flour in France, and marched 14 leagues in a day.

5. Is he dull or intelligent ?

He can neither read nor write, but is very intelligent and conversable, and can give a distinct and consistent answer to any question put to him. I have put a variety at different times and in different shapes, tending to throw all the light possible on his history, and never found that he varied, so that I am inclined to believe that he adheres to the truth.

6. Under what circumstances did his voracious disposition first come on ?

It came on at the age of thirteen, as has been already stated. He was then in the service of Prussia at the siege of Thionville ; they were at that time much straightened for provisions, and as he found this did not suit him, he deserted into the town. He was conducted to the French General, who presented him with a large melon, which he devoured, rind and all, and then an immense quantity and variety of other species of food, to the great entertainment of that officer and his suite. From that time he has preferred raw to dressed meat ; and when he eats a moderate quantity of what has been roasted or boiled, he throws it up immediately. What is stated above, therefore, respecting his never vomiting, is not to be understood literally, but imports merely that those things which are most nauseous to others, had no effect upon his stomach.

There is nothing farther to remark, but that since the attested narrative was drawn up, he has repeatedly indulged himself in the cruel repasts there described, devouring the whole animal, except the skin, bones, and bowels ; but this has been put a stop to, on account of the scandal it justly excited.

In considering this case, it seems to afford some matters for reflection, which are not only objects of considerable novelty and curiosity, but interesting and important, by throwing light on the process by which the food is digested and disposed of.

Monstrosity and disease, whether in the structure of parts or in the functions of appetite, illustrate particular points of the animal economy, by exhibiting them in certain relations in which they are met with in the common course of nature. The power of the stomach in so quickly dissolving, assimilating and disposing of the aliment in ordinary cases, must strike every reflecting person with wonder, but the history of this case affords a more palpable proof, and more clear conception of these processes, just as objects of sight become sensible and striking, when viewed by a magnifying glass, or when exhibited on a larger scale.

The facts here set forth, tend also to place in a strong light, the great importance of the discharge by the skin, and to prove that it is by this outlet, more than by the bowels, that the excrementitious parts of the aliment are evacuated; that there is an admirable co-operation established between the skin and the stomach, by means of that consent of parts so observable; and so necessary, in the other functions of the animal economy; and that the purpose of aliment is not merely to administer to the growth and repair of the body, but by its bulk and peculiar stimulus to maintain the play of the organs essential to life.

Large doses of Sub. Nit. Bismuth in Diarrhœa which succeeds Typhoid Fever.

Diarrhœa being at first produced by a lesion of the glands of Peyer, and by an irritable condition of the mucous membrane of the intestines, it would be chimerical at this period to attempt to check it; and even if it were checked it would be without advantage, and even dangerous to the patient; besides, the constipation which is found in some forms of this disease is far from being a favorable symptom. The utility of alvine evacuations then should not be doubted, purgatives having the property, if not to abridge its course, at least to prevent, and to render less intense the complications with other organs. But though the diarrhœa, when it confines itself to certain limits, presents nothing peculiar in the beginning, should not pass unnoticed beyond a certain period, during convalescence.

It sometimes happens that after a number of days the disease loses its intensity: the fever diminishes, the appetite appears, the tongue becomes moist, the face becomes natural, the bowels indolent, without meteorism, and nevertheless the stools remain liquid and frequent, so that it would seem necessary to nourish the patient, were it not feared by so doing the diarrhœa would be increased, or enteritis established. On the other hand, during convalescence, after the patient has passed the

most dangerous period of the disease, the alimentary canal not being accustomed to the contact of the food, the diarrhœa reappears, and it is with the greatest difficulty that the lightest and most digestible food can be borne. It is at this period of typhoid fever that the good effects of sub. nit. bismuth is seen.

M. Monneret first noticed in this journal the remarkable effects of large doses of sub. nit. bismuth in cholériform diarrhœa and in that of children. We are not aware that this physician used it in the diarrhœa of typhoid fever; but we are certain of the good effects of it in the practice of M. Briquet at La Charité and at the Hôtel Dieu. We report the following case, which shows the period of the disease at which the remedy may be used, and the circumstances which render its effects most sure.

CASE.—Dorville, 29 years of age, entered the Hôtel Dieu on the 26th January last. This woman, although of a feeble constitution, thin and delicate, enjoyed good general health, when about the 5th or 6th of January, she felt pain about the umbilicus, which soon spread over the whole of the abdomen, intense fever, cephalalgia and desire to vomit. She was greatly prostrated, mentally and physically; face altered and yellowish; cheeks colored; the mucous membrane of the buccal cavity injected; tongue gluey; thirst, anorexia, nausea; borborygmus in various parts of the abdomen; pains above the umbilicus; superior portion of abdomen rather tense; stools composed of greenish matter, with whitish granules; some rhonchus about the chest; buzzing in the ears; giddiness as soon as the patient sat up; pulse 116, weak; skin hot; no lenticular spots nor headache; there was besides diminution of sonorousness on the left side, with feeble respiration and ringing of the voice at this point.

Though some of the principal symptoms of typhoid fever were absent in this case, there were still enough to form an immediate diagnosis, and two days after, it was confirmed by the appearance of the lenticular spots. The feeble condition of the patient induced M. Aran to adopt a mild treatment. Baths, cataplasms, emollient injections and bland drinks were used. For three days there was delirium at night. Without other treatment the delirium disappeared. After each bath the fever was diminished for several hours, but diarrhœa continued, the tongue remained gluey, thirst great, skin hot, borborygmus continued and abdomen tense.

On the 3d January, the tongue became moist, the skin soft and the patient slept; there were however three watery stools during the night; pulse 108. M. Aran now began the subnitrate of bismuth in large doses (10 grammes in 24 hours), at

the same time continuing the warm baths. On the next day the stools had diminished and the pulse had fallen to 100. On the 5th January the stools were still less, though there was still some borborygmus about the abdomen, particularly about the right false ribs; the face more natural, and tongue moist. On the 8th January, there was but one stool during the day, and another at night; pulse 96; tongue moist. On the 9th January, the bowels were entirely checked; no stool during the 24 hours. On the 17th January the patient was discharged, doing well.—[*Bul. Gén de Therap.*]

Miscellany.

Cases of an unusual form of Fever and Dysentery. By HENRY F. CAMPBELL, M. D.—The present season has been marked by great heat and unusual drought, the thermometer ranging from 82 to 96, for nearly two months, and any thing like general rains being almost unknown during the months of June and July. In June, we heard frequently of a "*Dysenteric Fever*" that prevailed more or less in the upper portions of this State, but as to its exact character, we are unable to affirm, as our letters of inquiry to physicians, in these localities, have failed of reply. We therefore only refer to the rumor as evidence of the atmospheric tendency in other localities than our own. On the 12th of July, we were called about three miles from this city, over the river, to a case of dysentery. It was that of a child sixteen months old, in its first dentition. It had been attacked on the day before with diarrhœa and slight fever. During the night previous to our seeing it, the passages had become bloody, were attended with considerable tormina and the fever was quite violent. When we saw it, the pulse was rapid, the skin hot and dry, especially over the abdomen. The passages were not very frequent, were of a pinkish, bloody color, and consisted principally of mucus. The gums were incised freely, and we prescribed 5 grs. of blue mass, dissolved in syrup; and an injection of starch with 8 drops laudanum after each evacuation. Emolient poultices were directed to the abdomen, and occasional doses of camphor water administered. Shortly after our first visit, we were again called to see the patient which had had a convulsion. We found it in a state of great agitation, the pulse rapid and depressed and the passages very frequent. As the injections were not retained, opiates were administered by the mouth. A blister was applied over the abdomen, but with no good effect. The convulsive

movements were controlled by chloroform, but the child never appeared fully to recover from the depression and agitation induced by them. Calomel in one grain doses was administered every two hours. The passages became very copious and were watery in their character. It died about the middle of the second night from that on which it had been attacked.*

CASE 2d. In the same neighborhood as the above, we saw Thomas, a lad aged about 12 years. He had dysentery, with the discharge of considerable quantities of blood and mucus. The passages were frequent and attended with great straining and distress. The pulse, when we first saw him, was natural. The tongue slightly furred. The skin cool, soft and pleasant. His father had administered about 12 grs. of calomel previous to our seeing him. Our prescription was 3 grs. of calomel every three hours, in combination with injections of starch and laudanum after each passage. Mustard poultices were applied to the abdomen and moderate doses of Chloric Æther and champhor water were prescribed to allay the pain of which he complained. On the second day he had taken 18 grs. of calomel besides the twelve grs. given previously. He had had slight fever the night before but had rested well after the administration of laudanum. The passages were of a dark bilious character, with portions perfectly distinct from the general mass, which were dysenteric and bloody.

R. Calomel grs. 12.

Opii. P. " 3.

Gallae P. " 6.

Acet Plumb. grs. 4.

M. and F. 6 powders—dose 1 powder every 3 hours.

On the fifth day the passages were less frequent and much improved in character. The exacerbation of fever the night previous had been more decided. The skin still pleasant and suffused with a copious moisture. The opium, lead and galls were continued three times daily, the calomel being omitted from the prescription. Also prescribed 12 grs. of quinine during the morning in anticipation of the paroxysm. On the fifth and sixth days the passages continued of a feculent character, but on the seventh day, small quantities of bloody mucus were again apparent in the discharges. There was no return of fever. Prescribed two grs. of calomel each night. Diet, gelatin rice gruel and chicken broth. The passages became more and more natural, though always bearing the marks of considerable intestinal irritation, and the patient very slowly recovered.

* Dr. Paul F. Eve was called in consultation in this case at a late period.

CASE 3d. Mr. E. A., aged about 35 years, was attacked with dysentery on the 14th of July. The passages were frequent and bloody, but not attended with much pain; skin hot and dry; pulse somewhat depressed, and 90 per minute. He manifested great depression of spirits. Prescribed 5 grs. of calomel with one gr. of opium every three hours, with laudanum and camphor in the intervals when the passages were frequent. The passages became less frequent, were dark and very offensive, but were followed by great prostration of strength. The calomel was omitted and 5 grs. of blue mass substituted, with acetate of lead, nut galls and opium after each passage. The discharges became less frequent, and the quantity of blood and bloody mucus was greatly diminished, but the prostration of strength was still very great. Fever continued without remission: pulse 110 per minute. On the fourth day the skin is hot and dry. Thirst very urgent. The dysenteric appearances had subsided from the passages, but they were thinner than previous. On the fifth day the pulse is more rapid and weaker, the skin cooler and clammy; the passages are very frequent and watery, having the appearance of the washings of bloody beef, though sometimes darker, resembling muddy coffee; they contained no mucus or fœcal matter whatever. After consultation with Dr. J. A. Eve, we prescribed a large blister to the abdomen, and directed laudanum injections with starch in combination with acetate of lead after each evacuation. These injections were speedily rejected: the passages were very copious and passed involuntarily, often unconsciously. The pulse ranged from 130 to 140 per minute—every thing marked a state of actual collapse. Brandy and other stimulants were administered in large quantities, but the pulse did not respond. The passages continued large and were often of clear water. A blister was applied over the sacrum, but without effect. All stimulants were of no avail, only serving to distract the last moments of the patient, which without them were characterized by great calmness and collectedness of mind. He died on the morning of the 7th day.

CASE 4th. P., a boy aged 8 years, was attacked with dysentery on the evening after the above case. On examination, we found him remarkably free from excitement, the skin cool and moist. The passages produced but little pain, were very frequent, and invariably composed of blood and mucus. Prescribed, 10 grs. of calomel, to be given in two doses, one that night and the other in the morning, with 15 drops of laudanum in one oz. camphor water after each passage. On the second day, fever became apparent, and in the evening, the

pulse was very rapid, 130 beats per minute. The passages continued of a bloody character mixed with mucus. He complained of great pain immediately before and during the evacuation of the bowels, but at other times appeared quite easy. Injections of laudanum 30 gtt., acetate of lead grs. 10, with starch were administered after each passage, but without effect. The passages were still very frequent and unchanged in character. We applied a large blister over the abdomen and continued the administration of the calomel in two grain doses three times daily. On the fourth day the fever had not abated, though the pulse was much more feeble and the skin cooler: during the night he became restless and tossed constantly; the bowels were evacuated involuntarily and the stools had now assumed the appearance of bloody water. The skin was cold and clammy, the pulse being very rapid. Mustard poultices, brandy and other stimulants failed to arouse him from this state of collapse, though he still lingered till the sixth day, when he died. For nearly twelve hours previous to his death he suffered from the greatest restlessness, and only remained quiet on the administration of chloroform.

We might here report the particulars of two other cases very similar to this last; the one occurring in a child of 3 years of age, treated by myself in company with Dr. Ford, and the other, a boy of six years of age, treated by Dr. Ford, but in their course as well as the result of treatment, they were so exactly similar that it is entirely unnecessary to dwell upon them. In both, the fever was of a continued character, with great prostration of strength, and the passages also presented the same serous character as in the above case. Both these also died on the sixth day.

CASE 5th. Mr. L. A., a young man aged about 20 years, was affected with dysentery similar to the above cases, complicated with fever of a remittent type. On the second day of the disease we saw him and prescribed 20 grs. of blue mass and $\frac{1}{4}$ gr. morphine in camphor after each evacuation. The passages on the next day presented a very dark appearance and were offensive, but there were still lumps of thick mucus with blood scattered through some of them. Calomel in combination with the acetate of lead and nut galls, was administered three times during this day, with laudanum and starch injections after each evacuation. On the fourth day, the dysenteric character was not manifest in any of the passages, which were not very frequent. The calomel was discontinued and 10 grs. of quinine was given during the remission in the earlier part of the day. The passages were restrained by the laudanum and starch injections. On the 6th day, he

suffered great pain during the evacuation of the bowels, and the passages had again assumed all the characters of dysentery, and were also very frequent. Prescribed at night a large blister to the abdomen and to continue injections. Seventh day; blister had drawn well, passages again consistent and dark, though attended with considerable pain. Prescribed 3 grs. of blue mass every three hours, and to continue injections; also to take chloric ether and camphor to allay pain. After the administration of the quinine there was no return of fever. The character of the passages improved slowly, but for many days continued interspersed with small portions of whitish lymph. He has recovered, but is still feeble and enervated from the depressing disease through which he has so recently passed.

The location in which occurred the above cases was usually free from disease of any kind. The neighborhood is the summer residence of several families, having been selected for the salubrity and pleasantness of the situation. Five of the cases were fatal and occurred in one family, while the two others whose termination was favorable, took place in other houses. It will be observed from our report that the disease attacked only the white residents of the neighborhood, and although a number of negroes were subjected to exactly the same morbid influences as those whites, still not one of them that we are aware, was the subject of this form of dysentery.

Above, we have given a simple statement of the facts connected with the prevalence of this disease, and as our intention is to make an historical record, rather than to discuss or promulgate a theory, our remarks in relation to it will be neither copious or diffuse. There are but few points in our record that admit of any debate or observation. As to the cause, it is evident that the invasion of this ordinarily salubrious region by an unusual form of disease must be attributable (judging from its suddenness and partiality), to some peculiar and recent agent having its origin within the limits circumscribed by the extent of its manifestations. We must also perceive that the character of its effects is identical wherever manifested, only varying in their intensity and that this deleterious agent, of whatever nature, must have been more favorably situated by proximity or otherwise for the full development of its effects upon that location where its fatality was so marked. In reference to the indications for treatment, we have been strongly impressed that too much importance cannot be attached to the early and vigorous application of remedial means during the period of the *bloody* discharges; for, from our observation, it is only during *this* stage that any treatment is available. After the occurrence of the

serous dejections and their attendant rapid and feeble pulse, cold extremities and sunken countenance, we have, in no instance, seen the least benefit derived from stimulants astringents or revulsives. The disease must be resolved during the dysenteric period, as the succeeding state is one of *collapse*, in which the impressibility of the system, to remedies of any kind, is almost null. In the two cases which terminated favorably, it will be observed that the principal difference in the treatment from that of the others, consisted in the continued administration of small doses of calomel in combination with opium. In one of these a blister was early applied with excellent effect, while in the other, which was milder, no blister was applied during any stage. In several of the cases, the fever having assumed a remittent type, quinine prevented the return of the paroxysm; though given with any other view, (that is *specifically*,) we cannot speak well of its effects. As it is our object only to illustrate the peculiarity of the disease, we think it unnecessary to dwell longer on the treatment and with these brief remarks we leave the subject to the observation of those who have had better opportunities for witnessing and treating this formidable variety of dysentery and its accompanying fever.

Treatment of Asphyxia from Drowning and Hanging. From Dr. (D. H. STORER's Address before the Massachusetts Medical Society.)—How little understood, among many of the well-educated and intelligent in our community, is the treatment for the restoration of the drowned! How many lives must have been sacrificed by the barbarous custom of suspending the asphyxied by the feet, or rudely rolling them upon barrels with the head dependent, for the purpose of freeing the lungs of the water with which they were supposed to be filled!—a custom which, within a few years has fallen under my immediate observation.

How many, apparently dead, have been restored to their afflicted friends by means of long-continued, scientific efforts; by having their bodies carefully dried, and exposed to a moderate temperature—their heads and shoulders elevated—their lungs artificially inflated; by the exhibition of external and internal stimulants and judicious venesection!

How many have thus been resuscitated, after all human means seemed unavailing—long after the by-standers have ceased their efforts, and none saved the almost frantic parent or child, have in silent prayer continued their exertions! Numerous cases might be cited to show that life has been re-called after a body has been immersed for a very long period. These instances should cheer the desponding, and encourage them to labour while there seems the slightest possibility of restoration. Allow me to illustrate this remark with a single example, which was published during the last year in the "Nothorn Lancet and Gazette of Legal Medicine." It was communicated by Charles

McNeil, Esq., of Charlotte, Vt., and is the touching story of a grateful father. "One of my sons, 9 or 10 years of age, was on Sunday afternoon, in August, 1830, found to be missing. On inquiry, I ascertained that he had last been seen playing on a boat lying at the wharf. The day was calm, and the waters of Lake Champlain still and unruffled by a ripple; but, knowing that he had been on the boat, his brother was sent to search for him, but he returned without any tidings. Once more he returned to the boat, and, looking carefully in every direction, discovered him lying on the bottom of the lake in eight feet of water, where he must have lain half an hour, if not longer, when he was brought to the surface. I received the body: it was rigid and cold, as also were the limbs; a bluish cast was spread over the countenance; the deep solicitude of a father discovered no signs of life—no heat; the heart was stilled, and the lungs quiescent. No more would I have anticipated the presence of life, if he had been submerged for several years; and had I not, some days previous to the accident, providentially read in an old paper an article by Dr. Buchanan, of Philadelphia on the subject of restoring suspended animation after submersion, we should have consigned the body to the grave, as it was recovered from the lake. The body being placed on a bed, some of the neighbors were directed to rub it briskly with flannel cloths—an order which they obeyed with great reluctance, from the thought of performing this office on a corpse; and I will admit that I somewhat entertained the same opinion. Still, I would fain hope, and urge on my friends the continuance of their exertions; the friction was persevered in; warm flannel sheets were applied in rapid succession. This treatment was continued for thirty or forty minutes, when we were gratified by hearing a feeble murmur in the throat, followed soon after by a slight quivering of the lips. The case, however, was enveloped in doubt and obscurity for a long time, as the recovery was extremely slow."

The above remarks might, with equal propriety, be applied to the subject of hanging. Many judicious general practitioners entertain the most vague and unsatisfactory notions regarding its phenomena. They not only are unacquainted with the several appearances produced in individual cases of suspension, but they really are not aware how death is produced; and, cerebral apoplexy not unfrequently being considered the cause, copious depletion, employed instead of artificial respiration, checks the vital current forever.—[*Boston Medical and Surgical Journal*.]

Compression of Aorta at the Sacro-vertebral Angle, for Uterine Hemorrhage—M. Duhamel reports three cases in which compression was successfully employed. The two first instances presented themselves in a woman aged 30 years, one immediately after parturition, the other two years after. In the last, the hemorrhage came on 16 days after parturition. The third case was in a woman, eight months and a half advanced in her second pregnancy, in whom a little hemorrhage appeared at intervals during fifteen or twenty days, which confined her to bed. Believing the placenta to be attached to the neck

of the uterus, M. Duhamel brought on labor. In one hour after, the uterus had contracted greatly, and all hemorrhage had ceased, but in another hour the hemorrhage reappeared, and became so abundant that it caused frequent syncope, and when M. Duhamel returned the woman was absolutely exsanguine. The compression of the aorta was alone tried, and the hemorrhage was immediately stopped. Compression was continued nine hours. The hemorrhage had been so great, that it was not until 24 or 30 hours after its suppression that the pulse began to be felt at the wrist.—[*Arch. Gén. de Méd.*]

Successful Cæsarian Operation.—M. Bonchacourt, chief surgeon of the Maternity of Lyons, has just published, in the *Bulletin de Thérapeutique*, the case of a deformed woman, thirty-two years of age, upon whom he performed the Cæsarian section with complete success. The patient's sacro-pubic diameter was only two inches and two lines; embryotomy had been performed in a former confinement, four years previously, and in October, 1850, she again presented herself on the point of parturition. In about seventeen hours from the beginning of labour the cord and one arm were expelled by very powerful contractions, but the head could evidently not pass. M. Bonchacourt, finding that the heart of the foetus and the cord pulsated strongly, resolved, after consulting with his colleagues, to perform the Cæsarian operation. Chloroform was used, the child extracted alive, and the operation, with the dressing, did not last more than twenty-five minutes. The child died five days afterwards from ordinary causes, and the mother left the hospital twenty-four days after the operation, the wound being completely cicatrized, and all the functions performed in the most satisfactory manner. The patient was seen in April, 1851, five months and a half after the Cæsarian section, and found quite well; the only remains of the operation being a slight and easily reducible hernia, a little distance below the umbilicus.—[*London Lancet.*]

Ages of Animals.—A bear will seldom live beyond 20 years, dogs and wolves live 20 years, a fox 14 to 16. The ordinary age of cats is 17 years; that of a squirrel, hare or rabbit, 5 to 8 years. Elephants, it is said, live 400 years; the rhinoceros 50 years; horses may live 72 years, but their ordinary age is from 25 to 30 years: the chamois live sometimes 100 years. An eagle died at Vienna at the age of 104 years. Ravens live 100; swans 300. A tortois lived more than 190 years. A sheep seldom lives more than 10, and a cow more than 15 years.

On the Proportions of the Human Skeleton.—M. Carus, of Dresden, has been endeavoring to discover the standard measure upon which the human frame is constructed, and considers that he has found it in the vertebral column. He states that the spine in a new-born child is just one-third of what it becomes in the adult; and he therefore

takes the third part of the vertebral column as the abovementioned standard. The head, for instance, its length and breadth taken together (without the lower jaw, which is a sort of extremity of head,) is just the size of the standard measure; on the trunk each clavicle, with the acromion, the sternum, and each scapula, may be measured upon the same suit. Such is likewise the case with the pelvis. The normal length of the arm is three measures, the hand one, the foot one, the tibia two, the femur two and a half, &c. The whole length of the body comprises in the normal state nine and a half of the standard measure. M. Carus has had a small figure constructed upon which these various proportions are accurately marked.—[*London Lancet*.

More Wonders in Medicine.—A very short time since, the French medical papers were very busy in discussing a new method of curing sciatica, which consisted in cauterizing the lobe of the ear. Very respectable names, and numerous cases, supported this extraordinary procedure. We have now another startling manner of dispelling both neuralgia in the face and toothache; it consists in merely touching with a blunt probe the membrana tympani. The author of this method is M. Desterne, who has published in '*L'Union Médicale*,' the rise and progress of his discovery, accompanied with numerous cases, which are extremely surprising. The most intense toothache connected with decayed teeth, is relieved in a moment by the magic touch of the membrana tympani; agonizing neuralgia of the face, one of six months, the other of two years and a half standing, were cured in an instant by the same means; hemicrania vanishes, and quinine is left far in the background. These sudden effects are supposed to be obtained by the agency of the corda tympani nerve.—[*Ibid*.

Instrument for arresting Epistaxis. By M. Gariel.—This is a tube made of caoutchouc, carrying at its extremity a dilatable balloon, which, when introduced into the nostrils in its undistended state, may, by the process of insufflation, be made to assume such dimensions, and exerting such pressure as completely to arrest the hemorrhage.

A simple method of plugging the posterior nares suggests itself in examining this tube with dilatable extremity. This operation as at present performed, whether with a special apparatus, or with an ordinary catheter, is frequently very troublesome, though simple in appearance. If the tube be introduced from before backwards through the cavities of the nose, until it has quite cleared the posterior nares and arrived in the pharynx, and be then dilated and drawn forwards, we obtain a more complete and manageable plug than that usually made of lint.—[*Dublin Quarterly Journal of Medical Science*.

Disgusting Remedies.—If our homœopathic friends are in the habit of using in their practice such disgusting and absurd remedies as are recommended in Jahr's Pharmacopœia, the sooner a line of distinction is drawn between them and the regular practitioners, the better. We

know not whether any of Hahnemann's disciples have demonstrated, on scientific principles, the action or properties of such remedies. They certainly bring to mind the absurd ingredients used in the old treacle of Andromichus. The following, from the London "Institute" of Jan. 5th, will present the matter to our readers.

"Jahr's *Nouvelle Pharmacopée Homœopathique*, published at Paris in 1841, contains, in the list of *materia medica*, various disgusting absurdities, among which are *lachesis*, the poison of the rattlesnake; *formica rufa*, the red ant; *aradea diadema*, a species of spider; *rana bufo*, the toad; *lacerta agilis*, the lizard; *scarabæus melolontha*, the cockchafer; *viverra putorius*, stinking polecat, of which the officinal part is the fetid secretion from the glands near the anus. The *écrivissus*, or fresh-water crabs, are directed to be pounded alive in a mortar until reduced to a fine paste. This is diluted with about twice its volume of alcohol, then strained, and the liquor preserved for dilution in the usual way. Toads, lizards, cockchafers, and other reptiles and insects, are brayed (alive) in the same manner!

"We also learn from a contemporary that to such an extent is the doctrine *similia similibus curantur* carried, that 'syphiline' is administered to patients suffering under secondary syphilis; and 'blennorrhin,' which is gonorrhœal matter manipulated according to the rules of homœopathic confectionary, is mentioned in the Homœopathic Archives (published at Leipsic, 1841) as a remedy for gonorrhœa and gleet!"—[*Boston Med. and Surg. Journal*.

Remarks on the Cooking and Preserving of Meat. By Prof. LIEBIG. The view that broth derives its nourishing properties essentially from the dissolved gelatin—an opinion which has frequently been discountenanced in practice—is shown by this investigation to be completely untenable. The gelatin imparts no taste to broth, and forms by far too insignificant a portion to allow of its nutritious properties being dependent upon it. Chopped beef, or veal, previously exhausted in the cold, when boiled for five hours, yielded to the broth, the former 0.5 per cent. and the latter 1.5 per cent. of soluble constituents, of which gelatine formed, at most, but one-half. On the contrary, this investigation confirms the view of Prout, that the peculiar constituents of broth exist ready formed in the flesh, and are by no means merely products of the process of ebullition. The residue of the chopped muscular flesh of different animals—as of the fox and ox—after having been exhausted in the cold, cannot be distinguished the one from the other; all the peculiarities of the flesh, especially its flavour, depending entirely upon the soluble constituents which are found in the broth.

The researches of Liebig offer a simple and convenient method of preparing, in a few minutes, a broth of the highest nutritive properties. Finely-chopped lean beef is mixed with an equal weight of cold-water, and left, if possible, to macerate for a short time, and the whole then slowly heated to ebullition. After gently boiling for some minutes, the clear broth separates from the coagulated albumen, and from the muscular fibre, which has now assumed a sinewy appear-

ance. After straining, it requires only to be seasoned, and slightly coloured with burnt onions, or with caramel. The colouring of the broth is nothing but a concession to the common prejudice, which cannot, however, be well dispensed with. By evaporation in a water-bath, or at a still lower temperature, the broth becomes spontaneously coloured, and leaves behind a brown extract, possessing a delicate odour of roasted meat; this extract, when dissolved in about thirty parts of water, and flavoured with salt, yields, at any moment, a most excellent broth. The advantage of extract of flesh for the nutrition of invalids, its use in hospitals, or in field service, as well as in domestic economy, is sufficiently obvious. We see, likewise, that bone-broth, broth-tablets, &c., being preparations essentially different from a true broth from flesh, cannot enter into competition with it as articles of food.

As an article of commerce, extract of flesh bears somewhat too high a price. It appears, however, to offer a new source of profit to the inhabitants of the different settlements in America and Australia, who might successfully prepare it from their cattle at a cheaper rate and send it to the markets of our crowded populations.

As to the cooking of meat, it follows that to prepare, by boiling, a rich broth, and, at the same time, a savoury *bouilli*, is perfectly impossible. After preparing broth to the above directions, the meat which remains is perfectly unpalatable, tasteless and tough, and as dissimilar as possible to boiled beef of our tables. If, on the other hand, it be desirable to leave in the boiled meat the greatest amount of nutrition and flavour, it must be at once plunged into boiling water. If the temperature, after some minutes, be reduced to about 158° Fahr. by the addition of cold water, and the water maintained at that temperature until the meat is thoroughly cooked, all the conditions necessary for this purpose will have been fulfilled. If it be perfectly established that pure fleshy fibre—viewed independently of the juice—instead of being softened by boiling, is converted into a horny or sinewy mass, it is evident that this change is prevented by two different means in the ordinary mode of cooking meat: in the first place, by the temperature in the interior of the piece of meat never reaching the boiling heat; and, in the second place, by its being, nevertheless, sufficiently high to coagulate the albumen which surrounds, and, to a certain extent, protect the fibre. The temperature in the interior of the meat is not only sufficient to coagulate the albumen (132° Fahr.) but must attain even the point necessary for the coagulation of the colouring matter of the blood (from 149° to 158° Fahr.)

The investigation of Liebig exhibits the process of salting meat under a perfectly new aspect. The "brine," which meat and dry salt form when together, amounts to from one-third to one-half of the juice of the meat, and contains the chief constituents of concentrated broth. The brine presents an acid reaction, and, owing to the quantity of albumen present, coagulates when boiled; it contains, moreover, phosphoric acid, lactic acid, a large amount of potassa, kreatinine, and, doubtlessly, also kreatine. There can be no doubt, therefore, that

saltng diminishes the nutritious properties of meat, by the amount of constituents which pass into the brine ; hence the explanation of the well-known injurious effect on health produced by the continued consumption of salt meat.—*Liebig's Report*, vol. ii.

Tincture of Iodine in the Treatment of External Poisoning.—Dr. T. Smith, of Cincinnati, has used the tincture of iodine, with much success, in several cases of poisoning, the result of contact with the *Rhus Toxicodendron* (Poison Oak), *Rhus Radicans* (Poison Vine), *Rhus Vernx* (Swamp Sumac), &c. Having been frequently called upon to prescribe for this affection, Dr. Smith was led to the trial of local remedies, likely to prove more prompt than the ordinary antiphlogistic treatment resorted to on such occasions. Within the last two years, he has used the tincture of iodine as a local application, in some half dozen cases, and with such striking good effects, that he confidently recommends it to the profession.—[*Abridged from the Western Lancet. Med. Examiner.*]

Southern Medical Reports ; consisting of General and Special Reports on the Medical Topography, Meteorology and Prevalent Diseases of the States of Louisiana, Alabama, Mississippi, North Carolina, South Carolina, Georgia, Florida, Arkansas, Tennessee, Texas, and California. Edited by E. D. FENNER, M. D., of New Orleans, Member of the American Medical Association, &c., &c. Vol. II. 1850. New Orleans: D. Davies, Son & Co. New-York: Samuel S. & Wm. Wood.

We hail with pleasure the appearance of the second volume of these Reports. The success and popularity of the first volume was good earnest of a favorable reception for the second, and since its appearance we feel greatly encouraged from the ability and unremitting industry of which it bears the marks. The work must now go on, and as it has to depend for its support upon the sustaining aid of our profession, we hope that, for the honor as well as the benefit of that profession, its members will come out promptly and give their succor freely and liberally. As yet, we have not had an opportunity to give this volume such a perusal as would warrant an extended review : indeed this would be unnecessary, as the character of the work, the ability of its accomplished and indefatigable author, as well as the importance of the enterprize to the profession, has been sufficiently dwelt on elsewhere in reviews of the first volume. It is sufficient to say that this compares advantageously with it, and will well repay the trouble of a careful and thorough perusal.

SOUTHERN MEDICAL AND SURGICAL JOURNAL.

Vol. 7.]

NEW SERIES.—OCTOBER, 1851.

[No. 10.]

PART FIRST.

Original Communications.

ARTICLE XXX.

Veratrum Viride. By C. T. QUINTARD, M. D., of Roswell, Ga.

Our attention was first directed to this plant as a remedial agent, by Dr. Norwood, (vide Southern Med. and Surg. Jour., June, 1850,) and although our experience with it has not been such as to warrant or justify any exact conclusions, as to its power over the arterial system, it has nevertheless been sufficient to authorize us in calling the attention of the profession to it, as an agent whose powers have never yet been thoroughly examined.

The American hellebore, swamp hellebore, Indian poke, Indian uncas, poke weed, bear weed, itch weed, tickle weed, as it is variously denominated, flourishes abundantly from Canada to Carolina, and, according to Joscelyn, was known to the Aborigines. It appears to have been known to them, rather as a poison than a remedial agent. According to Joscelyn, a decoction of it was employed in the election of their chiefs. That individual whose stomach was least susceptible to its deleterious effects was regarded as the "strongest of the party, and entitled to command the rest." Dr. Osgood informs us that it has long been extensively used for the destruction of vermin and birds. "Among many of the farmers of New England," says Dr. O., "it still continues a common practice to protect their corn-fields from the havoc of birds, by scattering the ground with kernels of corn saturated in a strong infu-

sion of the root ; this is done just as the corn is springing from the ground, it being then most liable to depredations from the feathered tribe. With many it is customary to subject their seed corn to the same process before planting. A short time after partaking of corn thus prepared, muscular action becomes so much paralyzed as to prevent either flying or walking, and in this torpid state they are readily taken and killed. Unless caught while thus narcotized many of them recover and fly away."

The *Veratrum viride* belongs to the Linnæan class and order Polygamia monœcia, and to the natural family Colchicaceæ, of De Candolle, and Melanthaceæ, of Lindley.

V. viride foliis lato-ovalibus, plicatis ; racemis paniculatis ; corollæ laciniis ablongo, ovalibus, acutis.*

It usually grows in low, moist and swampy situations, is from three to six feet high, and flowers from May to July. The rhizoma is thick, fleshy, tunicated above, but solid below, with numerous whitish radicles. The stem is tall, roundish, striated, and pubescent. Early in the spring, before the stem rises, it bears a slight resemblance to the *Ictodes fœtidus*, with which it is not unfrequently associated. In its botanical characters it is closely allied to the *Veratrum album*, a distinguished medicinal plant, a native of the mountainous regions of continental Europe, and abounding in the Alps and Pyrennees. The analogy in external appearance between the *V. viride* and *V. album*, first led to an investigation of the properties of the former, and it was found, notwithstanding the analogy in botanical affinities, that there was a decided difference in their operation. The medicinal effects of the *V. viride*, have been very differently estimated. Bigelow says, that it is an acrid, emetic, and powerful stimulant, followed by sedative effects ; and in the U. S. Dispensatory, it is represented as agreeing in its effects with *Veratrum album*, while others do not find its operation attended, even in large doses, by so severe a train of symptoms as follow the administration of the *V. album*. Prof. Tully, of Yale College, who used this article extensively, was of the opinion that it ought entirely to supercede, not only the other species of the genus, but also the *Colchicum autumnale*. There

* Vide Pursh I., p. 242. Elliott, p. 419. Darby, 251.

are certainly fewer objections to its use. It is not liable to produce the uncontrollable purging, which often results from the exhibition of the colchicum; it is far more certain, and produces no irritation, in proper doses, on the mucous coat of the alimentary canal. On the contrary, it operates with as much certainty as any article in the materia medica, is never cathartic, and always leaves the bowels in good condition. Dr. Osgood states, that it requires but a moderate degree of attention to render the operation of the *V. viride* perfectly safe—while Dr. Norwood has never found any unpleasant effect resulting from its administration, other than nausea. It has produced vomiting, once, in the cases in which we administered it, after twelve drops had been twice given at an interval of two hours. But its operation was mild—more so even than ipecac. Prof. Tully thus enumerates the effects of this plant:—1st. Deobstruent or alterative; 2d. Acrid narcotic; 3d. Emetic; 4th. Epispastic; 5th. Errhine.

Dr. Osgood and others have noticed an emmenagogue effect—while Dr. Peabody found, in one instance, the continued internal use of this medicine produce as powerful salivation as is witnessed from the use of mercury. This, however, passed off directly with the discontinuance of the medicine, leaving no unpleasant effects. Of the different operative effects of this medicine, its alterative has, heretofore, been considered the most important. When the system is brought fully under its influence its narcotic properties are manifested by faintness, somnolency, dimness of sight, dilation of the pupils, vertigo, headache, impaired muscular action, hiccough, cold, clammy sweat, small, unfrequent and compressible pulse. These effects may always be counteracted by the use of laudanum and brandy. We shall give a synopsis of the experiments of Dr. Osgood, as detailed in the *American Jour. Med. Sci.*, and see how far they go to support the conclusions of Dr. Norwood, and the experience we have had.

The experiments of Dr. O. were made with the extract of this plant, immediately after the preparation was formed, and before it had been at all used as a medicine.

At 12 o'clock, M., Dr. O. took two grs. of the finely pulverized extract. "At one," he says, "I began to experience a

slight sense of uneasiness at the stomach, but not amounting to nausea. This uneasiness at the stomach, though so slight as to be attended with very little inconvenience, continued until about half-past one, when vomiting commenced. The contents of the stomach were thrown off *without nausea*, but with a sense of rising in the œsophagus, which perhaps might be compared to the rumination of animals. Judging from my sensations at the time, I should suppose the muscular fibres of the stomach steadily, gradually contracted upon its contents, until they were expelled, the diaphragm and abdominal muscles remaining entirely inactive. After the vomiting had continued a considerable length of time, it appeared to be more the effect of spasmodic action, and was attended with chills and coldness of the whole body, but moisture of the skin. At the expiration of about an hour the vomiting ceased, and was followed by dimness of sight, dilatation of pupils, vertigo, faintness and somnolency; pulse, at the wrist, *40 in the minute, and scarcely perceptible*. I then took 25 m laudanum, and fell asleep. After the lapse of an hour, awoke with the continuance of the same symptoms, together with a dull pain in the epigastrium, and immediately repeated the laudanum. But finding no relief, the dimness of sight increasing, and on motion of the body, or turning the head, amounting almost to blindness—a sensation of stiffness in the voluntary muscles supervening, particularly the temporal and extensors of the head, together with considerable general prostration, the dose of laudanum was doubled. This produced a partial abatement of the symptoms, and after another similar interval was repeated, with half a gill of brandy, which now effected entire relief." In connexion with these symptoms, Dr. O. observes, "I am unusually susceptible to the operation both of emetics and narcotics." A friend of Dr. O.'s, much less susceptible to the operation of medicinal agents, commenced at 9 o'clock in the evening, with two grs. This, in ten or fifteen minutes, produced slight uneasiness at the stomach. In a half hour from the first dose, he took four grains more; at 10, a sensation was felt as of a ball rising in the œsophagus, which extended up as far as the top of the sternum, as if propelled by a gradual tonic contraction of the stomach; at a quarter past 10, vomiting

commenced. This was attended with very little inconvenience at first, but after continuing a short time became more severe, the ejections consisting principally of bile; together with the vomiting there was much ineffectual retching, almost constant hiccough, chilliness, dimness of sight, vertigo, inability to control the voluntary muscles, distress at the stomach; pulse, *small and creeping, and 34 in a minute*; the ordinary frequency ranging from 56–58. As these symptoms were becoming more aggravated, he took 5ss. tr. opii., and went to bed scarcely able to walk. In ten or fifteen minutes the laudanum was repeated, which soon produced sleep. In the morning he was apparently in better health than he had been for several months. At 7, the same morning, three grains more were taken; at 9, complained of a confused sensation in the head, and almost an entire loss of power of the gastrocnemii muscles. At 12 M., three grains more were taken, and at half-past 12, all the muscles of the forearm were affected in the same manner. At 1, vomiting; pulse 40, and other symptoms essentially as the day preceding, excepting a less degree of chilliness. At half-past 2, took 45 ℥ laudanum, and in the course of two hours the effects of the medicine entirely subsided, excepting the inability of using the gastrocnemii muscles. At 11 in the evening, two grs. more were taken, which, in about three-quarters of an hour, produced vomiting like the other cases, but without any appreciable narcotic effect.

This gentleman afterwards experimented with smaller doses, and at regular intervals. Doses of half a grain, once in three hours, after being repeated three or four times, were followed by an uneasiness of the stomach, with the same paralyzed condition of the extensors of the feet. The dose was then diminished to a quarter of a grain, and continued three days, at the same intervals. With these doses, muscular action was not so constantly interrupted, this effect occurring only after considerable exercise, as walking or jumping. By continuing this article three or four days, in doses of one-eighth of a grain, once in three hours, moderate diuresis was produced.

Dr. Osgood states that, "in no single instance in the experiments with the article upon myself or others, did it operate in the least as a cathartic."

Dr. John Ware, of Boston, who experimented with it in the form of a powder of the root, states that he administered it in thirty cases, and, "in no instance was it very clear that purging was produced."

This remedy has long been used in the treatment of arthritic inflammations. In the hands of the late Dr. Bayard, of Savannah, it was successfully applied as a remedy in severe cases of chronic rheumatism as early as 1811, and probably before that period, it had frequently been used with success in the treatment of this and kindred diseases. It is not our intention to point out the various therapeutic applications of this remedy, or to enumerate the various diseases in which it has been employed.

The first case in which we administered the remedy was one of pneumonia, which had been subjected to the ordinary treatment, but which was accompanied and followed by a very troublesome cough. It evidently relieved the irritable condition of the lungs, although comparatively small doses were given—eight drops, at intervals of three hours.

The second case, was that of a young and stout laboring man, aged twenty-three years. He had been working at a mill-dam—had been wet, and heated, and in the evening was seized with a severe chill, and pains in various parts of his body. We saw him the following morning: his pulse was full, hard, and bounding; tongue coated with a white fur; skin hot and dry; pain across his brow, and an almost entire suppression of urine. This was an admirable case for testing the action of the *V. viride*, and we determined to use it, notwithstanding the circulating system was in so phlogistic a condition, as to present what is known as a "bleeding pulse." Ten drops of the remedy were given at intervals of two hours, to which were added five drops of laudanum. The third dose had been taken about half an hour, when a manifest abatement was noticed in the severity of the symptoms. At the fourth dose, twelve drops were administered, without the laudanum, and the pulse, which had been gradually approaching the natural standard, fell to about 60. The tongue was moist, the skin soft and at its natural temperature, and we concluded to give no more of the remedy. The result was truly surprising, and

we were anxious to see if the medicine had indeed achieved, in so short a time, what the lancet, and more energetic, or, rather, severer measures would not accomplish in a much longer period. The patient slept tolerably well that night; he had felt no nausea, but had taken some light food in the evening. There had been no action on the bowels. The next morning his fever had returned, but only partially; and it occurred to us at the time that the action of a dose of castor oil would relieve him entirely; but it was withheld and the veratrum resorted to. Its operation was as favorable as on the day preceding, and it was persevered in until six doses had been taken, when the patient, beginning to feel some uneasiness at his epigastrium, at first slight, but gradually increasing, vomited freely. His pulse was at this time lower than at the first exhibition of the remedy, and a cold perspiration stood out in large drops over his chest, neck and forehead. Twenty-five drops of laudanum were administered, and the patient went quietly to sleep. He awoke greatly refreshed, and feeling quite well. There was not left a single unpleasant symptom. A dose of oil was, however administered, and the patient soon after returned to his occupation.

This case is parallel, if we recollect rightly, (we have not the first paper of Dr. Norwood at hand,) to some published by Dr. Norwood in the South. Med. and Surg. Jour., and finely illustrates the powers of the remedy over febrile excitement.

Dr. Norwood feels confident that it is an agent by which arterial excitement can be at all times controlled. "It is," he says, "the only article or agent known that will control certainly, and without disappointment, the action of the heart and arteries—that it is the only therapeutic agent known, through which we can say to the heart and arteries 'so fast shalt thou beat and no faster;'" and again, he says,* "we unhesitatingly assert, that it has not failed us in a single case to reduce the action of the heart, and down to any point we wished, as to the number and frequency of the pulsations."

In the experiment of Dr. Osgood, made upon himself, the pulse was reduced, in two and a half hours, to 40 in the minute, and was "scarcely perceptible." This was from a two

* Vide Southern Med. and Surg. Journ., Jan. '51.

grain dose of pure extract. Six grains reduced the pulse of the other experimenter, in one hour and a quarter, to thirty-four in a minute—"the ordinary frequency ranging from 56-58." On another occasion the pulse fell to 40: and Dr. O. states that he has frequently known it reduced from 75 to 80 in the minute, to 35 or 40. It must be remembered that, in the cases given by Dr. Norwood, the dose never—if we recollect—exceeded 12 or 20 drops of the tincture, always beginning with the minimum, and increasing drop by drop; but in the experiments recorded by Dr. Osgood, much larger doses were used.

There are so many circumstances which modify the dose of any remedy, that we are obliged in all cases to have our wits about us in the administration of medicines. The age, sex, and temperament, idiosyncrasy—all are to be kept in view in prescribing. Who among us has not been so unfortunate as, unintentionally, to salivate, with minute doses, persons unusually susceptible to the action of calomel? There are very many important circumstances to which the mind of every practitioner is directed, and in giving the *V. viride* a trial, it is important that facts only should be put on record. In one instance in which it was administered by us, two doses of twelve (12) drops of the tincture, at an interval of three hours, produced vomiting. This, we believe, was an accidental circumstance, depending upon the particular condition of the patient's stomach. We shall not, with our trifling experience, do more than urge a trial of the remedy upon the members of the profession. We have used it sufficiently, to warrant this recommendation, and properly administered, not the slightest danger is to be anticipated. We need, however, more facts—we need undoubted testimony in a great number of cases—we need information as to its peculiar action on the system: we must have a methodical arrangement of all that has been observed with regard to this remedy. "Facts can only," says Whewell, "become portions of knowledge, as they become classed and connected."

If the *Veratrum viride* really does possess the power of controlling arterial action, it is important that the profession should be put in possession of the fact. Dr. Osgood's experiments

were made about the year 1833-4, and his papers may be found in the American Jour. Med. Science, vol. IX.; and in the Amer. Jour. of Pharm, N. S., vol. I. In the Amer. Jour. of Pharm., N. S., vol. III., may also be found a paper on the subject by Mr. Marshall; and in the same journal, vol. IV., an article by Mr. Worthington, who found it to contain veratria, gallic acid, extractive, &c. The analysis of Mr. W., though far more accurate and complete than any previous one, requires repeated examinations. As the Hellebore is abundant all over this region, and as the season most favorable for gathering it is at hand, we cannot but hope that the profession will give it a fair trial.

ARTICLE XXXI.

Physiology of the Pneumogastric Nerve and Spinal Accessory of Willis. By JURIAH HARRISS, M.D., of Augusta, Ga.

(CONCLUDED.)

It will perhaps be remembered that I attempted to give, in the August No. of this journal, the relations of the pneumogastric and spinal accessory nerves and their agency in the production of voice. I shall now endeavor to show the influence of the par vagum upon the functions of respiration, circulation and digestion. It will be seen from references made, that I have derived most of the facts from Longet* and Bernard.† I will also add that all the experiments referred to, I have seen performed by M. Bernard in his course of lectures. Respiration is beyond all doubt influenced by the pneumogastric nerve. If the connections of the pneumogastric nerves with the brain are destroyed, the respiration will immediately diminish in frequency. In a dog the respiration is normally 20 or 25 per minute, but after these nerves are incised, the respiration amounts only to 15 or 16 in the same length of time. An animal rarely lives after this operation more than four or five days. Animal heat rapidly diminishes and continues to do so until death. The most important lesions, however, occur in the lungs. These organs become ecchymosed

* Longet's Anat. and Physiology.

† Bernard's Lectures.

and congested, and very considerable effusions of serosity and mucus take place in the vesicles and bronchial tubes. These lesions of course prevent the free admission of air into the pulmonary cells and greatly obstruct the arterialization of the blood, and hence suffocation to a more or less extent occurs. These effects, as shown by Bernard, are induced much more rapidly in the rabbit than in the dog.

Bernard has seen dogs perish, after cutting the pneumogastric nerves, and yet the post-mortem did not reveal the lesions already spoken of, as having occurred in the lungs. He therefore concluded that there must be other causes that operate in inducing death. He believes that in these cases, death is partially due to the absence of sugar in the blood.* He says, as will be seen by the reference, that the secretion of sugar by the liver (now uncontroverted) is influenced by the par vagum. If, then, the pneumogastrics are cut, the secretion of sugar will be arrested, and the fuel for the production of animal heat will cease to be furnished to the economy. As then the section of the pneumogastrics arrests the secretion of sugar, and as the heat of the body diminishes so rapidly after these nerves are cut, Bernard concludes that the absence of sugar in the circulating fluid is one of the causes of death. The absence of sugar and the diminution of animal heat is at least a striking coincidence.

After the section of the pneumogastric nerves, the movements of the œsophagus are arrested and the tube is paralysed. The lower portion of the tube, however, has a peculiar peristaltic action, and is not paralysed until after the lapse of some hours or a day. After the section of the nerves the animal will swallow, but nothing can penetrate into the stomach, the food being arrested at the cardiac orifice. When an animal has taken a certain amount of food, a few boluses may pass into the stomach in a purely mechanical way; one bolus pushing down the other. They are not passed into the stomach by the contractions of the œsophagus, as this tube is paralysed. If the animal continues to eat until the œsophagus is filled, the respiration will be embarrassed by pressure upon the pharynx

* Production of Sugar in the human system. See *Med. and Surg. Journal*, vol. vi., May, 1850.

and trachea, and the animal will attempt to relieve himself by vomiting. M. Bernard discovered the arrest of food in the œsophagus in a dog. He made a fistula in the stomach of an animal for the purpose of observing the effects that cutting the pneumogastrics would have upon digestion. He severed the nerves and gave the animal food, but to his astonishment none passed into the stomach until the next day. The few boluses that did enter the stomach were pushed down by mere mechanical force.

M. Longet with many other physiologists entertains the opinion that the movements of the stomach are dependent upon the par vagi. The experiment upon which he principally relies, is galvanizing these nerves after they have been cut. He has always succeeded in inducing stomachal contractions with this stimulus.

According to the experiments of M. Bernard the influence of the par vagi is essential to stomachal digestion. Their influence is the sine qua non, of the function, as it is by their nervous power alone, that the gastric juice is secreted.

In order to determine the effects that cutting the pneumogastrics would produce upon the stomach, Bernard formed a fistula in this viscus of a dog. Previously to severing the nerves, if the stomach was irritated, the mucous membrane would become red and congested and as secretion of gastric juice would take place. This effect can be more plainly seen, if the animal is made to fast a short time, before the stomach is irritated. If after the mucous membrane has been congested by irritation, the par vagi are severed, the membrane will be relieved of its cogestion, its surface will become pale and the secretion of gastric juice will be arrested. If food is now put into the stomach, it will not be digested and if allowed to remain it will decompose, which result can not occur in the normal State. In making this experiment care should be taken not to introduce food into the stomach before the nerves have been severed. If food is placed in the stomach previously to cutting the nerves, a certain amount of gastric juice will be created, and a partial digestion will be accomplished before the nerves can be exposed and severed.

M. Breschet in opposition to many authors, believes that the

sensation of hunger is dependent upon the pneumogastriks. His opponents aver that this cannot be, since animals eat after these nervous cords are cut. M. Longet, however, coincides with Breschet and thinks that animals under such circumstances do not eat, because they feel a local sensation of hunger, but from a general demand on the part of the whole system. He says that after a few days of fasting, debility, exhaustion and emaciation occur and the whole economy suffers for the want of nourishment. The desire for food then is not stomachal or a local hunger, but a general desire on the part of the whole organism.

The circulation is as manifestly under the control of the pneumogastric nerves as respiration or digestion. The normal pulsations in a dog amount to 80 or 90 per minute; they amount to 140 or 150 after severing the pneumogastriks. Narcotin when given to an animal, produces very decided trouble in the circulation and respiration. If the pneumogastriks are cut previously to administering the poison, the trouble in these two functions will not be produced. Bernard supposes that this substance acts first upon the brain and its influence is afterwards conducted to the lungs and heart by the pneumogastric nerves. If then the connections of the brain, with the lungs and heart are severed, the influence of the narcotine will be intercepted and cannot reach these organs to derange their functions. When this pair of nerves is cut the heart beats much more rapidly, but its force of impulsion is diminished and continues to decrease until it ceases to beat. The central organ of circulation, the heart, is but gradually paralyzed, as it is only after the lapse of several days that it ceases to perform its function. In order to determine the effects that would be produced upon the circulation, by destroying the connections of the pneumogastriks with the brain and heart, Bernard is in the habit of introducing into one of the large arteries, what he calls a hygrometer, (Magendie's instrument) different, of course, from the ordinary instrument of that name. This instrument is composed of a cup filled with mercury; from the top of the cup passes out a short, pointed tube, curved downwards. When the instrument is used, this tube is placed in the open extremity of an artery. Another tube, which is grad-

uated, passes up vertically from the top of the cup, with its lower end in communication with the mercury. When the instrument is applied, the blood is forced up the curved tube, presses upon the mercury and forces a column up the vertical or graduated tube. M. Bernard applied this instrument before the class, to the carotid of a dog. A column of mercury was forced up the graduated tube and sustained at a certain point by the contractility of the arteries. The blood oscillated from two to three inches above the fixed point by the contractions of the heart. The point to which the column of mercury was raised by the arterial contractility and the distance of the oscillations caused by the impulsions of the heart, being determined, one pneumogastric was cut; immediately the column of mercury was raised to a higher point, but the distance of the oscillations (the heart's force) was diminished. The oscillations, however, though smaller in extent, were more rapid. The latter points were determined and the other nerve was cut. The fixed column from arterial pressure rose still higher in the tube, but the oscillations occupied less space and were more rapid than before. The augmentation in constant pressure of the arteries, was owing to the collapse of the thoracic walls. The collapse of the walls of the chest always occurs after cutting the pneumogastric nerves. In the normal state, the arterial pressure augments during expiration, and after these nerves are severed, the same cause comes into operation to produce the same effect. In the latter case, the force is only increased. Galvanism applied to these nerves, or to the heart, after their connections with the brain have been severed, will immediately arrest the contractions of this viscus. This shows that the galvanic stimulus acts very differently upon the heart, from what it does upon the voluntary muscles.

ARTICLE XXXII.

A Case of Monstrosity. By Wm. C. BRANDON, M. D., of
Hermitage, Floyd Co., Georgia.

It should be the object of every one who writes for a public journal, not only to interest, but also to benefit the reader. The writer of this article, however, does not expect that it will

prove of any practical utility to the profession. But, from the infrequency of the occurrence of cases such as that given below, he hopes that it may not prove altogether uninteresting to the reader.

On the 16th of June, ult., I was called to Mrs. N——, in parturition. On my arrival at the house, I found that she had been delivered, some fifteen or twenty minutes, of a male child. Immediately on my entering the room, one of the ladies present informed me that "there was something very strange about the child, and wished it examined immediately:" she supposing that it had sustained some injury during labour. To my great horror, upon uncovering the child, I found a large portion of the intestine lying out upon its pubis and thighs. Upon a further examination of the case, I found a considerable opening in the abdomen, to the right of the umbilicus. The diameter of the opening was near an inch and a half, and almost entirely circular. The peritoneum, the entire body of the rectus at that point, and those portions of the transversalis and obliqui muscles that approach the umbilicus, and also the external integuments, were entirely wanting. The umbilical vessels, which seemed to be natural in size, position, &c., formed a portion of the left border of the opening. The skin and peritoneum appeared to have united upon the edges of the opening, the surface being smooth and entire, as the lips on other natural openings. The stomach impinged upon the upper border of the opening. The small intestines, beginning at the upper portion of the duodenum even as far as the cæcum protruded through the opening, and were (as remarked above) lying upon the pubis and thighs of the child. They were, or appeared to be much larger than natural at birth. From the size of the child and size of the protruded viscera, it did not seem that the cavity of the abdomen could possibly have contained more than half of them. This I find to be the ordinary condition of the parts in congenital umbilical hernia. In Todd's *Cyclopædia of Anatomy and Physiology*, vol. 2, p. 762, it is remarked, "In this affection, (speaking of umbilical hernia,) there seems to be a want of correspondence between the size of the viscera and that of the abdominal cavity, the former appearing enlarged and swollen." The protruding mesentery and bowel were

pretty firmly, and irregularly agglutinated, so as to present the appearance almost of a solid body. I made a slight effort to return them to their natural positions; but from the adhesions and size of the protruded mass, and smallness of the opening, it was impossible to succeed even in the least degree.

The other viscera of the abdomen, colon, liver, &c., seemed to occupy their natural positions; but I have been informed by the father of the child, that after a few hours other portions of bowel protruded. I was not able to detect any thing abnormal in relation to either the cord or placenta; nor could I perceive any other deficiency, or redundancy of development in the child. According to the calculation of the parents, the child was born at eight months gestation; and from its general appearance I would believe them to be correct. It was below the ordinary size, and quite feeble, and that plumpness which we usually see, even in small children, was wanting in this. Its breathing was not difficult, though the movements of the diaphragm appeared to be much more extensive than in an ordinary condition. This I supposed to be owing, in some degree, to a great portion of the resistance offered to the diaphragm in a normal condition, being taken off from below, as well as the inability of a portion of the abdominal muscles to perform their part in this function.

Deeming it impossible to save the child, or even benefit it in the least, I had it placed in as comfortable a condition as possible, and left it to the results of nature. It lived about twenty-four hours. I did not have an opportunity of seeing the case any more. No post-mortem examination was allowed.

This case, I suppose, would come under Dr. Denman's second class of monsters, namely: "Monstrosity from deficiency of parts." And under Vogle's third class, viz: "Malformations, in which parts normally united are separated from each other—fissures." Here we have a case of arrest of development; but at what period of gestation it may have commenced, or the causes or circumstances which produced it been in operation, is uncertain. In this article we shall not enter into any argument in regard to what physiological law may have been suspended or perverted, or what pathological condition may have existed to produce the results in the case. The reader will,

however, pardon me for introducing the opinions of the parents, as to the cause of the condition of their child—not that I, by any means, subscribe to their opinions; but as the reader is aware, whenever a child is born with any preternatural development or defect, the parent can *then* always refer to some particular circumstance or condition, as fright, “longing,” &c., as the cause by which it was produced. In this instance it was attributed to the mother having been exposed for several days, at an early period of gestation, to the annoyance of a dog which had been shot through the abdomen, but still living and going about the yard with the wound exposed to view. It may not be improper to state that the mother’s general health had been very feeble during the whole period of gestation. This was her sixth child. Her temperament is lymphatic: that of the father sanguineous.

ARTICLE XXXIII.

Death from Ovarian Tumor in a Child. By ROBERT CAMPBELL, M. D., Assistant Demonstrator in the Medical College of Georgia.

Sidny, a mulatto girl, aged about 10 years, had been for the last six months (thus far only could be traced in the history of the case) the subject of general emaciation, with excessive abdominal protuberance. She had been treated, domestically, time and again, for intestinal worms, upon the presumption of their being the cause of her unsightly peculiarity; but whatever success might crown the anthelmintic exhibition, there would be no parallel mitigation in the abdominal enlargement.

On the 6th of July ult., she was attacked with fever, attended with obstinate irritability of stomach and great intestinal distress; which condition, after several days’ duration, terminated fatally.

Autopsy, fourteen hours after death, revealed the following facts, viz: Peritoneal cavity filled with whitish serum; stomach exhibiting no traces of inflammation nor extraordinary congestion of its coats; spleen about thrice the size of the healthy adult spleen; bowels apparently normal, except those

portions occupying the right iliac and hypochondriac regions, and proximate to a large, dark-colored body resting about the right brim of the pelvis, possessing a portion of that cavity and extending into the right iliac region of the abdomen. Here, the ascending colon, cæcum and lower portion of the ilium, present a mottled aspect: in those portions lying contiguous to the tumor are extensive blackish spots, giving the appearance of partial decomposition; peritoneum generally very much injected, though especially in the neighborhood of the diseased mass, and also evincing this discoloration; the omentum majus containing but little fat, exhibits an inordinate degree of vascularity, which condition characterizes all the tissues in this vicinity; mesenteric glands are much enlarged, although apparently unaltered in structure; lumbar lymphatic glands also enlarged, resemble in appearance and structure the diseased organ.

Its attachments having been distinctly ascertained, the tumor was carefully dissected out, pains being taken to preserve its connections—the uterus and its appendages. Thus freed from all embarrassment, it was possible to study particularly the nature and relations of this object, which is the size of a man's fist, weighing 36 drachms; the color, externally, of ordinary liver, somewhat of a reniform shape, and presenting several nodules or rounded protuberances upon its circumference, which latter are much softer than the body of the mass—almost fluctuating to the touch. It appears to possess several serous tunics, acquired by adhesions contracted with the broad ligament of the uterus, and through its intervention with the greater omentum; and is connected by one portion of its surface to the upper angle of the uterus, by the right utero-ovarian ligament, and by another portion with the fimbriated extremity of the right Fallopian tube, by its Fallopio-ovarian ligament—which facts conclusively demonstrate it to be *the right ovary*. Upon this side, the ligaments are in a state of hypertrophy, as also is the Fallopian tube, the coats of which are much thickened, and the area of whose canal is greatly expanded. On the left side, these seem to have undergone a proportionate diminution. The uterus exhibits no mark of disease.

After this, both lungs were removed and submitted to care-

ful inspection, in quest of tubercular deposition, but no abnormal alteration could be detected, each portion being permeable and crepitant on compression.

The diseased ovarium, on being cut into, presents a variegated or mosaical appearance, containing several shades of purple, brown and red, interspersed with white matter, which in some places seems to predominate, being disposed in a semi-lunar manner. These structures are of a cellular character, and the cut surface is soon moistened by a dirty brown fluid, which appears to exude from the darker and more vascular portions of these section.

The tuberosities described upon the organ consist, internally, of a homogeneous brownish-red pultaceous matter—as if the process of *softening* had commenced at these points.

We have been thus minute in the delineation of the pathological appearances presented in the foregoing case, for the purpose of establishing, without the aid of the microscope, (which opportunity did not furnish) the histological nature of this tumor. And in a review of the most prominent features therein portrayed, we recognize satisfactorily the lineaments of carcinomatous disease. For in proof of its malignancy, we have the marked sympathetic condition of the constitution, as evinced in the emaciation, ascites, enlarged spleen and mesenteric glands, the enlargement and *structural alteration* in the neighboring lymphatics, the vascularity and *melanotic* appearance of the adjoining viscera, which latter, indeed, is not an unusual concomitant of *encephaloid cancer*. Besides, we have the more immediate evidences of the precise nature of the disease, in the physical characteristics of the local phenomena—viz., the immense development of the organ, the irregular tuberosc condition of its surface, its cellulo-vascular constitution, brainlike consistence, peculiar colors, and the *softening* of certain portions of the mass, which gives some insight into the historical relations of the disease. Hence, we deem it improbable that we should err in the diagnosis of this case, with the testimony of these data before us, when we determine of them, that they are the conditions constituting *cerebriform carcinoma of the ovarium*.

The one fact in its history, which appears to invest it with

peculiar interest is—the invasion by disease of this organ, at a period in its development, generally unobnoxious to morbid influences: diseased action ordinarily depending upon or being in some way connected with, the state of activity of an organ—in the ovaria, with the procreative stage of life. So far as our inquiry has extended, we have been unable to find on record, one instance wherein the ovarium has been the site of morbid action in so very youthful a subject. The occurrence of *this* disease, in *this* locality, so early in life, must be regarded as merely accidental, and not as resulting from the ordinary, conservative operation of the laws governing the natural economy.

We did not attend this case, consequently know no more of its particulars *ante mortem*, than heretofore cursorily described; but its fatal termination is probably attributable to the mechanical embarrassment or irritation exerted upon the abdominal viscera.

PART II.

Eclectic Department.

Digitaline.—*Reports of MM. RAYER, SOUBEIRAN and BOUILLAUD. Translated for the New York Journal of Medicine by H. N. BENNETT, M. D., of Bethel, Ct.*

[The following Reports (of which this translation is an abridgment), presented to the National Academy of Medicine by the distinguished gentlemen whose names are at the head of this article, have reference to the “Memoirs upon Digitaline by MM. Homolle and Quevenne,” and were made by them in the capacity of a commission deputed by the Academy for this special purpose. The first Report was made at the session of the 8th January, 1850, the second the 4th February of the present year.]

Chemical and Pharmaceutical Part.

The authors of the Reports, after presenting to the Academy some extracts from the work of MM. Homolle and Quevenne, showing the motives which induced these gentlemen to prosecute their labor, proceed to their own task, which is set forth in the following paragraph. Now that we have laid

before the Academy the reasons for which MM. Homolle and Quevenne have devoted themselves to new investigations upon one of those plants whose physiological and therapeutic power excites in the highest degree the curiosity, not to say the admiration of all true observers, we pass to the analysis of the memoir upon which we have been desired to report. This work is naturally divided into two parts; the one relative only to Digitaline, the other exclusively devoted to all the other principles which are found in digitalis. According to the declaration of the authors, the first part, that which treats of digitaline, is the only one which presents any interest in a practical point of view. They add, that it is only to this that they have wished to call the attention of the Academy, and that they have handed in the second part only as collateral information.

Be that as it may, MM. Homolle and Quevenne investigate in so many separate paragraphs; 1st, *the purification of digitaline and its chemical properties*; 2d, *the exhibition of digitaline*; 3d, *the medicinal form in which digitaline is best administered*.

To purify crude digitaline, our authors treat it by ether slightly alcohised (density 780,) which separates the digitaline. But this liquor dissolves also the *digitalose*, and leaves the *digitalin*. To separate the former from digitaline, the solution is submitted to evaporation, and the residue treated by alcohol at 60 degrees, which dissolves digitaline, and eliminates digitalose. Evaporated by a mild heat, this solution in weak alcohol finally gives digitaline *pure*, or at least retaining but very little of the two substances just mentioned, which cannot be completely isolated, so great, say MM. Homolle and Quevenne, is the *obstinacy* with which digitaline retains the last traces of these principles. These able experimenters have in vain made numerous attempts to crystallise digitaline thus obtained in a purer state than that described anteriorly by one of them.

This purification produced, digitaline appears under the form of scaly striæ or masses (according to the quantity of this substance) of a straw yellow, or resinoid aspect, more or less transparent, broken up with ease, and forming then a pale yellowish powder. Once isolated, digitaline is unalterable in the air, it gives out a slight aromatic odor *sui generis*, and has a very marked bitter taste.

Digitaline is dissolved in small quantity in the serum of the blood, without producing in it any apparent change. The proportion dissolved appears to be the same as in water. It is also dissolved in the filtered gastric juice. (If an attempt is made to dissolve digitaline in the crude gastric broth or chyme, it is absorbed by the alimentary matters, and its bitter taste

disappears, as is the case when animal carbon is thrown into its aqueous solution.)

Digitaline exerts no action upon either of the test papers. It is dissolved in small quantity in water, and, so to speak, in all proportions in feeble or concentrated alcohol.

The characteristic and distinctive property of digitaline is that of producing an emerald green color with concentrated hydrochloric acid, and forming a muddy solution.

Digitaline belongs, as has been said, above, to the class of immediate neutral principles. It combines neither with acids (tannin excepted) nor with alkalies.

To be certain of the *identity* or of the *quality* of digitaline, so important and truly capital a matter in a therapeutic point of view, MM. Homolle and Quevenne have had regard to the different degrees of bitterness of this substance; because, say they, "we believe it incontestable that its degree of bitterness is its degree of energy." To succeed in this test, it is necessary to proceed in the following manner:—

"Dissolve a given quantity of digitaline, one centigramme in two grammes of alcohol, and dilute with water progressively until the complete disappearance of the bitter taste. If the digitaline is good it should require for this quantity the employment of two quarts of water."

"With a little practice, and with the aid of a pattern specimen which serves for comparison, the degree of bitterness may be appreciated in a very exact manner. According to the result obtained, it is admitted as good, or requires purification."

This means of appreciating the intensity of the taste of substances, (called the method of progressive dilution,) already employed by other observers to judge of the richness of certain medicinal preparations, has been considered by MM. Homolle and Quevenne as a proper manner of obtaining digitaline always of equal strength.

MM. Homolle and Quevenne having thus succeeded in obtaining digitaline in that state which they call *identical*, it remained for them to determine what pharmaceutical form should be given to this medicine, having a fourfold reference to *safety, convenience, good preservation, and easy administration.*

The solution of this problem was the more important, since it concerned a product which possesses very great activity, an activity so great that it ought only to be administered by milligrammes, or, so to speak, in infinitesimal doses. It could not then, like sulphate of quinine, for example, and some other medicines, be prescribed in a direct manner and without a medium.

Among the different pharmaceutic forms, the choice of which might be a matter of dispute, MM. Homolle and Quevenne

adopted that of *pill*, and to this they think the preference should be given. This form itself includes, as every one knows, several varieties. The variety "which has appeared to us to excel," say MM. Homolle and Quevenne, "is that of very small comfits, in which the active matter is enveloped under a layer of sugar, in such a manner that the patient, by reason of their very small volume, may swallow them easily without mastication, and consequently without perceiving any thing except the sugar which covers them." By reason of their small volume, these comfits have been designated by the name of *granules*. Two great advantages, among others, are found in this medicinal form; 1st, a certain dose, easily controlled both by the physician and patient, since it is only necessary to count the number of granules, each of which contains a milligramme of digitaline; 2d, a preservation to which MM. Homolle and Quevenne do not thus far know any limit.

Physiological and Therapeutic Action of Digitaline.

MM. Homolle and Quevenne have not been tardy in communicating to the Academy the desired compliment of their first researches, under the title of "Third Memoir upon Digitaline."

In this new Memoir, the authors propose to establish, "that digitaline is the sole active principle of digitalis; that it presents all its physiological and therapeutic properties, and that the constancy of its effects, as well as its inalterability, give to it an incontestable advantage over the pharmaceutic preparations of digitalis."

But let us see what are the physiological and therapeutic properties attributed to digitalis by the numerous observers who have devoted more or less important labors to its investigation.

1st. A constant emetico-cathartic action when it is administered in a sufficiently large dose.

2d. A diuretic action generally observed, although more rare.

3d. A very remarkable and altogether special action upon the circulation.

Besides, it provokes a slight cerebral irritation, characterized by dimness of vision, vertigo, cephalalgia, sleeplessness, and delirium.

Insisting, then, more particularly upon the *special* property of digitalis, we might say almost *specific*, our authors endeavor to determine the mode of action which digitalis exerts upon the central organ of the circulation, and afterwards the indications which this therapeutic agent is called to fulfil.

The French physicians in general, they say, consider digitalis as a sedative of the circulation; the English physicians recognize in it a primitively excitant action upon the circulatory

centre, which is depressed only secondarily. Rasori places digitalis in the first rank of contra-stimulants, and makes it a succedaneum of blood-letting.

Joerg, of Leipzig, concludes from his researches upon digitalis, that it determines depression of the vascular system only consecutively.

“W. Hutchinson, who made conscientious and persevering experiments upon himself, verified the primitive increase of the action of the heart, followed by depression and disorder of this function.”

To fulfil, as worthily and as completely as possible, the important and delicate task which the Academy has confided to us, we have not limited ourselves to presenting merely an accurate analysis of this new portion of the work of MM. Homolle and Quevenne. It seemed necessary for us to remind our colleagues of the actual state of science in regard to the properties of digitalis, and to render an account of the clinical experiments to which we have devoted ourselves, in order to appreciate as rigorously as our methods of observation at the present time permit, the action of the new *active principle* of digitalis, the discovery of which belongs to the authors just named. This is the manner in which we have proceeded, and thence naturally, the division of our report into three parts; the first, devoted to a rapid historical sketch of the opinions advanced upon the properties of digitalis; the second, relative to the exposition of the researches of the authors of the memoir upon the properties of digitaline; and the third, containing the experiments of the commission.

Analytical Exposition of the Physiological and Clinical Experiments of the Authors of the Memoir upon the Properties of Digitaline, compared with those of the other preparations of Digitalis.

ARTICLE FIRST.—*Physiological Experiments upon Man and Dogs.*—One of the authors made a trial of digitalis upon himself, with a truly exemplary precision, and repeated it seven times, leaving sufficient intervals between the trials.

From the analysis of the six first experiments made in the course of the years from 1842 to 1850, the result is, that the mean of the diminution of the pulsations of the heart and arteries was about four pulsations during, and five after, the administration of the medicine (sometimes digitalis, sometimes digitaline.)

But we should note here the symptoms of intoxication which were manifested during the 5th experiment, under the influence of two grammes and one decigramme of the powder of digita-

lis, of a superior quality, taken in the course of nine days, and the very notable diminution of the pulse which supervened at the same time. (From 72 it fell to 53 during the continuance of the symptoms, and to 55 after their disappearance; thus giving the differences, 19 and 17, that is to say, a mean difference which represents exactly one-fourth of the normal pulsations.)

We think it necessary to report, with some details, the seventh and last experiment, made in the month of Nov. last (1850,) with the syrup of digitaline, 20 grammes of which contain a milligramme of this substance.

The experimenter administered, in eight days, 33 milligrammes of digitaline in syrup (4 milligrammes during four days, 2 one day, 5 one day, 6 one day.)

<i>Mean of the pulse during six days, counted at different hours of</i>							
the day, before the experiment,							
	-	-	-	-	-	-	67.47
<i>Maximum,</i>	-	-	-	-	-	-	73.00
<i>Minimum,</i>	-	-	-	-	-	-	63.00
<i>Mean of the pulse during the eight days of administration,</i>							
<i>Mean during the ten days after administration,</i>							
<i>Minimum during the administration</i>							
<i>Maximum,</i>							
<i>Minimum after the administration,</i>							
<i>Maximum,</i>							
	-	-	-	-	-	-	72.00

Comparing the mean of the normal pulse (67.47) with the minimum of the same pulse, after the administration of the medicine (50,) we find a difference of 17.47, a difference equal to one-fourth of the pulsations in the normal state, the same as in the case of intoxication which we have noted above.

The patient suffered rather a sense of weakness than of pain in the stomach, and he observed that his appetite did not increase, as often happened to him after the use of the granules; slight disturbance of vision: nothing appreciable on the part of the kidneys. The experimenter remarked that the pulse returned suddenly to the normal state, or nearly so, under the influence of a long walk made the tenth day after the cessation of the exhibition of the syrup.

From the whole of the experiments, the result is, that the action of digitaline is nearly the same, whether we administer it in the state of syrup, that is to say, in previous solution, or in the state of granules, in which case it is dissolved gradually but with sufficient promptness in the gastric juice. If any differences exist, they are slight, and do not appear to declare in favor of the syrup, which gives rise to nausea and more marked cerebral symptoms.

Let us pass now to the experiments made upon two dogs,

with the same precision as those the results of which we have just made known.

The first dog took interruptedly, but at six not very distant intervals (from the 10th of May to the 3d Sept., 1849,) 552 granules or milligrammes of digitaline, in a daily dose of from two to ten milligrammes.

The mean of the pulse, before the experiment, was 59.94. This mean after the sixth successive administration, was 51.22. being a diminution of 8.72.

The second dog took in the same manner (from the 13th June to the 8th Sept., 1849,) 398 granules of digitaline in the same daily dose.

The mean of the pulse, before the experiment, was 87.30. This mean, after the administration at four approximate intervals, was 69.93. Consequently, the diminution was 17.37. Add to this, that during the experiment, there were momentary diminutions amounting to 23 and even 31 pulsations, the latter being nearly double that just mentioned.

According to the experiments above analyzed, the *minimum* of the depression of the pulsations never corresponds, so to speak, with the period of administration of the digitalis or digitaline, but rather to that of repose after the cessation of the use of the medicine.

This curious result proves that the medicine continues its action some time after the cessation of its exhibition, and, without doubt, until no trace of it longer remains in the economy; whether its diminution is produced by any one of the numerous emunctories, or whether by a reaction yet unknown, it is transformed into some new compound completely deprived of the property which pure digitaline possesses.

ARTICLE SECOND.—*Clinical Experiments Reported by MM. Homolle and Quevenne.*—"It concerns us now to examine," say these authors, "whether ulterior practice has confirmed our first results. The facts collected by us, from observations taken in several hospital services, furnish an affirmative reply upon this subject.

"As to the *intolerance* of digitaline, which is in our opinion only a manifestation of its emetico-cathartic action, we have encountered it but three times out of more than a hundred persons to whom we have administered this medicine; and in these three cases, discontinuance of the digitaline, or even diminution of its dose, was sufficient to suspend very quickly every unpleasant symptom.

"We think that those of our colleagues who have observed more obstinate gastric symptoms, should attribute them to this

circumstance, that they commenced with too high doses or exceeded those approved by experience.

“As for the toxic action noticed by MM. Bouchardat and Sandras, who having injected digitaline into the veins of some dogs, saw these animals perish in a short time, these facts, which have become the basis of exaggerated fears, have been badly interpreted. No account is taken of the mode of absorption, which so profoundly modifies the action of medicines, and of the danger which this may of itself produce. How many therapeutic agents, in fact, would medicine be compelled to renounce if it were necessary to put them to the same test? We can say truly, that these toxic phenomena, independent of the emetico-cathartic action, have never presented themselves to our observation. We may add, that vomiting, when digitaline is administered by the stomach in an exaggerated dose, frees the economy from the excess of the ingested medicine, and fulfilling, so to speak, the office of safety-valve, presents an impediment to the development of true toxic symptoms.”

As to the comparative efficacy of the preparations of digitalis hitherto used, and digitaline, MM. Homolle and Quevenne leave to clinical experiment the burden of a final decision. They think that their own observations and those published hitherto, lead to the belief that the question will be resolved in favor of digitaline.

After these preliminaries, MM. Homolle and Quevenne report their own observations.

Having detailed eight cases, they add that, “in order not to increase the size of their memoir, they have omitted several observations of pleuritic effusions, and one of pericarditis, in which digitaline appeared to them manifestly to increase the resorption of the fluid; that they have also neglected those observations which only go to establish the fact that digitaline, like digitalis, diminishes the number of the pulsations of the heart, the experiments quoted in their first memoir leaving no doubt upon this point.”

The commission, strengthened by its own experience, had no need, on its part, of more numerous observations, to be convinced that to digitaline belongs, no less than to digitalis, the admirable property of retarding the pulsations of the heart and arteries. But it received with great eagerness facts proper to demonstrate, in an irrefragable manner, that digitaline possesses an action, a diuretic virtue, sufficiently energetic and constant not to be called in question by any of the observers who have long and conscientiously studied, at the bedside of patients, the real effects of digitaline.

MM. Homolle and Quevenne sum up thus their [pharmaceutic] parallel between digitaline and digitalis:

DIGITALINE.

An unalterable type, to which digitaline may be invariably referred.

Constant energy of the medicinal agent.

Possible appreciation of the quality of a given specimen of digitaline by the measure of the intensity of its bitterness, which ought to be such as to require two quarts of water to destroy the bitterness of a centigramme of digitaline.

The extreme facility of its administration to every person, without exception.

DIGITALIS.

Impossibility of preserving unalterable a specimen which will serve indefinitely as a point of comparison.

Unavoidable uncertainty in regard to quality, and consequently the degree of activity of the plant employed.

Absence of any positive character, upon which we can depend, to appreciate the difference of quality between the different specimens of digitalis.

Difficulty of administration to some persons, in consequence of its taste and smell.

To decide the choice in favor of digitaline, and cause the balance to incline definitely to its side, it is only necessary to cast into the scale the weight of a long and exact clinical experience. MM. Homolle and Quevenne have perfectly comprehended this. "As to the difference of action which may exist," say they, "between digitalis and digitaline, and the preference to be accorded in this respect to the one or the other, *it is for clinical experience to determine*. We may, for the present, refer to the testimony of Professor Bouillaud, who has studied the action of digitaline for several years, and does not hesitate to say, relying upon a long experience, that it far excels every other preparation of digitalis, in this, that it is less liable to cause vertigo, dimness of vision, ringing of the ears, &c."

The following fundamental propositions sum up all the researches of MM. Homolle and Quevenne, and to these the Reporters desire especially to call the attention of the Academy:

1. Digitaline (properly prepared) represents all the therapeutic properties of digitalis.
2. Digitaline exerts a regulating action upon the circulation, and retards its movements. This action, which is *essential* and nearly constant, requires only feeble doses, (ordinarily from two to five milligrammes in twenty-four hours, in adults.)
3. If we exceed the dose of four or five milligrammes in twenty-four hours, digitaline exerts, an emetico-cathartic action, sometimes harsh and sudden, sometimes slow and gradual.
4. Digitaline produces a toxic action when it is absorbed in

large doses. This action has been produced by injecting into the veins of a dog one centigramme of this substance. But, when administered by the stomach, the toxic action does not appear as dangerous as is generally supposed, the excess of the medicine being expelled from the economy, for the mere reason that it is not tolerated.

5. Compared with the powder of digitalis, which is considered as the best pharmaceutic preparation of this plant, digitaline should have the preference, since it offers greater facility of ingestion, a more certain action, and a more constant tolerance.

6. MM. Homolle and Quevenne add in a note, that digitaline produces also two other order of phenomena; a diuretic action and an excitation of the nervous centres, but that this double action is far from being constant.

7. Finally, among the effects of digitaline we shall notice the following, mentioned in the course of the work of MM. Homolle and Quevenne:

I. *Action upon the Eyes.*—In the course of the chemical manipulations, some fragments of digitaline being projected into the eye, the following was the result—a slight painful sensation in the organ, and after four or five hours, disturbance of vision in such a manner that the flame of a taper appeared surrounded by an areola, having the colors of the rainbow. This effect was dissipated in the space of one day, without leaving any trace of it. During its continuance the pupil was a little dilated, appeared less contractile, and the crystalline lens appeared to present a certain degree of opalinity.

The effect above indicated is produced also when a person remains a length of time in an atmosphere charged with pulverulent emanations of digitaline.

II. *Action upon the Skin denuded of its Epidermis.*—Applied upon the denuded skin, in the feeble dose of a milligramme, and renewed at intervals of eight or ten hours, digitaline provokes a lively inflammation, with swelling, redness, and disagreeable numbness of the neighboring parts. This effect deprives us of the advantage of administering digitaline by the endermic method, a method so valuable in certain cases.

Clinical Researches made by the Reporter, [Bouillaud.]

For about four or five years that we have administered digitaline in our clinical service, perhaps not a single day has passed in which we have not studied its action upon one or more subjects, attacked either by simple neuroses of the heart, but chiefly by more or less grave *chronic-organic* affections of this viscus and of the aorta; we have also administered it in six or eight well-marked cases of intermittent fever.

The total number of patients to whom we have given digitaline, amounts to, at least, one hundred and fifty or two hundred of different age and sex.

With the exception of three, we have observed in all a more or less considerable retardation of the pulsations of the heart and arteries, an effect which could not be attributed to any other cause than the administration of the medicine indicated. We hasten to say that in the three patients, in two especially, whose pulse was not sensibly retarded under the influence of the administration of digitaline, sufficiently prolonged, there existed a frequency of pulse kept up by a febrile phlegmasia. In two of these three cases, this phlegmasia had its seat in the envelopes of the heart (pericardium and endocardium), the same thing which was encountered in one of the cases reported by MM. Homolle and Quevenne.

It is true that three or four observations alone cannot resolve a grave question of therapeutics. But those to which we have referred authorize us to consider as very probable, at least, this proposition: that the frequency of the pulse entertained by our inflammatory *foyer*, presents an obstacle to the effect of digitaline, against which it almost completely fails.

In several cases in which the acceleration of the pulsations of the heart (palpitations) was accompanied by irregularities and intermissions, by disorder, *ataxia*, of the normal rhythm of *this great spring* of the human machine, digitaline, like digitalis, not merely quieted the palpitations but also regulated, sometimes completely, sometimes incompletely, and for a longer or shorter period, the disorderly pulsations of the circulatory centre, and, in a manner, *restored this organ to reason*, which before—to use a common expression—*was light-headed* (*battait lacampagne*.)

Digitaline and digitalis are, then, at once a *retardator* and a regulator of the action of the circulatory centre. They constitute, also, a debilitator of this same action. In fact, in all cases in which the pulsations of the heart have been retarded in a notable manner by digitaline, they have at the same time lost their force or their intensity; (so also of the normal or abnormal *bruits* of the heart.)

With a special dynamometer, or with the *sphygmometer*, the different degrees of this diminution of force might be *exactly* determined. Hitherto we have only appreciated this diminution with the hand, a sort of living and feeling *dynamometer*, which, by long training, acquires, like other organs of the same nature, an exquisite and—so to speak—a *mathematical* sensibility. Inspection concurs with palpation in our appreciation of the intensity of the pulsations of the heart. Finally, if the

stethoscope is applied over the region of the point of the heart, this instrument is lifted up, undergoes a kind of extension, which is proportionate to the strength of the impulse of the circulatory centre.

We shall give, now, more precision to what we have hitherto said upon the retardation of the pulse, by laying before the Academy the *numerical* results furnished by fifteen patients, taken indiscriminately.

The mean of the pulse, before the administration, was ninety-six. The mean of the retardation in these fifteen cases was forty-one, consequently about one-half the number (ninety-six) which represents the mean before the administration of digitaline.

In three cases, the number of pulsations to which the action of the heart was reduced remained as high as eighty, one hundred and two, one hundred and six. But these were patients attacked by grave *chronic-organic* affections of the heart, in whom the pulse beat from one hundred and forty to one hundred and seventy per minute.

In comparison with these *maximum* numbers of retardation, let us place those which represent the *minimum*. The latter are twelve and fourteen in two patients, and sixteen in two others; but, in one of these two last, the pulse beat only sixty before the exhibition of the medicine; and in the other, whose pulse was eighty-two, the digitaline was administered only during four days, (in all sixteen milligrammes.) In the two others, the pulse was seventy-two in the first, and eighty-six in the second, who took the digitaline for seven days only.

The *mean* of the number of granules administered to our 15 patients, was 58. They were taken in the dose, of 2, 3, 4, 5, 6, 7, per day. The mean of the number of days during which they were administered, was from 13 to 14.

In one subject, we gave 70 milligrammes in 18 days; in a second 82 in 14 days; in a third 98 in 20 days; in a fourth 164 in 40 days. None of the patients suffered any remarkable symptom. As soon as headache, vertigo, or distress at the stomach supervened, we immediately suppressed the medicine.

In the 150 or 200 patients for whom we have prescribed digitaline, we have taken the pains to have the medicine administered in our presence at the time of our visit. In those cases in which the dose was taken in two parts, that of the evening was administered by the chief of the clinique himself, or by the matron of the hall, to those patients who were to take digitaline upon going to bed. One only, of whom we have spoken above, has manifested grave cerebral symptoms, which however completely disappeared under the influence of the treatment directed against them.

One of the 15 patients who have made the subject of our statistical *résumé* belonged to the category of six or eight persons attacked by well marked intermittent fever, whom we have treated with digitaline in order to ascertain whether this principle possesses the febrifuge virtue which we have anteriorly verified in the powder of digitalis administered internally and externally (by the endemic method.) These six or eight persons, treated thus in the presence of those who followed the clinique, were cured very promptly and without relapse. Under this new point of view, digitaline may replace, then, the preparations of digitalis formerly used.

In noticing these facts, having an authenticity which cannot be contested, the reporter has no other object than to call the attention of observers to a new and very curious property of digitalis, and does not pretend to maintain that this plant can be advantageously substituted for quinine, which he considers, on the contrary, the first, and, so to speak, the prince of febrifuges. But ulterior researches will demonstrate, perhaps, that digitalis is called to figure, with some distinction, among the best succedanea of Peruvian bark.

Now even admitting that digitalis, in spite of the oft-repeated experiments of the reporter, does not possess a febrifuge power, it is very certain that it possesses the property of retarding, moderating and regulating the pulsations of the heart, and that it does this not in a secondary consecutive manner, as different experimenters have pretended, but in a primitive and immediate manner. A proposition not less certain, according to the researches consigned in the Memoir of MM. Homolle and Quevenne, and according to those of the commission, is, that digitaline is the principle to which digitalis owes the precious and admirable property which we have mentioned, as quinquina owes to quinine the property, certainly not less precious and less admirable, of curing intermittent fevers.

On the Physical Examination of the Abdomen. By CHARLES J. B. WILLIAMS, M.D., F.R.S.

The physical examination of the abdomen by *palpation*, is a matter of importance, and demands a few directions and cautions. The parts of the hands best qualified to feel are the palmer surfaces of the fingers, the pulpy portions of the last phalanx being endowed with the finest touch. But they should be flatly applied, at least at first; as nothing is more likely to start the abdominal muscles into rigid contraction than to poke them abruptly with the ends of the fingers. For the same rea-

son the observer should not be too much above the patient ; and if the bed on which the patient is placed is low, the practitioner should sit or kneel beside it, in order to bring his arms and hands more on a level with the abdomen. It is equally obvious that the observer's hands should be comfortably warm, not only for the sake of the patient, but also because the touch is more sensitive in that condition. The posture of the patient generally best suited for the examination is the supine, on a mattress or sofa, with the limbs lying straight, but without the least straining or muscular effort. It is often recommended to draw up the knees in order to relax the abdominal muscles ; but this action is very apt to produce just the contrary effect, and further interferes with the free accessibility of the lower parts of the abdomen to the observer's hands. It is the great object, that whilst the body and limbs are so equally and completely supported as to be in a state of perfect relaxation, the whole abdomen shall freely admit of the application of the hands to every part, and in every direction. This further suggests, that in all cases requiring nicety in manipulation, the abdomen should be laid bare, the clothing sufficiently put out of the way, and that either the bed be narrow enough, or the patient be moved to each edge of it, so that the observer may be able, if necessary, to examine from both sides ; this is especially requisite in the deep palpation to be described further on.

The kind of tact by which the different properties of *shape*, *softness*, and *hardness*, *elasticity*, *weakness*, &c., are distinguished, is so instinctive as scarcely to be capable of description ; but it may somewhat guide a novice to say, that we trace the *shape* by applying the flat fingers and gently rubbing them over and between the prominences and depressions. We test the *softness* or *hardness* by pressing gently with the pulpy surfaces of the last phalanx on the successive parts over which the hands are passed ; that which readily yields being soft, while the hard resists the pressure ; the *elastic* opposes it to a spring, which, although yielding to strong pressure, sensibly pushes away the fingers as their application is relaxed. *Weight* is felt by lifting upwards a part, and observing what force is required to do this, and with what force it presses down again on the fingers as they are slightly withdrawn ; trying the weight of part of the abdomen can be practised only in the lumbar and lateral regions in the supine posture ; the anterior regions may be in a measure lifted and weighed by the hands, when the patient lies on his face, or sits leaning the body forwards ; and this kind of examination is often of much importance in the diagnosis of ascites and solid visceral enlargements.

In the healthy state, the abdomen is in every region soft and

moderately elastic; it feels smooth and uniform to the hand rubbed gently over it; when pressed superficially or deeply, its resistance is elastic and increasing with the pressure; when percussed on one side, no shock or fluctuation is felt on the opposite side; the different parts when lifted, feel pretty equal in point of weight. This uniformity of soft elasticity represents the intestinal canal in its normal state, containing a moderate amount of gaseous, liquid, and semi-solid matter, pretty uniformly distributed through it. As observed by inspection, slight prominences may often be *felt*, corresponding with the stomach, the cæcum, and occasionally below the umbilicus; but these also feel soft and elastic as other parts, and do not detract from the generally equable character of the healthy abdomen.

Variations from this normal state of the abdomen may arise from alterations in the walls; and it will be well to advert to these before describing those caused by disease of the interior. Excess of fat gives a fullness which will feel firm and elastic, or soft and flabby, according to whether it is on the increase or on the decrease; but it may be known to be fat by its peculiar lumpy feel on the surface, so that it may be pinched up by the fingers and thumb; and in this way the thickness of the layer may be estimated. In the case of very fat paunches, there are masses of fat also in the omentum and mesentery; these form the large-rounded prominence of the *pot-belly*, which renders the palpation of the abdominal viscera very difficult. In order to reach them in such a case, it is necessary to press firmly into the fat, and then to feel by moving the hand in a circular, or backward and forward direction; this may be best accomplished in the iliac and hypogastric regions, where the fat in the walls is not so thick as in the central regions of the abdomen. Where a corpulent person has lost much flesh, which is often the case in chronic disease, the loose flabbiness of the pendulous abdomen may embarrass the practitioner; and the more so, as the fat which is left often has a granular or knotty feel, which might be mistaken for formations within the abdomen. These inequalities are to be distinguished by their being superficial, and capable of being grasped between the fingers and thumb; and the examination of the deep parts is to be effected by the hands steadily pressed inwards, pushing aside these inequalities, and rolling about or handling in divers ways the viscera underneath. Œdematous walls are to be treated in a similar manner, are distinguished by their inelastic, boggy feel, and pitting on pressure.

Permanent tension of the abdominal muscles is a common impediment to the palpation of abdomen; it causes a feeling

of superficial hardness, especially in the direction of the recti muscles. If there be any doubt as to its seat, it may be dissipated by keeping the flat hand steadily and firmly applied with increasing pressure for a few minutes, whilst the patient's attention is taken off by conversation on another topic. The muscles will then be found either to relax altogether, or so much to vary their degree of tension, as plainly to reveal the cause of the hardness. This test is equally successful, whether the muscular tension be caused by nervous irritability, or by actual pain or tenderness; for even in the latter case, flat pressure, gently and very gradually applied, may be managed so as not to increase the pain.

Extreme flaccidity of the walls of the abdomen might be supposed to remove all sources of fallacy in the manual examination of the abdomen; and it is, in fact, an easy matter to feel the place, shape, and consistence of almost every abdominal viscus; yet, under such circumstances, it is not very uncommon to find the pulsation of the abdominal aorta mistaken for an aneurism, and I have known the anterior convexity of the lumbar spine mistaken for a tumour. These errors arise from the observers not being aware how deep in the abdomen the hand reaches, and would be readily corrected by placing the other hand behind the back, and by thus feeling how small is the distance between the two hands.

Palpable Signs of Disease in the Abdomen.—*Feculent accumulations* occur chiefly in the large intestines and the lower part of the ileum; but the remaining portions of the small intestines are commonly more or less distended with gas. Hence the abdomen is generally enlarged; and the fulness feels more elastic around the umbilicus, and more resisting, yet not absolutely hard, in the iliac and hypochondriac regions. The course of the large intestine may often be traced, the greatest prominences being usually caused by the cæcum and transverse arch; and if the abdominal walls are not tense, it is quite possible, by the pressure of the fingers, to distinguish the feculent masses by their substantial but inelastic feel, from the tympanitic contents of other parts of the intestine. If the accumulation be considerable, there is a very appreciable difference in the weight of different parts of the abdomen, as tested in the mode before described. If there be an excessive accumulation of air in the intestines, this distends them so much that the feculent masses cannot be felt, and the whole abdomen has a tight elastic feel of rounded shape, but commonly showing more prominence in some parts than in others.

In *colic*, and constipation with severe *colicky pains*, there may in most instances be felt at parts, depression, correspond-

ing with contracted portions of intestines, which are hard and knotty, in comparison with the tympanitic elasticity of other parts. The abdominal muscles are often spasmodically contracted over these painful parts; but they become eased and relaxed under the steadily increased pressure of the flat hand. In these cases the weight of parts of the abdomen, as tested by lifting them when the patient is in the prone or leaning forward posture, is less than usual, the contents being proportionally lighter, and prevented from ponderating by the tight contraction with which they are bound.

The *tympanitis* of low fever and sinking states of paraplegia, from destruction of the lower portions of the spinal cord, or serious injury to its reflex function, is distinguished by the more uniform and less tense elasticity of the abdominal enlargement. The deficient tone of the intestines and abdominal muscles in these cases causes a larger and easier distension; and although the quantity of air pent up may be much greater, the tightness and resistance is less than in colic.

The presence of *liquid in the peritoneal sac* is capable of altering, in various ways, the palpable qualities of the abdomen. The size and shape may be felt to be changed in the same manner already described as obvious on inspection; that is, the size is more or less enlarged, with bulging projections in the most dependent parts; the shape of the parts occupied with fluid is smoother and more uniformly rounded than usual, and in extensive accumulations the surface may feel polished. But the resistance, as felt by pressure, is also distinctive; it is peculiarly soft, inelastic, and heavy. These qualities are most obvious where the abdominal walls are thin and flaccid; and under these conditions, there is no difficulty in detecting by the feel only, even moderate quantities of fluid in the abdomen. Ponderating to the most dependent parts, the fluid is to be felt in the flanks in the supine posture; at or near the umbilicus in the prone posture; in the iliac region when the patient lies on a side; and in the hypogastric region, when the patient sits leaning forward. The soft and mobile, yet weighty feel of these dependent parts, is to be compared with the more elastic and lighter resistance of other regions occupied by air-filled intestines; and by making the patient change his posture, allowing two or three minutes to give time for the subsidence of the liquid, we may have the further evidence that it is free in the peritoneal sac, if we feel it still showing itself in the most dependent parts. It does not often happen that there is enough liquid in the intestines to give any thing like the same superficially soft and heavy sensation under the fingers; and when there is, it may be distinguished by its not thus gravitating from

one region to another, as well as by its giving less sensation of superficial softness, and more of a gurgling or elastic feel, from the presence of air.

In proportion as the quantity of fluid in the peritoneum becomes greater, the distension of the walls also increases and hereby the softness of the abdomen is diminished, whilst the weight is more palpable. The liquid still gravitates in greatest volume to the lowest parts; but it also spreads more or less upwards in a layer between the viscera and walls of the abdomen; and gives a smoothness of surface, and uniformity of the peculiar weighty fluctuating resistance which characterizes its presence. So long as the distension is moderate, the air-filled intestines will usually float on the surface sufficiently to be in contact with the uppermost portion of the abdomen; and this will, of course, vary with the posture of the patient: that part can generally be distinguished by a delicate touch, which may be verified by the more obvious difference of percussion.

But there is a palpable sign of the presence of liquid, often manifest with a moderate degree of distension, more characteristic than any sign, not excepting those of fluctuation and percussion; namely, the striking of the fingers against the liver, or any other resisting body beyond the surface of liquid. It is plain that this sign will be most evident where there is considerable enlargement of the liver, spleen, or some other solid body; and as the liquid and solid become both more distinguishable on account of the contrast in the resistance which they respectively offer to the fingers, the sign serves to detect the presence of both. Thus, a slight enlargement of the liver, reaching two or three inches below the margin of the ribs, may not offer resistance enough to be distinctly palpable by itself; but if there be a layer of serum interposed, and this be abruptly displaced by the fingers, they will strike the solid body with a distinct shock which will unequivocally prove the presence of both fluid and solid. It so happens that in a large proportion of the cases in which there is fluid effused in the peritoneal sac, there is also more or less enlargement of the liver, or some other large viscus; hence this sign becomes more generally useful. By its means the author has been enabled to detect the presence of even thin deposits on the omentum and intestines, and of deeper-seated mesenteric and ovarian tumours. Solid feculent matter in the intestines may give a similar resistance to the fingers under a layer of fluid; and it is important that this source of error should be avoided, by having the bowels well cleared by aperients before the examination.

In manipulating for this sign of liquid and solid resistance,

the fingers should be gently pressed more or less into the walls, and then a deeper impulse is to be abruptly made with their ends, so as to strike at any body which may offer resistance, and which then meets the stroke. Over an enlarged liver or spleen, this sign is best felt in the sitting or erect posture; or if the quantity of fluid be considerable, in the prone or leaning forward position. Indurations of the omentum and intestines are better detected when the patient lies on the back. In considerable enlargements of the spleen or left kidney, the author has found this sign in the left lumbar region; but in other cases the front of the abdomen is its usual seat.

On two occasions, through a modification of this sign, the author has diagnosticated the presence of something adhesive on the striking surfaces. One was a case of ovarian tumour with ascites; the other, chronic peritonitis with enlarged liver. In both, the fingers readily felt the displacing fluid and the striking against the solid underneath; but on withdrawing the pressure, the surfaces in contact could distinctly be felt to adhere for an instant before following the fingers, being retained by something adhesive. This sticking sensation was plainly felt by several students, to whom the author pointed it out at the time, ascribing it to a coating of adhesive lymph. The *post-mortem* examinations did not occur till long after, but there was in both cases a deposit of false membrane on the surfaces which justified this inference. Probably this sign may prove useful in aiding to detect the presence of the low forms of peritonitis which frequently accompany abdominal tumours.

Fluctuation has long been recognised as a characteristic sign of the presence of fluid in the abdomen. It is commonly felt on applying the left hand on one side of the abdomen, and striking abruptly on the opposite side with the right hand, so as to direct the impulse towards the other hand. A wave is moved by the impulse, and rapidly passing to the opposite side, gives to the left hand the soft shock of fluid in motion. This wave-stroke is transmitted from one side to the other only when there is a continuous layer of fluid. If a fold of intestine rises to the surface in the centre of the abdomen, it intercepts the wave. Hence it often happens, where the quantity of liquid is moderate, that the fluctuation is felt at the lower part of the abdomen in the sitting or standing posture, and not when the patient lies on his back. The distinctness and character of the fluctuation vary according to circumstances that can be pretty exactly defined. The most remarkable degree is that produced when the abdomen contains much fluid, whilst the walls are thin and their tension is increased by a good deal of air in the intestines beneath the fluid. This adds to the elasti-

city of the walls; and wherever struck, they instantaneously transmit the shock with a kind of spring to every part of the surface; and so faithfully that if a quick tattoo be played with the fingers on any part of the abdomen, each stroke can be felt in every other part with the greatest nicety. This highly vibratory fluctuation may therefore be considered a sign of flatus in the intestines, together with liquid in the peritoneum. Nor is this indication unimportant in its practical relations, for it leads to the use of mercurial purgatives and carminatives; which, by dispersing the wind, not only relieve the tension of the abdomen, but also bring the organs into a state more favourable for the absorption of the fluid.

When the distension of the abdomen depends on an excessive quantity of fluid alone, the fluctuation is still very distinct, but different in character from the last variety. It is less vibratory and spring-like, strikes the feeling hand with a deader, heavier shock, and requires a stronger stroke from the other hand to set it in motion. This kind of fluctuation, together with the peculiarly weighty feel of the abdomen in every part, when lifted with the hand, and the equally dull sound on percussion, is a conclusive proof of the presence of a vast quantity of fluid, such as can seldom be removed without the operation of tapping. In fact, the same weight and pressure which we feel through the walls of the abdomen, is also oppressing and obstructing the organs of circulation, absorption, and secretion, to such a degree as to impede their functions, and to render them little amenable to purgatives and diuretics, which ordinarily excite their action. Hence, with such a state of abdomen, we find the urine and faeces continue very scanty, and vitiated in spite of all the remedies we employ.

A moderate amount of liquid in the peritoneum requires more care to develop its fluctuation. It is most distinctly felt in the iliac regions when the patient is in the sitting or standing posture, which not only accumulates the fluid in greater quantity in these parts, but also thereby gives the requisite tension to the walls. If the liquid is in still smaller quantity, it may be insufficient to reach from one side of the abdomen to the other; and then it must be tried for in a more limited space, such as the hypogastric or in the one iliac region in the sitting posture. A delicate method of detecting small quantities of fluid by fluctuation, is by keeping the patient for some minutes lying on one side, then after he has turned nearly but not quite on his back, feeling for the fluctuation with the fingers lightly applied to the iliac region of the undermost side, whilst the other hand gently taps the flank or the hypogastric region; or the position of the feeling and striking hands may be reversed. By this

expedient the fluid accumulates in contact with the walls, where they are thin, and renders evident the gentlest fluctuation.

In feeling for such delicate and superficial fluctuation it is important that the fingers should be very lightly applied; not pressed into the walls; otherwise the most sensitive parts of the fingers will be out of the reach of the wave. The soft and weighty feel of the liquid, as before described, may be taken in confirmation of the sign of fluctuation. The sensation of undulation itself, in fact, comprises more or less of the same impression of something heavy, soft and in motion. It is important not to mistake for fluctuation an undulatory movement excited by percussion in the soft fat of the walls. This may be distinguished by its more uniform diffusion over the surface of the abdomen; by its not occurring more in dependent parts than in others; by its less liquid and weighty impulse against the fingers; and by its occurring chiefly in fat subjects, in whom the flabby adipose texture can be pinched up between the finger and thumb in such quantity as to prove that the walls are too thick to transmit any very superficial feeling of fluctuation from liquid in the abdomen. In fact these cases of flabby fat abdomen are those in which small quantities of liquid are less readily detected by fluctuation, than by the comparatively greater weight and duller stroke-sound of the most dependent parts.

The discovery of a small quantity of liquid is of great importance in some cases; as for example, in distinguishing peritonitis from colic or neuralgic abdominal pain, and from tympanitis, with which it is sometimes combined: and in detecting the first commencement of ascites, when remedies are more likely to be effectual than in the advanced stages. We are accustomed to watch for the signs of fluid effusions as indications of analogous conditions in the pleura; and by careful attention to the tests of palpation and percussion, these signs are not less available in the peritoneal cavity. It is true that we have not the striking contrast of the hollow-sounding lung, and the variations of voice and breath-sounds, to aid in their detection; but, on the other hand, we have the combined tests of percussion and palpation, and above all, we have in changes of posture of the patient the means of concentrating the liquid in a dependent part, and of transferring it to another, in a degree unattainable with pleural effusion.

An obscure fluctuation may sometimes be elicited in the liquid contained in the portions of intestine, in the urinary bladder, and even in the gall-bladder, and in cysts connected with the ovarium, the liver, the kidney, and other viscera; and if the quantity of fluid be considerable, it will manifest some-

what of its property of weight and soft resistance. But liquid thus contained may be distinguished from that free in the peritoneum, by its more obscure and less superficial fluctuation, and especially in its circumscribed position, which may not be the most dependent, and which is not materially changed with the position of the patient. It is quite possible to have combined, and to distinguish the two kinds of fluctuation, the peritoneal, superficial, or gravitative, and the visceral, deep-seated, or circumscribed. The detection of the superficial fluid is even easier than usual, for the sac or cyst underneath raises the liquid, and makes it to spread more or less over its surface; which renders the fluctuation more obvious, and affords the additional criterion of the striking by displacement of the superficial liquid, as before described. The deep fluctuation is to be felt by first displacing the surface liquid by pressure, and with the hands thus deeply applied, succussion is attempted, and the contents of the cyst further felt. Change to the prone or leaning forward posture will commonly help in this investigation; as the cyst is often heavier, and, displacing the superficial fluid, comes in contact with the wall sufficiently to be manipulated. When the peritoneal fluid is scanty, the sac is more widely in contact with walls, and may not be separable by any change of posture. In this case it may be of importance, in reference to an operation, to determine whether the sac adheres to the peritoneal walls; and in one such instance, a case of ovarian cyst, the author was enabled to decide this point by the following expedient. The funnel end of a stethoscope being applied to the abdomen where the tumor was in contact with the walls, suction was used at the other end so as to make the walls adhere (by atmospheric pressure) to the instrument; it then became a means of separating the walls from the sac, the peritoneal fluid occupying the space; the presence of which fluid was determined by palpation around the part adhering to the stethoscope, and thus the non-existence of adhesions was proved. This patient was subsequently tapped; and when the fluid again accumulated, the surfaces could no longer be separated, adhesion having taken around the puncture.—[*London Jour. of Med.* *Ranking's Abstract.*

Researches upon Foreign Bodies in the Air-passages. By M.
LE DOCTEUR JOBERT DE LAMBALLE.

Under the above title, a series of interesting observations, illustrated by cases, has just been published in the *Union Médicale*, and from them the author draws the following practical conclusions:

1. That foreign bodies have a greater tendency to lodge in the right than in the left lung, on account of the direction and greater size of the right bronchus.

2. That they penetrate into the air-passages during the greatest possible divergence of the chordæ vocales, while a strong column of air is rushing into the trachea, as occurs during a rapid succession of inspirations and expirations, for example, in a fit of laughter.

3. That they pass through the superior opening of the larynx, without raising the epiglottis, which is never depressed on the opening, as has been stated.

4. That the epiglottis is always raised in virtue of its own elasticity.

5. That the latter organ appears to act principally as a kind of groove, in guiding certain liquids and solids, during the complicated act of deglutition.

6. That foreign bodies rapidly traverse the air-passages in obedience to the laws of gravity, and in consequence of the impulse of the column of air.

7. That they are only momentarily arrested in a given point in the air-tube; that they may, in consequence, move about and change their position, until they have induced an inflammatory action, owing to which they become lodged in a certain part.

8. When their dimensions are unequal, they are arrested in their oblique direction, at a division or subdivision of the bronchi, and they assume the direction of the tube when they fill a normal opening.

9. That they more or less impede respiration and oxygenation; that they cause cough, often intermittent, sometimes continued, and that they give rise to pain and a fixed sensation which indicate their seat.

10. That their presence gives rise to a peculiar *bruit*.

11. That the bronchial secretion is always augmented by their presence, being mucous and sometimes sanguineous.

12. That the respiration becomes puerile in the lung of the other side.

13. That foreign bodies may give rise to a slow or rapid asphyxia, to suppuration, emphysema, &c.

14. That foreign bodies which measure more than four lines in all directions do not allow any hope of their expulsion by the unaided efforts of nature, since their dimensions exceed the shortest diameter of the glottis.

15. That they have been spontaneously expelled from the human trachea only when they were small.

16. That in dogs, on the contrary, in whom the glottis is in the same plane as the upper opening of the larynx, the expul-

sion of foreign bodies is accomplished with facility, in consequence of the dilatibility of this opening, and of its dimensions, which are considerable in all directions.

17. That, in the dead subject, foreign bodies clear the glottis with difficulty; even when blown upon with a bellows which furnishes a considerable column of air.

18. That in the living subject, foreign bodies have not only to overcome this passive resistance, but also the very active opposing force of the constricting muscles of the glottis.

19. That we can, consequently, reckon on the expulsion of only very small foreign bodies in man, and that when they have a certain volume we can expect nothing from the efforts of nature.

20. That the operation of tracheotomy becomes indispensable in nearly all cases of the introduction of foreign bodies into the air-tubes, and that it is only exceptionally that it can be dispensed with.

21. That the operation should be performed as soon as possible, in order to avoid inflammation, local bad consequences, and immediate or chronic asphyxia.

22. That the opening of the air-passages is a delicate operation, which ought to be performed by successively dividing all the tissues, and not by an incision including at once a great part or the entire of the soft parts of the region; in this way we avoid hemorrhage, the admission of air into the veins, injury of the thyroid gland, &c.

23. That the air-tube ought to be opened as freely as possible, so that the foreign bodies may escape easily.

24. That we can be certain of the division of the trachea only when the air escapes from it, producing a peculiar noise easily recognised by those accustomed to this kind of operation. The author purposely lays particular stress on this phenomenon, to which Dupuytren did not attach sufficient importance, since according to the statement of MM. Marx and Brierre du Boismont, that able surgeon had, in some instances, penetrated only into the kind of hollow which is situated above the sternum, although he thought he had entered the air-tube.

25. That when the foreign body does not escape by the opening at the moment of the operation, it is well, before proceeding further, to stimulate the sensibility of the trachea by the introduction of a blunt instrument, so as to excite cough, and cause efforts at expulsion.

26. That the trachea ought to be opened more freely than under other circumstances, when a body capable of swelling with moisture has been for some time enclosed in it.

27. That reunion of the wound after the operation may be obtained by the first or second intention.

28. That when reunion by the second intention is sought for, it is accomplished by granulation, which requires a long time to effect a complete cure.

29. That reunion by the first intention may be obtained by simple compression or the interrupted suture. This latter mode appears to the author much more favourable to immediate reunion, as experiments on animals have shown him its possibility.

30. That immediate reunion may be obtained by the interrupted suture taking in only the *dartoid lamina* which surrounds the trachea.

31. That adhesion may be obtained by another mode, which consists in including partly or in whole the walls of the trachea, letting the thread hang externally.

32. That the threads fall out between the fourth and the thirteenth day.

33. That the plastic secretion serves as a means of union between the lips of the wound.

34. That cicatrization is accomplished only by means of an intermediate product, and not by the direct adhesion of the lips of the trachea.

35. That a suture which includes the entire thickness of the walls of the trachea exposes this tube to an internal and external inflammatory action, to the risk of deposits of organized bands of lymph, and to encysted abscesses.

36. That a suture which includes only the investing structures and a part of the substance of the trachea, gives rise to a plastic inflammation, and is to be preferred to that which includes the cartilaginous parietes of the tube.—*L'Union Médicale. Dublin Quarterly Journal.*

Evidence of Puerperal Fever depending upon the Contagious Inoculation of Morbid Matter. By Dr. F. H. ARNETH, of Vienna. (*Ed. Med. Chir. Society.*)

There is, perhaps, no subject in medicine that has excited so much of the attention both of the medical profession and of the public at large, as the epidemic puerperal fever, sometimes prevailing to a fearful degree in lying-in hospitals. So strongly, indeed, has the apprehension of the public been excited, that the entire closure of lying-in hospitals has been repeatedly urged, even by professional men and some of them have really been shut up for a certain time.

Whilst I am fully convinced that the facts I shall have the honor to communicate to the society, illustrative of one of the

most frequent causes of so dreadful a disease, and of a means preventive of its spreading so fearfully, will prove interesting, I have serious doubts as to my being competent to do justice to the subject in a language with which I am but imperfectly acquainted. This difficulty becomes still greater, as the outlines of what I have to say to-night are well known in this country, through a very able paper of Dr. Routh's.

Having, however, been connected with the Vienna Lying-in Hospital, and thus been enabled to watch the disease, and the manner of its being spread and limited, I hold it a duty to give the subsequent observations all possible publicity.

Before entering on the subject in question, I must apologize for alluding somewhat to the history and the management of the Vienna Lying-in Hospital.

The lying-in hospital was opened, together with the "General Hospital," of which it forms a part, in 1784. Its wards being always excessively crowded, the government, by which it is supported, gave orders to build some new wards, and to use them as a second obstetric clinic. Thus, in 1833, the first midwifery clinic took possession of the new wards, leaving the old buildings to the more recently established clinic. I think it proper to observe, that these two clinics form only one large building, their wards being in fact, so close together, that they are merely separated by a wooden door.

The other arrangements are entirely similar in both establishments. The linen of both is washed together. The two clinics admit pregnant females on alternate days.

In 1839, however, from different reasons not connected with our subject, it was thought advisable that the new wards should be exclusively devoted for the instruction of the male pupils; whilst, in the other establishment, the female pupils should be trained. As, of late, a far greater mortality had prevailed in the lying-in hospital than is generally to be met with in private practice, councils of leading men of the profession were held at different times, with a view to ascertain the cause of this fearful amount of mortality.

These gentlemen were struck with the fact that the amount of mortality was, without any exception, far greater in the wards where the medical students pursued their studies, than in those where the midwives were taught. But this was the case only since the year 1839; that is to say, from the time that the medical students and the midwives occupied different wards; the mortality having been nearly equal in former years, when each clinic included among its pupils both medical students and midwives.

This will be exactly shown by the following table :

<i>Died in the whole lying-in hospital.</i>				<i>In the second clinic alone.</i>			
In 1834,	8.06	per cent.	-	-	8.	per cent.	
" 1835,	5.33	"	-	-	4.98	"	
" 1836,	7.61	"	-	-	7.8	"	
" 1837,	8.28	"	-	-	6.9	"	
" 1838,	3.75	"	-	-	4.94	"	

Whereas, since the separation of the pupils took place, there died in the

<i>Clinic of the medical students.</i>				<i>Clinic of midwives.</i>			
In 1839,	5.4	per cent.	-	-	4.5	per cent.	
" 1840,	9.5	"	-	-	2.6	"	
" 1841,	7.7	"	-	-	3.5	"	
" 1842,	15.8	"	-	-	7.5	"	
" 1843,	8.9	"	-	-	5.9	"	
" 1844,	8.2	"	-	-	2.3	"	
" 1845,	6.8	"	-	-	2.03	"	
" 1846,	11.4	"	-	-	2.7	"	

The commission generally adverted to the linen being washed without sufficient care, and to the unfavorable situation of the wards, as explanatory of the great amount of mortality; but did not suggest any explanation of the great difference in the relative number of cases ending fatally in the two clinics. At length, in November, 1846, it was decided to diminish very considerably the number of young medical men attending the lectures of midwifery, as the opinion prevailed, that the too frequent examinations of the patients, by great numbers of students eager to make the best of their time while in the lying-in hospital, had brought on epidemics of puerperal fever. And, in fact, in the subsequent months of December, January, February, and March, the mortality was much less; but reappeared, unfortunately, in April, when, in this clinic, 37 died out of somewhat more than 300; and in May, when 36 cases, out of about the same number of confinements, ended fatally. This failure proved, better than the best concocted theory, that the real cause of the fact occupying our attention had not then been discovered.

In March, 1847, Dr. Semmelweiss was nominated assistant to the professor of midwifery, in whose wards this frightful mortality had prevailed. From the first moment he was occupied in finding out the cause of the epidemic which it became his lot watch. As "time," the best judge of opinions, had so loudly spoken against the views that had hitherto prevailed, it was certainly out of the question to inquire into *them* a second time. His attention was very soon drawn to the management, both as to dietetics and medical treatment, in the

neighboring clinic, which had to boast of results comparatively so favorable. But, to his amazement, he found that there neither was the ventilation better carried on than in his own wards, nor had any particular plan of treatment been resorted to. At length, however, he was struck with what was very likely the sole difference to be found in the management of the two clinics, namely, the pupils of the one being midwives, those of the other medical students. The latter were, almost without exception, in the daily practice of assisting at autopsies, of which eight or ten took place in our large hospital almost every day. The dissections were sometimes executed by the students, or they handled at least the pathological preparations, and examined them carefully. Moreover, the assistant used to lecture on the obstetrical operations. These were performed on dead bodies, and, of course, sometimes repeatedly. Now, after such investigations and such practice, it was not rare to see the students going immediately to the wards of the lying-in hospital, and examining the pregnant and parturient women.

It is scarcely necessary to state, that the pupils of the other clinic, being midwives, did not take any share in the occupations just alluded to; nay, even the assistant of that clinic had comparatively seldom to deal with post-mortem examinations, as it was not a part of his duty to give instruction to the midwives in pathology or operative midwifery.

Having convinced himself that the great prevalence of the disease in his wards was caused by the inoculation of the female genitals, Dr. Semmelweiss began to entertain a hope of being able to diminish this frightful mortality.

Every one who has been engaged in post-mortem examinations must be aware of the very disagreeable smell, which it is impossible to get rid of, even by the most careful washing. Of course this smell which remains about the hands is more penetrating and more lasting the more dissections have been made at a time.

The existence of any smell can only be dependent on a substance detained on the epidermis, and is still sensible as long as there remains the least atom of that substance. It is impossible not to recollect how actively the process of absorption, generally speaking, is carried on in the pregnant female.

The opinion of Dr. Semmelweiss on this important matter is as follows: *Any fluid matter in a state of putrefaction**—communicated by linen, by a catheter, by a sponge, by small particles of the placenta, even by the ambient atmosphere impregnated with the foul substances—*may produce puerperal fever.*

* I think every morbid matter undergoes a degree of putrefaction, after it has been discharged from the body for any length of time.

If these ideas are admitted, it appears very likely that washing the hands with some powerfully cleansing fluid must destroy the smell, and remove the morbid particles from the hand.

After having tried various substances, he was laterly in the habit of prescribing for that purpose a solution of chloride of lime as the cheapest.

In the course of the month of May, 1847, it was arranged that no one should examine any woman in the clinic without previously having washed his hands with solution of chloride of lime, and having made use of a nail brush.

Even in June, it was impossible not to be sensible of the influence of this precaution. Out of more than 300 women that were confined in that month, only six died; in July, three out of about the same number; in August, three; in September, twelve; October, eleven; November, eleven; December eight; whereas, in April, fifty-seven; and in May thirty-six, cases had ended fatally. In the year 1848, the mortality amongst the puerperal women delivered by male pupils was 1 in 84; while, in the second clinic, amongst the women delivered by midwives, it was 1 in 76.

Since the year 1827, the rate of mortality in the hospital had never been so low. I need not say more than that from the time when this precaution was introduced into the practice, up to the time of my leaving Vienna in November last—that is to say, for more than three years—the mortality in the two clinics has always been nearly the same, which had never been the case since 1839—that is to say, since the time when the male and female pupils were exclusively confined to separate clinics.

A very similar observation was made at Kiel. Dr. Michaelis, professor of midwifery in the university of that town, wrote a letter to Dr. Semmelweiss, bearing the date March 18th, 1848, in which he informed Dr. Semmelweiss that they had been obliged, in 1847, (July 1,) to shut the lying-in hospital because of the puerperal fever spreading fearfully. In November they opened the doors again, but a very short time afterwards the disease began again to make its appearance—so that they had already come to a determination to close the house for a second time, when the news of Dr. Semmelweiss's opinions, and the measures adopted in our lying-in hospital, reached Kiel, (December 21st, 1847.) From that time, up to the 18th of March, they had no case of puerperal fever, excepting one, that was caused, as Dr. Michaelis believes, by a dirty catheter.

I must call your attention to some more facts which, as it appears, speak strongly in favor of the opinions of Dr. Semmelweiss.

In the course of those years when the post-mortem exam-

inations were less frequent with us in Vienna, owing to the little importance attached to pathological anatomy, the puerperal fever was comparatively seldom met with; and this was certainly not explicable by the wards being less crowded than at present, as the one clinic of those former times contained—in comparison with its room—nearly as many women as the *two* clinics do now. You may recollect that I have stated before that when first the second clinic was established, its pupils included both midwives and medical students, and that in these days the mortality in its wards was not less than in the other clinic. It was only after the separation of pupils already mentioned, that the reports of the second clinic became much more favorable than those of the first clinic. They remained constantly so; with one striking exception, however, occupying the period between 15th October, 1841, and 15th October, 1843, when the mortality was much higher than usual, the then assistant being very busy in anatomical pursuits.

According to the reports of the lying-in houses in the whole empire of Austria, in none of those institutions where midwives have been the only pupils has a puerperal epidemic made its appearance; but it reigned obstinately in Pavia, where they used to dissect—in one of the rooms of the lying-in hospital—the bodies of the children who died in the hospital.

There existed, within the last few years, two clinics in Strasburg, as closely adjacent as those in Vienna, and divided on the same plan in reference to the pupils. The professors of Strasburgh told me that they had always less mortality in the clinic to which the midwives were admitted, and that there was no instance of any epidemic in those wards.

While in search of the true cause of the prevalence of puerperal fever, the impression on Dr. Semmelweiss was unfortunately at first exclusively directed to the influence of postmortem examinations, and the precautions that were already in practice were not resorted to in the case of a pregnant woman who was admitted into the lying-in hospital laboring under cancer of the uterus. As several days elapsed before her confinement was over, and as the case was highly interesting, of course every one wished to examine her. The consequence of this was most deplorable. Fourteen mothers who had been confined at the same time with this woman, and who had been examined by the same students, exhibited puerperal symptoms, and three of them died, although puerperal fever had not been prevailing immediately before, nor did any other case occur except these fourteen.

I think there can scarcely be any doubt as to the mode in which the matters of putrefaction act on the system. All the

symptoms that are so often observed in puerperal fever, and the more so the greater the number of patients at a time is, show clearly that it is phlebitis, ending in purulent infection, with which the women are seized. The rigors, the shivering, that prevail as the first symptoms, the dirty yellow discoloration of the skin, the metastatic deposits that are collected in the cellular tissue and in the articulations, are ascribed by most of the very eminent writers on the subject to the purulent infection; and every accoucheur is aware that those are also the symptoms of the most hopeless cases of epidemic puerperal fever. But allow me to observe that, as far at least as I am aware, purulent infection is never met with except in cases where the purulent matter comes in immediate contact with the blood, as is observed in punctures inflicted in dissections, that present quite similar symptoms, and end unfortunately too often in a similar way.

If the discharges of the uterus and the vagina were the true causes of puerperal fever in most cases, it would be decidedly very difficult to say why they did not infect the women in both clinics of the Vienna hospital with equal frequency.

Before concluding, I have only to express a hope that most of the great variations of mortality may be avoided in future by the greatest care after dissections, and by attention to cleanliness, the air of the wards, the instruments, linen, &c.

It is, however, my firm conviction that any individual who may not choose to submit to what we have stated to be necessary after the contact with matters in putrefaction, may artificially produce the puerperal fever on many persons whom he may successively examine, and who, in all probability, would never have become ill but for this inhuman carelessness.

Allow me, in conclusion, to state, that I know of no case of the puerperal fever having been communicated through the clothes of a medical man to a patient. As far as I know, there is no such case on record, neither in hospital nor in private practice in Vienna, where it had been possible to trace a puerperal fever to such a source. As to erysipelas, which I am aware is very often admitted in this country to produce puerperal fever, I feel myself bound to aver that erysipelas has by no means been most frequently observed in the Vienna hospital when we had epidemics of puerperal fever, nor did we find the children of the puerperal women seized with erysipelas. Scarcely one of our pupils, and none of the nurses, were taken with erysipelas during my stay in the Vienna hospital.—[*West-ern Lancet*.

Unconsolidated Fracture of the Thigh successfully treated by Acupuncture. By M. LENOIR.

The rationale of the various plans of treatment which have been adopted, in order to prevent the formation of false joints, consists in the establishment of an inflammatory action in the fibrous tissue situated between the bony fragments, and the consequent secretion of a secondary callus. One of the methods proposed has, in the hands of its inventor, M. Malgaigne, been unattended with success: we mean acupuncture. But the following case, communicated to the Société de Chirurgie by M. Lenoir, proves that this mode of treatment deserves some notice, even although it has not afforded similar results to M. Maisonneuve. Much of the success obtained by M. Lenoir must, doubtless, be attributed to the many precautions observed by him.

Dupéché, a carpenter by trade, aged thirty-three years, in falling from a height of fifty-two feet, fractured his right thigh. He was immediately conveyed to La Pitié, and placed under the care of M. A. Bérard. After fifty-four days of treatment the patient began to walk with the assistance of crutches, when M. A. Bérard, in order to remove a stiffness which existed in the knee-joint, endeavoured by force to extend the motions of this articulation; in one of these manœuvres the neck of the femur gave way, and the signs of fracture re-appeared. The broken bone was again reduced, and an immoveable apparatus applied to keep the fractured ends *in situ*; at the termination of a month the apparatus was removed, but the fracture had not consolidated, and the patient had himself conveyed home.

Six months afterwards M. Lenoir took him into hospital, for the purpose of employing the treatment by acupuncture; but before trying this plan he used all the means likely to insure success, and, amongst others, he had him placed on a mechanical bed, so as to maintain complete freedom from motion, even in attending to the calls of nature. As the fracture was oblique and the upper fragment very sharply bevelled, and the fragments, by overlapping, occasioned a shortening of about two and a half inches, M. Lenoir had an apparatus for maintaining extension constructed by a carpenter, a friend of the patient. This apparatus consisted of a sort of long box, nearly in the shape of the limb, and consequently wider above than below, but longer than it; it was about three inches deep, and was composed of three pieces of light wood closely united to one another; of these three splints the external was eight inches longer than the others, which terminated at the junction of the thigh with the trunk; this longer portion had at its upper end

a mortise intended to facilitate the employment of counter-extension; to the lower end of this groove a kind of toothed wheel and axle was adapted, to which was applied a catch for the purpose of fixing it. This apparatus, lined with carded cotton, received the limb, the foot being covered with a gaiter of ticken furnished with a foot-strap; by means of this strap rolled round the wheel extension was made, while counter-extension was maintained by another strap well padded, passing along the fold of the groin, having the ischium as its *point d'appui*, and its ends fixed in the mortise in the outer splint of wood.

For several days nothing was done except to tighten the straps according as they became relaxed. At last, on the 12th August, seven months and some days after the accident, M. Lenoir proceeded to insert the needles. At first he introduced four, each being four inches long, and furnished with a head. Their points were directed along the inner surface of the upper fragment, from below upwards; an interval of but half an inch being left between each needle. Contrary to his expectation, and although he passed them in as far as the heads, he met no obstacle to their introduction. This, doubtless, depended on the existence of an interval between the two fragments, the extension effected by the apparatus having reduced the fracture only in the direction of the length of the limb, and not transversely. The four needles remained *in situ* for six days; at first they excited redness of the skin, then a little pus appeared about them, and rendered them moveable, and finally a slight swelling and pain in the limb occurred. These symptoms indicating that inflammation had developed itself, M. Lenoir withdrew the four needles; and, after having cleaned them, he re-introduced them higher up, following carefully the direction of the upper fragment, and leaving between them the same intervals as before. The same symptoms followed this second operation; at the end of five days the needles had become moveable, and were taken away; and the inflammatory action now appearing to be sufficient to produce union, the introduction of the needles was not repeated. The inflammatory swelling of the limb was treated by poultices, antiphlogistic diet, and cooling drinks; and when it was subdued, the two surfaces of the fragments were brought into closer proximity by means of small splints placed around the thigh, and tightened by two straps of leather, a practice previously employed by Amesbury. The apparatus was inspected daily, and tightened when necessary. At the end of twenty-three days, in order to ascertain how far consolidation had advanced, the limb was completely uncovered; it was found to have neither got out of shape nor undergone retraction; but when the hand was

passed over the seat of the fracture, it still yielded; splints were immediately re-applied, the limb was replaced in its groove, and extension continued. No fresh examination was made until the expiration of thirty-five days from this time, and then the callus was found to be sufficiently solid to justify the removal of the entire apparatus. Carefully measured, the limb was now found to be rather less than eight-tenths of an inch shorter than that of the opposite side; the knee-joint was stiff, but the patella was still capable of some transverse motion; the thigh and the upper part of the leg were œdematous, but otherwise there was no apparent deformity at the seat of the fracture, and the callus was not very bulky. Lastly, the coxo-femoral articulation was capable of motion, and the patient was able to raise the limb by the unaided action of the muscles. As an additional security, he was advised to keep his bed, and, during a fortnight that he was confined to it, the œdematous swelling of the limb was treated by fomentations of aromatic wine, and by bandaging. At the end of that time he got up and walked at first with the aid of crutches, and afterwards of a single stick; finally he left the hospital cured, and M. Lenoir subsequently ascertained that on his return to his native district (Auvergne) he had, during the entire of the following autumn, driven a plough, and that he now experiences no difficulty in the pursuit of field labour.—[*Bulletin Général de Thérapeutique.* *Dublin Quar. Jour.*

Scutellaria Laterifolia as a Nervine. By C. H. CLEAVE-
LAND, M. D., of Waterbury, Vermont.

One of the most valuable *nervines* that have been discovered for our use, is to be found by the side of many of our streamlets, and in low marshy places in nearly every part of this state, and in sufficient quantities to supply the entire profession from Maine to Texas, should they but be convinced that with us grow plants possessing medicinal properties as useful and as potent as are obtained from distant climes.

Such, I think, must be the belief of all who will make a trial of the *Scutellaria Laterifolia*, in the place of the English or German Valerian (*valeriana officinalis*), that has been the main article in use in this region in all *nervous diseases* since the day when *Assafœtida* went out of fashion.

The *Scullcap* has not only a most remarkable power of controlling the nervous excitability, as manifested in patients of an irritable temperament when fatigued, over-excited, or suffering from slight physical derangement, but its most valuable properties are displayed in those severe and painful cases

where we are led to use our most potent and active remedial means. In *Delirium-tremens*, *Tic-douloureux*, *Convulsions* from irritation of the ganglionic nerves or spinal cord, in *Chorea Sanctiviti*, *dental irritation* among children, as well as in the ordinary diseases of the nerves, where a soothing quieting medicine is indicated, I have been led to prefer the *Scutellaria* above all other nervines or antispasmodics, except in those cases where an immediate effect is desirable. In such cases, of course, we should resort to chloroform, ether, musk, castor, and the other drugs of the same class.

Among my reasons for this preference, I would mention the tonic property of the herb, which gives strength as well as quiet; its sudorific and its diuretic powers, both tending to relieve the congestion that is usually present, which tends to perpetuate the disease. It never leaves that excitable irritable condition of the system when its soothing influence has worn away, that follows some of the other nervines; and it is so readily administered that but little delay need occur, and no evil results be anticipated.

I am led to call the attention of the profession to this plant in the earnest and decided manner I have used, mainly because of the high estimate I place upon it; but in part from the disparaging remarks of the *United States Dispensatory*. I would not wish to detract from the fair and just fame of the compilers of that great work, or lessen the confidence that is so properly placed in it, but I think I know more of this plant than those authors could know, and write according to my own observations. Without doubt, the plant has been extolled too highly by some, and also recommended in cases where it has failed of answering the expectations of those who relied upon it, but not, I think, when used in such diseases as are indicated above. To me it has never seemed inert and powerless, and having had occasion to make personal use of it when the whole system was suffering severely from being poisoned by decomposing animal matter, I feel qualified to testify that "it does produce an obvious effect."

I have used it in the form of a saturated *tincture*, a *syrup*, and a *cold* and *warm infusion*; and I prefer the infusions to the other preparations when they can be conveniently prepared, the cold when it is desirable to obtain the tonic, and the warm when the sudorific effect is demanded. Half an ounce of the dried leaves to a tea-cup full of water will be very strong, and it may be drank *ad libitum*.

Of its curative power in cases of *hydrophobia* I can say nothing, never having had an opportunity to give it a trial; but should such a case present itself, I should make a trial of the plant in conjunction with other means.—[*N. Y. Register*.

Miscellany.

Effects of various Therapeutical Agents on the Animal Heat. By MM. AUG. DUMERIL, DEMARQUAY and LECOINTE.—The authors, in their researches, propose to examine the effects on the animal heat of various agents, introduced into the economy in successively increased doses. The experiments were made in the laboratory of M. Flourens, at the Jardin des Plantes. The animals experimented on were full-grown dogs. As far as possible, they were kept fasting from the evening before till the evening of the day on which the experiment was made. The duration of the experiments—or rather the time during which it was thought requisite to keep the animals under observation—varied from six to twelve hours. The medicinal substances were introduced in three ways,—sometimes, and most frequently, by the stomach; sometimes by the veins; and sometimes by the cellular tissue. Before any experiment was made, it was ascertained that the ligature of the œsophagus, intended to prevent vomiting, did not produce any effect on the temperature of the animal during a number of hours equal to the duration of the experiment with the medicines.

The temperatures were taken with accurate centigrade thermometers, capable of being substituted for each other in a case of accident. All the temperatures were taken by the rectum, the thermometer being introduced to the same depth, and being left in the gut, until it remained at a fixed point for five or six minutes.

By taking these precautions, and by repeating in some instances the experiments as often as ten or eleven times with the same substance, the authors have been able to assure themselves that the action of any given remedy is always the same as regards the animal heat, whether administered by the stomach, the veins, or the cellular tissue.

All the substances employed were prepared by M. Mialhe. They used sometimes powder or extracts, sometimes alkaloids or salts. The latter preparations were the only ones ever injected into the veins. The substances were dissolved or suspended in water, oil, or mucilage; the quantity of vehicle varying from one to four ounces, according as it was to be introduced into the veins of the stomach.

The temperature of the atmosphere in which the animals were placed was from 53° to 59° Fahr., and the temperature of the liquids containing the drugs was from 68° to 95° Fahr.

The results now published are confined to those which bear upon the animal heat. To give a systematic form to their series of experiments, they have adopted Trousseau and Pidoux's classification; they have omitted those remedies of each class which are not sufficiently energetic to produce some decided effect on the animal heat.

EXCITANTS.—This class of remedies has been examined carefully; only the more powerful have been experimented with. These are cantharides, ergot, phosphorus, and strychnia. As a general result, it may be stated, that all these agents have produced an elevation of temperature varying from a few tenths of a degree to several degrees.

Cantharides.—This drug was experimented with four times, and given in doses of $1\frac{1}{4}$ grains to 3 grains and 6 grains. At the dose of $1\frac{1}{4}$ grains the thermometer rose $2^{\circ} \cdot 1$ (centig.) in a period of six hours, during which the temperature was noted at intervals of two hours. In three other experiments made in the same manner, and with equal care, in which the doses were on two occasions 3 grains and on one occasion 4 grains, there was again an augmentation of temperature which with the 3 grains approached to 2° , but with the 6 grains did not exceed 1° . It will be seen in the course of the experiments that cantharides is not the only substance which gives different, sometimes even opposite, results with varied doses.

Ergot.—This substance, whose special actions on the uterus are so well known, was given only once in the dose of a drachm; and in a period of five hours, during which the temperature was observed several times, there was ascertained to be a rise of $0^{\circ} \cdot 8$.

Sulphate of Quinine.—It was introduced twice into the stomach in doses of 15 and 30 grains. In these two experiments the final results were increase of temperature, which varied from $1^{\circ} \cdot 5$ to $2^{\circ} \cdot 2$. But a remarkable phenomenon was, that, at the beginning of the experiment, the temperature fell during the first two hours some tenths of a degree.

Phosphorus.—Six experiments were made. In doses of three tenths of a grain, and of three fourths of a grain, there was a constant and successive augmentation,— $1^{\circ} \cdot 7$ in the first, and of $2^{\circ} \cdot 2$ in the second—whilst in the four last dogs to which the drug was given in doses of $1\frac{1}{2}$ grains, $2\frac{1}{4}$ grains, and 3 grains, there was a constant lowering of the temperature, though to an inconsiderable amount; for it never was to a greater extent than $0^{\circ} \cdot 2$ except in the case of one animal, which died in fifty minutes. Thus phosphorus in small doses raises the animal temperature very manifestly, whilst in large doses it slightly lowers it.

Sulphate of Strychnia.—Four experiments were made with this substance. Twice it was introduced into the stomach, and twice into the veins. Only slight results were obtained; but the temperature was slightly elevated. The animals, however, always died rapidly under the influence of this agent.—[*Gazette des Hopitaux*. *Western Lancet*.

On the treatment of Small Pox. By JOSEPH GROSVENOR PASQUIN, M.R.C.L.S., Birmingham.—Having for many years watched the unsuccessful treatment of small-pox, and also the fatal termination of that disease, even when placed in the hands of the most skilful surgeons and physicians, I was therefore induced to give to it my most devoted and undivided attention, supposing, as I did, that some valuable addition might be made in the treatment thereof. After mature deliberation thereon, it was my opinion that the pitting and consequent disfigurement of the face after that disorder, was dependent upon the confinement of the matter in the pocks for too great a length of time, which thereby would cause a slough to form in the cellular tissue

lying between the cuticle and fascia of the face, which, being so thin, is never more regenerated, thereby causing the cuticle to fall into the space where the cellular tissue is then wanting, and thus follows the pitting.

Secondly I was of opinion, that by puncturing each pock previously to its coming to perfection, and then treating it with poultice, as a common abscess, I should not only avoid the pitting but also draw out of the system that putrid matter which, had the pock been left to ripen, would not only have caused the slough and pitting, but would have been absorbed into the body, and produced most injurious results to the system in general.

Thirdly. In numerous instances, I have seen patients die from the eruption breaking out, not only on the tongue, fauces and pharynx, but also on the most delicate part, the larynx. This I also thought might be obviated by placing a few leeches over the external region of the larynx, supposing it would, by diminishing the circulation in that region, reduce the size of the pocks, and also give play to the thyro-arytænoid muscle, and thereby prevent suffocation.

Having formed these opinions, I felt determined to try them in the next case of small-pox that came under my notice, which I have now done, and beg leave to report to you the result of my experience:—I have had seven cases, four wherein the larynx was not at all affected, on which I tried the experiment of puncturing every pock on the face, and afterwards applying repeated poultices. This treatment succeeded to my utmost satisfaction, the face being left as clear of marks as it was previously to the attack of small-pox. I had three more with affection of the larynx, the respiration being so difficult that I expected asphyxia would come on in a few hours. To these I applied leeches over the region of the larynx, and on the following morning, I found the respiration had become perfectly free and easy.

One of the three cases last reported, is that of D—, a laborer. This was the worst case of confluent small-pox I ever witnessed in the whole course of my medical career. He was, in the early stage of the disease, attacked with great difficulty in breathing; his tongue, soft palate, pharynx, and larynx, as far as I could see, being covered with pocks. I applied leeches at night, and on the following morning his breathing was perfectly free and easy. His face was so completely covered with pocks, that I could not find one space over his whole face, sufficient to lay on a grain of sand, which was uncovered by any pock. In this case I punctured as many pocks as I could myself, and requested his mother and sister to puncture the remainder. He is now up and doing well, and he has not a mark upon his face; but upon this case I will report to you hereafter. In this case the feet were very painful previously to the appearance of the pocks. I applied poultice.—[*London Lancet*.

Frictions with Sulphuric Ether in Tetanus.—We find a case of Tetanus related in the *Bul. Gén. de Thérap.*, 15th June, 1851, taken from an Italian Journal, in which frictions with sulph. ether produced

very remarkable effects. The physician, M. Tibaldi, was called to a man who had imprudently slept upon the moist ground, after getting into a profuse perspiration from exercise. M. Tibaldi, finding his bowels constipated, administered a purgative, and the next day he was much better, but during the day he was taken with tetanic convulsions. On the 17th, when M. Tibaldi saw the patient, tetanus had developed itself. There was complete immobility of the whole body, except the arms; the eyes were fixed, but brilliant; there were spasmodic contractions of the muscles of the trunk and thighs, and especially the muscles of the neck and back, with constriction in the region of the diaphragm and throat. The pulse was 85, small and hard, and there was an abundant perspiration. The patient was bled twice during the day. The 18th, no better; was re-bled, and a dose of tart. emetic administered. This rendered him more calm, and he had several operations upon the bowels. The evening of the same day, he was bled again and eight leeches were applied along the sterno-cleido-mastoid muscle, and mercurial frictions made upon the back. Considerable amelioration upon the 19th; another bleeding and a blister to the region of the diaphragm. In the evening the patient was bled again. Upon the 20th, the accidents returned: the pulse was 100, high fever and violent tetanic contractions. Another bleeding was practiced, a dose of tart. emetic was administered, and leeches applied to the anus. In the evening he was again bled, but did not improve much. Later in the evening the patient got worse, and M. Tibaldi attempted to relieve the pains and induce muscular relaxation, by frictions with sulph. ether. A bleeding was practiced, a half gramme of acetate morphine administered, and two frictions made upon the lumbar regions, when the pains disappeared. The frictions were continued for several days. On the 23d, the patient could sit up in bed and hold his head erect. There was still a little rigidity of the abdominal muscles. The patient continued to improve daily; during the mean time frictions were made. Upon the 4th July, he was perfectly convalescent.

Frictions with Chloroform in a Case of Tetanus.—The *Bul. Gén. de Therap.*, (July, 1851,) relates a case of Tetanus cured by frictions with chloroform. It would seem that the beneficial effects of chloroform are much more striking and more immediate than those of sulphuric ether. A report of each is given in this number. A man about 40 years of age was admitted in the hospital, under the care of M. Morisseau, affected with tetanus, the result of a wound upon the

anterior and lower part of the leg. M. Morisseau had immediate recourse to chloroform: he ordered general frictions with four grammes of this anesthetic, three times a day. In the evening the patient was placed in an acidulated vapor bath. The next day he was better; slept well during the night; perspired freely and the convulsions were less frequent. He was able to take a little liquid nourishment. 20 grammes of chloroform were ordered to be used in three frictions during the day, and two acidulated vapor baths to be taken. This treatment was continued during several days. The third day all the serious symptoms had passed away, and in a few days more the patient had quite recovered.

Scarlatina.—Dr. Volz has recorded his experience of a severe epidemic of scarlatina in Carlsruhe, from which he draws the following deductions:—

1. The extent and redness of the eruption are not in direct ratio to the severity of the disease.
2. The proximate cause of the exanthem is a stasis in the cutaneous capillaries.
3. The exfoliating scales of epidermis do not transmit the contagious principle of the disease.
4. The mucous membranes undergo the scarlatinous eruption equally with the skin.
5. The lesions of the throat are of three varieties—catarrhal, inflammatory, and gangrenous.
6. The inflammation of the parotid which accompanies scarlatina, seldom terminates in suppuration; that which follows the subsidence of the exanthem, often suppurates.
7. In the consecutive anasarca the alterations in the kidney are secondary, and depend on the change in the composition of the blood.
8. Death may occur in scarlatina from the following causes: congestive apoplexy, suffocation, pyæmia, and anæmia.—[*Med. News*.

Treatment of Urticaria by the Sulphate of Quinine.—This is an eruptive disease, usually distinguished by elevations of the cuticle in the form of *wheals*; it is sometimes exceedingly obstinate, resisting all the means that may be brought to bear against it. We are induced to notice this affection, because recently we have met with two or three cases that yielded only to large doses of quinine.

It is often quite simple in its nature, yielding readily to tepid baths, mild cathartics, and a restricted diet; but again, it is accompanied with much febrile disturbance, pain in the epigastrium, nausea, fullness in the head, and a burning sensation over the surface of the body; the face, hands and feet swell; the eyes are almost closed; the tongue is loaded with a white coat, and the itching is intolerable at times. Again, the eruption is accompanied with severe articular

pains, all of which phenomena serve to complicate the exanthema, and augment the difficulties of the case. Dr. Wickham and M. Legrouse of the Hospital Beaujon report some cases of the worst forms of Urticaria, which were promptly cured by full doses of quinine, continued for two or three days.

Treated with quinine the articular pains, the painful tumefaction of the face, feet and hands, the eruption itself, rapidly disappeared, together with the nausea, febrile excitement, and indeed all the distressing symptoms.—[*N. O. Med. Journ.*

Treatment of Hemorrhoidal Tumors by Collodion.—M. H. Gassier, of Marseilles, reports a case in the *Bul. Gén. de Thérap.*, March, 1851, of hemorrhoidal tumors, treated with collodion, which we will relate :

M. Guès, aged 48 years, of a nervous constitution, was affected for six years with hemorrhoids. All ordinary remedies were applied to relieve the disease, but they were without effect. The patient suffered intolerable pains for several hours after each stool, or indeed until the hemorrhoidal tumors had re-entered into the rectum. The cautery was proposed as an extreme resource, but the patient would not consent to its use. As the usual applications in such cases had been used in vain, collodion was proposed and applied before the tumor was reduced. The collodion produced very intense pain, but diminished very considerably in about twenty minutes. The following night he slept well, which was the first good night that he had passed in ten. From this time, the patient continued to do well, and six months has passed away without any return of the disease.

M. Gassier asks whether it was the constricting power of the collodion, or the anesthetic property of the ether, that produced the cure?

On a New Method of Treating certain cases of Epiphora. By WILLIAM BOWMAN, F. R. S.—This paper describes a new mode of treatment of those cases of epiphora which depend on a displacement of the puncta lacrymalia out of the course of the tears, or on an obstruction of the canaliculi between the punctum and caruncle, the inner extremity of the canals, together with the lacrymal sac and nasal duct, remaining healthy. The author describes the exact nature of these cases, and relates examples. The treatment which he has devised consists in slitting up the canal from the punctum on the conjunctival aspect, so as to carry backwards the orifice at which the tears are received on to the mucous membrane near the caruncle ; and he finds that the tears are in fact taken up by the remaining portion of the canal, while the end towards the punctum is converted into a groove. For the cases of obstruction from injury or other cause, he

suggests a modification of this operation, by which the canal between the obstruction and the sac may be slit up for some way, so as to receive the tears at a new opening. The cases to which these new operations are applicable, have been for the most part abandoned by surgeons as incurable.--[*London Lancet*.

Eclampsia ; various incisions of the neck of the Uterus ; delivery ; recovery.—Doctor de Beule, of Lokeren, has communicated to the Medical Society of Ghent a very remarkable case, which has been itself the subject of an excellent report by M. Frayes. It was a case of violent eclampsia, occurring between the sixth and seven month of pregnancy, in a woman with her first child, and who presented undoubted symptoms of labor. Indeed, from the commencement of the disease she complained of heaviness in the lower belly, of continued or bearing down pain in the region of the loins ; the neck of the uterus, effaced and slightly opened, admitted the end of the finger. The first attack of eclampsia was in the morning just as she awoke, having had the evening before some premonitory symptoms. Notwithstanding the active treatment which was adopted, the attacks became more and more frequent, and more and more intense, leaving the woman, in consequence, in a comatose state more and more profound and prolonged. Towards the evening the death of the foetus was almost certain, and the condition of the patient was so critical, that M. de Beule and the attending physicians, seeing her life in imminent danger, considered it necessary to terminate the accouchement. M. de Beule, by means of a straight bistoury furnished with a button, made, not without some difficulty, four incisions in the neck of the uterus, which was but little more dilated than at the commencement of the disease. Two of these incisions were made at the sides ; the other two in front and behind. The accouchement was then rapidly terminated by the abstraction of a dead foetus, by means of the lever ; and the convulsions ceased. The next day the woman was restored to consciousness ; the consequences of the confinement were natural, and the re-establishment of health was speedy.

The unbridling (*débridement*) of the neck of the uterus, sometimes designated under the pretending and even inexact name of *hysterotomy* or vaginal *cæsarean* operation, has already been practiced a great number of times either for the extraction of uterine polypi, or in case of occlusion, complete or incomplete, of the neck, or to make the extraction of the foetus more easy in case of sudden or apparent death of the mother, or in case of rigidity, organic alteration or spasmodic contraction of the neck of the uterus, or, finally, to replace forced accouchement under the different circumstances which may require it, among which figures eclampsia.

Confining ourselves to this latter, the cases in which it has been combatted by the unbridling of the neck, are still not very numerous. The first report of this kind is due to Dubose, who addressed it to the Royal Academy of Surgery. The operation was followed in five or six minutes by the spontaneous delivery of a dead child ; but calm-

ness was immediately restored to the mother, and she had a favorable confinement.

Contonly was more fortunate in a case of this kind; after the operation he delivered with the hand an infant apparently dead, but which they succeeded in recalling to life.

M. P. Dubois performed the same operation, in 1840, at the Clinical Hospital, on a woman affected with eclampsia. He afterwards applied the forceps, and delivered a living child. The woman, carried back to her bed without consciousness, had, nevertheless, renewed convulsive attacks, which were met by the application of thirty leeches to the mastoidal apophyses. About ten o'clock in the evening consciousness returned, and the next day the woman was in a satisfactory condition.

M. Godemir has published two private cases of his in which he was enabled, thanks to this operation, to terminate the accouchement speedily, in the midst of attacks of eclampsia, and to save the two mothers. The children lived but a few hours.

Lastly, M. Hubert, Professor of Midwifery at the University of Louvain, has performed the same operation under similar circumstances, and with equally fortunate results to the mother.

Accoucheurs are still divided in opinion as to whether, in a case of eclampsia, which resists the ordinary remedies, (bloody evacuations), and this is the case most frequently, it is proper to resort to forced delivery. Some, and they number among them high names in the Profession, think, that the operation of forced delivery adds still more to the nervous excitement, and only serves to increase the liability to accidents. Others, and they are becoming more numerous every day, contend that the delivery, forced or spontaneous, is the best means of arresting the convulsions of the mother. Notwithstanding the example of Dubose and Contonly, the unbridling of the neck was not resorted to in order to produce forced delivery, and Mr. Velpeau, after having delivered a woman, but with great suffering, by the introduction of the hand into the womb, regretted that he had not incised the neck. This operation, he afterwards said, is neither very painful, nor very dangerous in such a case; and the result has since justified his prediction.

The very remarkable case reported by Mr. de Beule, and those which M. Fraeys has mentioned, are certainly a powerful encouragement to resort in case of need to the unbridling of the neck and artificial delivery in puerperal convulsions, although delivery, natural or provoked, does not always terminate the eclampsia, as the case reported by M. Dubose suffices to prove. But, as an operation of this kind is always a serious matter, we will mention a remedy which has already succeeded twice in similar cases, that is: inhalations of chloroform.—[*Bulletin Med. So. Ghent. N. O. Journ.*

The influence of the hours of the day on Mortality.—The observations and calculations of Dr. Caspar lead to the conclusion that the maximum of deaths occurs in the fore part of the day, and the mini-

num between evening and midnight. The explanation of this, Dr. Caspar seeks in the analogy between sleep and death: sleep being the period in which great organic changes occur, is *pro tanto*, favorable to the dissolution of the individual.

The diseases which are the causes of death exert a modifying influence on the hour at which death occurs. The origin and progress of diseases, their exacerbations, and remissions, are frequently observed to occur at certain times of the day. The influence of these on the hours of death is shown by the following table of 5591 deaths from various diseases.

ACUTE DISEASES.	From midnight. to 6 A. M.	From 6 A. M. to noon.	From noon. to 6 P. M.	From 6 P. M. to midnight.
Fevers	64	75	66	66
Inflammations	160	164	182	160
Exanthemata	44	45	43	59
CHRONIC DISEASES.				
Phthisis	186	240	215	186
Atrophy	347	381	282	255
Hæmorrhages	163	186	161	121
Chronic Catarrh	41	47	34	24
Dropsies	90	119	93	64
Neuroses	267	267	191	179
Other chronic diseases,	76	102	89	85
Totals, { Acute	268	284	291	285
{ Chronic	1140	1344	1065	914

The next table exhibits the variations from the general rule, in the same class of diseases, on the side either of excess or deficiency, as indicated by the signs —|— or —.

ACUTE DISEASES.	From midnight to 6 A. M.	From 6 A. M. to noon.	From noon to 6 P. M.	From 6 P. M. to midnight.
Fevers	—16	—15	— — 1	— —30
Inflammations	—12	—45	— —31	— —26
Exanthemata	—22	—55	—18	— —95
CHRONIC DISEASES.				
Consumption	—27	— 1	— —17	— —11
Atrophy	— — 5	— —18	—15	— 8
Hæmorrhages	— — 6	— — 4	— —12	—22
Chronic Catarrh	— —29	— —31	—10	—50
Dropsies	— 6	— —17	— —11	—33
Neuroses	— —43	— — 6	—32	—17
Other chronic diseases,	—36	— 1	— —10	— —27

The following are briefly the conclusions of Dr. Caspar on the influences investigated by him:—

1. *As to births.*—More births occur from nine o'clock in the evening to six o'clock in the morning than during the other hours of the twenty-four. Labor-pains commence more frequently between midnight and three o'clock in the morning than at other times. Of those births which terminated during the day, the majority were male children. Labor is longer if the pains begin in the day-time than if it commence during the night. This influence is more striking with still-born than with living children.

2. *As to deaths.*—The maximum general mortality occurs during the earlier hours of the day, the minimum in the evening. Of special causes of death the relative mortality with reference to the time of day presents many variations. Inflammatory diseases present their maximum in the after-part of the day; fevers and exanthemata in the earlier hours of the night; hæmorrhages in the fore part of the day and in the after-noon; and the neuroses generally in the hours after midnight.—[*London Med. Gaz.*, April, 1851. *Med. Examiner.*

Feigned Insanity.—The Gazette Médicale Lombarde reports the case of a young herdsman, seventeen years of age, who, having violated a child seven years old, killed her on the spot by a blow on the head. When arrested, he stated he had been urged to the commission of the deed by the devil. On the day following his imprisonment, this youth, who was remarkable for his gaiety and intelligence, was found in a state of almost complete imbecility, unable to make a single step without trembling and crouching down, his head bent forward inclined to one side, his speech incoherent, and stammering, not giving any collected answers to the questions put to him. He did not seem at all conscious of the fate that awaited him. Two physicians, M.M. Windler and Zinck, declared the insanity feigned, upon the ground that they had never known such a form of the malady occurring suddenly at his age. The prisoner was subjected to the closest surveillance, but he was in everything consistent with his disease. Recourse was had to stratagem; his couch was set on fire, water was unexpectedly poured upon him through the windows of his cell; but he remained impassive beyond faint inarticulate cries.

The physicians nevertheless persisted in their opinion. When put upon his trial, the prisoner answered no questions, seeming to doze, and preserved throughout the same impassibility. The jury found him guilty of the crime, but admitted his insanity in extenuation! He was condemned for three years to the House of Detention. Returned to his cell, the prisoner, finding that he had escaped capital punishment, declared that he had been perfectly sane since his arrest, and that he had simulated idiocy at the suggestion of a fellow-prisoner.

There are few instances on record of feigned madness carried so far, or persisted in for so long a time, under the circumstances.—[*London Med. Gaz.* *Med. News.*

The reciprocal duty of Physicians and of the Public towards each other: An Address, delivered before the Medical Society of the State of Georgia, at its second annual meeting, held at Atlanta, on the 9th April, 1851, by RICHARD D. ARNOLD, M. D., President of the Medical Society of the State of Georgia, Member of the American Medical Association, &c. Published by order of the Society. Savannah: J. B. Cubbege. 1851. 8vo. pp. 30.

We take pleasure in calling the attention of the readers of the Journal to the clear, forcible, yet practical remarks of the able Pre-

sident of our State Society, on the reciprocal duties of the medical profession and of the public. We are especially pleased with the bold stand which he takes in favor of a liberal education, including a knowledge of the Latin and Greek languages as the proper basis for a medical education. We are satisfied that it is to the general deficiency in this particular, much of the discredit into which our profession has fallen, is justly due. What estimate must an enlightened public place upon a professional man who is grossly ignorant of all those branches of knowledge with which intelligent men every where are conversant?

The address urges upon physicians generally to receive no one as a pupil "unless he be properly qualified by a good preliminary education." This is a very proper rule, though we fear the instances are not very frequent in which it is applied. Yet the precept cannot be too frequently impressed upon those whose duty it is to guard the portals of the profession.

The duties of the public to the medical profession are briefly examined, and the speaker seems to think "it is in vain to seek any legislative aid to the ends proclaimed," though "he does not press any extravagant claims upon the public." We entirely agree with him in this opinion. We need expect no aid or protection from our legislative bodies, though we are justly entitled to it, and without it must suffer many evils. The difficulty we believe is incident to our democratic institutions.

This address is worthy of a more extended notice, but neither time nor space will permit more than these brief desultory remarks, which we close by urging upon our friends who have not a copy of the address, to procure it, and give it a careful perusal.

Health of Augusta.—During the present season, our city has enjoyed its usual good health. There have been a few cases of small-pox which was re-introduced among us in July, but the prompt efforts of our city authorities to confine it to the locality whence it appeared, were entirely successful, and the disease has since been extinguished by a thorough and general vaccination.

Editor of the Southern Medical and Surgical Journal.—We are pleased to announce the return of the Editor from his European tour, and that he will at once resume his editorial duties.

SOUTHERN MEDICAL AND SURGICAL JOURNAL.

Vol. 7.]

NEW SERIES.—NOVEMBER, 1851.

[No. 11.]

PART FIRST.

Original Communications.

ARTICLE XXXIV.

Typhoid Fever. By JOHN S. WILSON, M. D., of Muscogee County, Ga. (Continued from Sept. No.)

In the No. of this Journal referred to above, I had the honor of submitting to the profession, in a brief and imperfect manner, some remarks on the treatment of typhoid fever, concluding with the promise to extend them at some future time. I now proceed to comply with that promise; but before doing so, I beg the indulgence of the reader while I adduce some arguments and authorities, to sustain the third position assumed in my article, which were omitted for want of space. The position referred to is this: "3d. Typhoid fever is only a milder form of typhus."

The identity or non-identity of typhoid and typhus fever has, perhaps, given rise to more discussion than any subject connected with them; and when we remember that it is a practical question—when we remember that its settlement would remove much of the confusion and difficulty which now attend the study of these fevers, we should look favorably on every effort at its elucidation, however humble it may be. The evidence in favor of the position that typhoid fever is only one of the forms of typhus fever, and not a distinct specific disease, seems to me to be almost conclusive, notwithstanding the high authority of those who advocate the opposite opinion. I do not intend to weary the readers of the Journal, by long

quotations from numerous writers ; I will only refer to some of them, and briefly enumerate the arguments on the affirmative side of this question. It appears, then, to use the words of Dr. Bell, that "Both diseases have commonly an eruption, and both may prevail without any ; both are attended with frequent anatomical lesions ; the typhoid more generally with one kind ; but both may run their course without any such organic change. In both fevers the digestive and nervous systems are much deranged ; and in both there occur complications of bronchial and pulmonary disease and a morbid state of the heart." * * * * * "There still remains one point of reputed contrast between the two, and that is, the contagiousness of typhus," &c. * * * * * "But here again there is a nearer approximation than might first appear. Writers are not at all agreed respecting the non-contagiousness of typhoid fever. M. Louis now inclining to the belief of its being contagious, and some of the warmest advocates of the contagiousness of typhus admit its occasionally spontaneous origin." The next authority to which I shall refer is that of Dr. Joseph M. Smith, Chairman of the Committee on Prac. Med. Am. M. Assoc. His able report may be found in the Amer. Journal of Med. Sciences, Oct., 1848, p. 512, and I bespeak for it a careful perusal, by all who feel interested on this important subject ; and I believe with the author of this report, that *all* should feel interested in it, and that it should "continue to attract attention, until a unanimity in regard to it is attained among the great body of enlightened physicians." After examining the symptoms of each fever as given by different authors, he concludes by saying : "Upon the whole it seems to us, that the ingenious attempts which have been made to establish a specific distinction between typhus and typhoid fever, by an analysis of the *symptoms*, have yielded no other profitable result than a lucid exhibition of the modifications which genuine typhus assumes under different circumstances of temperament, habits and mode of life, climate, &c. In no other light can we regard the able researches on the subject by Louis and numerous other pathologists in Europe and America."

Dr. Smith next discusses the question, whether a distinction can be founded on the *anatomical character* of the two dis-

eases; and I regret that I can only enumerate the writers and observers quoted by him to show that lesion of the intestinal mucous membrane is a frequent occurrence in typhus fever. The authorities quoted by him, in Europe, are: Dr. Kennedy, of Dublin; Dr. M'Cormac, of Belfast; Drs. Graves and Stokes, of Dublin. All these, be it remembered, founded their opinions on their own observations, and from actual dissections. He also quotes the distinguished names of Drs. Hewet, Tweedie, Copland, Southwood Smith, and Marshall Hall, to the same point. He also quotes Dr. Davidson of Glasgow; and the oft-quoted Dr. Lombard, who, on examining cases of typhus at Dublin and Glasgow, recanted his continental pathology, and declared that the British typhus and the French typhoid fever were "not specifically distinct." The next physician referred to is Dr. Staberoh, of Berlin, who, after studying the typhus of England and Ireland for six months, declared that it "was the same as the typhoid affection of the Continent." The same report informs us that Dr. Stone, after treating and dissecting numerous cases at Bellevue Hospital, declares his belief, "that typhus and typhoid fever, so called, are identical." Prof. Clark has arrived at the same conclusion. Dr. Sweet acknowledged that his belief in the "non-identity of the diseases lessened daily." "Dr. Griscom, one of the physicians of the New York Hospital, states, that of ten autopsies of patients dying of ship-fever in that institution, in July, August, and September, 1847, six presented follicular disease of the intestines." With an extract from the concluding remarks of this report, I leave it; but before doing so I would call attention to the fact, that all the physicians last mentioned, founded their opinions on observations made among the emigrants sick of the ship (*typhus*) fever, which prevailed so extensively among that class in 1847 and 1848.

The report concludes thus: "In view of the facts which have been stated, it seems to the committee that no reasonable doubt can remain that typhus and typhoid fever are identical. Were medical men united in this conclusion, might we not hope, that with undivided attention and a union of effort, more rapid advances would be made in determining the causes of the morbid condition of the Peyerian glands and

other organs which occur* in some cases of typhus and not in others?"

The next and the best authority to which I shall refer, is that of Dr. D. F. Condie. This accomplished physician and writer, in a review of Dr. Bartlett's work on the fevers of the United States, in which he maintains the non-identity of typhus and typhoid fever, says: "In the present state of our knowledge on this subject, it is, we conceive, much safer to consider the typhoid as one of the forms of typhus fever. Its distinct and specific character has not, as yet, been satisfactorily established."

I now return to the *treatment*. In my former article, I briefly alluded to *bleeding*, local and general—*cathartics* and *opium*, in the treatment of the fever under consideration: before noticing other remedies, I will make a few additional remarks on the two last mentioned—opium and cathartics. I know that the cathartic practice, in the beginning of this fever, is founded on a plausible theory, and that it is recommended by the highest authority; for Dr. Watson says, in his Practice (p. 959), "*This I believe—that the intestines should be cleared by an active aperient in the outset; and that laxatives should be continued, if the bowels do not act every day without them.*" As to the theory, I believe it to be false, and do not regard it; but I confess that I feel some diffidence in opposing the views of so experienced and intelligent a physician as Dr. Watson; yet I feel constrained to do so in this case, and to object to the use of *active* aperients and *daily* laxatives in typhoid fever.

I do not deny that an active cathartic, in the very beginning of the fever, before it is fully developed, will occasionally succeed in arresting it; for a few instances of this kind have come to my knowledge; still I think, as a general rule, that the practice is unsafe and should be avoided. In some cases where there was a decided tendency to coma, without much manifest intestinal irritation, I have used cathartics a little more freely than usual, but I can not say that I have ever seen any marked benefit from them; yet I should remark that my experience in such cases has been limited; for I have seldom ventured to use them freely, even in the few cases above described, which

* It appears to me that the sense requires that this verb should be singular.

have been treated by me. The practice of giving *daily* laxatives, I consider almost as objectionable as that of using active cathartics: it may be admissible in some cases, and even beneficial, but as a general rule, I think the practice objectionable.*

I would remark, while on this subject, that, according to my theory, the coma and stupor in typhoid fever are more nervous than vascular—are produced more by exhaustion of the vis nervosa, than by cerebral congestion or vascular fulness; and this is the reason, I think, why we derive so little benefit from bleeding and purging, even where they seem to be indicated. By reference to my previous article, it will be seen that I enjoin much caution in the use of *opiates*, on account of the tendency to coma; but, admitting the correctness of the above theory, they might perhaps be used with more freedom than I have been in the habit of doing: and apart from theory, I have heard of cases in the practice of others, where they have had the most happy effect, even in comatose cases; still I think they should be used cautiously, for fear of converting a nervous coma or stupor, into a vascular congestion or inflammation. I see by an extract contained in the Sept. No. of this Journal, (p. 551) that Dr. A. G. Henry recommends the use of opium in *four or five* grain doses, in typhoid fever. He says: "I claim to have demonstrated beyond all reasonable doubt, by a long and careful experience and observation, that while the maximum doses of the schools are of doubtful utility, and often prove injurious in fever, by increasing the dryness of the skin, aggravating pain in the head, &c., &c., a *five grain dose* will, nineteen times in twenty, produce free perspiration and relieve every unpleasant symptom. The notion that so generally prevails among the profession, that opium cannot be used to advantage in fever while there is determination to the brain, is certainly erroneous, if it is given in the doses which I recommend, unless there is *actual* inflammation," &c. Now, notwithstanding my cautions with regard to the use of this remedy, I am inclined to favor the practice here recommended, if we only had infallible diagnostic symptoms by which we could distinguish the cases to which it is applicable; for I believe if

* Vide Dr. H. V. Wooten's excellent article on this subject, in this Journal, Jan. 1850, p. 22. Also Dr. Long's, before referred to.

we could do this, it would be better to give the opium in decided doses ; but as it is very difficult for us to determine the cases in which large doses would be safe, I think they should be used with much circumspection. (Vide Dr. Latham's opinion, Watson's Practice, p. 961.)

I now proceed to notice some other remedies, in this fever, and the next and perhaps the first in importance, is—*Calomel*. I think I may say, that there is no great difference of opinion in the profession, as to the use of calomel, as a purgative and alterative ; but when we come to consider its constitutional or sialagogue effects, we find physicians and writers much divided. It is not my intention to introduce the authorities, pro and con, on this point—I shall, in discussing this, as well as other points connected with the treatment, confine myself mostly to the opinions which I have formed from my own experience and observation ; for, apart from the latter, I consider my *opinions* of but little consequence to any one. On the use of calomel as a sialagogue, in typhoid (continued) fever, I will make only one short quotation from Dr. Watson (op. cit. p. 960)—He says, in referring to a particular fever, (continued) which had prevailed in London : “A great number of the patients were brought, sooner or later, under the specific operation of that mineral (mercury) ; and in these patients (with one exception only, where the mercury appeared to do neither good nor harm) a *decided improvement** was almost immediately apparent upon the supervention of soreness of the mouth ; and all such patients ultimately recovered.” Notwithstanding this strong evidence in favor of the specific action of mercury, in continued fever, Dr. Watson says farther on : “In the form of fever that is *now* epidemic, I do not think mercury is so proper.” He concludes the subject with this remark : “I should give mercury very cautiously, therefore, if at all, in this typhoid variety of the fever.” In my practice, I have seldom been able to produce the specific action of mercury, (my experience corresponding with Dr. Long's) but in the few cases in which ptyalism, even in the slightest degree, supervened, there was invariably a decided improvement.

I shall not attempt to say whether the constitutional action

* The italics are Dr. Watson's.

of the mercury was an evidence of the mildness of the disease, or the cause of the amelioration : I only state the fact, and declare myself one of the advocates of the mercurial practice, for reasons which I will now give.

I advocate the use of mercury then because I *think* it moderates the fever, and if it does not shorten it, it renders it less dangerous and more manageable. I advocate its use, again, because it is universally admitted to be the best remedy we have to unlock the emunctories of the whole economy and restore their secretions ; which secretions are always suspended in typhoid fever. These are my principal reasons for giving mercury in this fever, and I am influenced more by the latter than the former : I give mercury principally with a view to its action on the secretions, glandular, follicular and cutaneous ; if ptyalism supervene I am pleased to see it, because my experience has taught me that it is an omen of good—that it is either the evidence of a mild and manageable disease, or the efficient cause of the amendment which invariably ensues. But while I advocate the judicious use of mercury, I must condemn the practice, too prevalent among some, of giving mercury in all cases, under all circumstances, and in unreasonable quantities. I only advocate its use as a *general* rule, and in small doses ; for I believe that large doses are not only unnecessary, but positively injurious ; and I believe, again, that there are cases in which it is not so appropriate, even in small doses : for instance, where there is great prostration of the vital powers, with more nervous than vascular excitement. To conclude, then, on this part of the treatment : I generally give calomel in 2 or 3 gr. doses, every 2 or 3 hours until slight ptyalism supervenes, or until the restoration of the proper secretions, and the abatement of all the unpleasant symptoms indicate that the disease is subdued. I always endeavor in this fever, as in all others, to avoid profuse salivation ; I watch closely for the effects of the mercury on the gums, and as soon as they are manifest in the *least* degree, I suspend its use. I always, before commencing the use of mercury, note particularly the condition of the gums and teeth, for without this precaution, they become so coated with sordes and the secretions about the mouth so depraved, that we might be de-

ceived were we to rely on the usual signs of salivation. I have noticed if the gums are red at first, the first evidence of the action of the mercury on them, is a change to a blueish appearance.*

Cold affusions.—To the use of this remedy, when applied to the whole body, I will devote but few words, as I have no experience with it in that way: I will only remark, then, that I have the same objection to it that I have to emetics and active cathartics, viz., that we cannot expect to *wash out* the disease by drenching the surface with cold water; and I object to the remedy again, because, I consider it dangerous and too much on the heroic order, for such a disease as typhoid fever. Of cold affusions to the head, and cold sponging to the surface generally, when the skin is dry and the heat above par, I can speak in terms of decided commendation, having often witnessed the most happy effects from them. My experience has taught me that cold affusions to the head, in the delirium of typhoid fever, are more effectual than any other remedy, unless cups to the temples, in the early stages of the fever be excepted. The head should be shaved and the coldest water should be poured on it, *freely and continuously*, until the delirium is subdued; and the remedy should be repeated again and again, on every exacerbation of the delirium. A very good plan, after the patient has been quieted by the cold affusion, is to have a funnel partly filled with cotton, through which the water may pass guttatim; regulating the quantity so that it may evaporate and not wet the clothing, bed, &c. It is needless for me to say that cold water, used after the manner above recommended, is a potent remedy, and that its effects should be watched closely; and more especially when there is a strong tendency to coma.†

In the use of this remedy the effect produced on the pulse, the temperature and the brain, will generally be a safe guide. As to the use of cold sponging, the feelings of the patient, when he is sensible, will be an additional and important aid, directing

* On the use of mercury, external and internal, consult a very interesting article in the Sept. No. of this Journal, translated from the French, by Dr. James Bryan, p. 546, et seq.

† I have known a case where coma was relieved, *after* cold water had failed, by *warm* affusions to the head.

us with almost infallible certainty, when to persist and when to discontinue.

Diaphoretics.—Diaphoretics, magnified by some into principal remedies, in typhoid fever, with the intention of eliminating a supposed morbid matter from the blood, I consider only secondary and auxiliary remedies, in this fever, as in all others. Still, while I place them in the secondary list, I esteem them an excellent class of remedies, when they are given in conjunction with others, and in accordance with the general indications of treatment; and I might add that they are, perhaps, more entitled to be regarded as principal remedies, in typhoid, than in any other fever. I will not attempt even to enumerate all the numerous articles of this class which have been recommended, for I think that a few well-selected ones are sufficient to fulfil all the indications that may arise. I will therefore confine my remarks to those to which I am most partial—those which I have almost exclusively used. My list is short, consisting only of ipecac, pulv. Dov., spts. mindereri, camphor, carb. ammonia and the asclepias tuberosa. The ipecac and spts. minder., I use in the earlier stages of the fever—the camphor and ammonia in the more advanced stages; while the pulv. Dov. may be used at any time, with due caution, according to the circumstances of the case. The carb. ammonia, may be merely neutralized by the acid forming a simple neutral mixture, or if a stimulating diaphoretic be indicated the ammonia can be added in excess.

The Asclepias, or butter-fly weed, I esteem highly as a diaphoretic, and I do not think its virtues are properly appreciated. As it is not stimulating, it may be used in conjunction with other remedies, in any stage of the fever. It acts finely when given in warm infusion, combined with Dov. powders or either of the remedies already mentioned.

Blisters, external stimulants, &c.—I can say with Dr. Wooten,* that “there is no single agent of which I think more highly than blisters” in typhoid fever. It is impossible to say at what time they should be applied; as this will vary in different cases it must be left to the judgment of the physician;

* Vide this Journal Jan. 1850, p. 25.

but I think that they might frequently be applied earlier than they are generally, with much advantage to the patient, and with but little risk. If the abdominal soreness or the diarrhœa persist, after the free application of cups, I apply a large blister to the abdomen, unless the febrile action still continue of a pretty high grade.* I keep the blister open for a considerable time, and then suffer it to heal. Should the abdominal soreness still remain I use the cups again, and reapply the blister; this course, I continue as long as it is indicated by the symptoms above mentioned, and I am pleased to say that I have never seen any ill effect from it, but on the contrary the most decided benefit. I know that blisters are no neutral remedies, and that they will do harm when ill-timed; but I think that some commit an error, in waiting until the necessity for them no longer exists, or until the case becomes hopeless, before they resort to them; instead of using them earlier and keeping them open, regarding them not as stimulants to re-excite the exhausted powers, but rather as the most potent agents at our command for the subduction of local disease. As to other external applications I have but little to say. Stimulants to the extremities, frictions, &c., are generally used in the last stage of this fever, when coma and other symptoms, too familiar to all, fore-shadow approaching dissolution; I use them too, not omitting blisters,† but I have never seen any advantage from them. I only use them as a forlorn hope, and for the purpose of satisfying the friends, that all has been done that afforded the slightest prospect of relief. Of course, it will be understood that I now speak of those *desperate* symptoms, which immediately precede death; before these appear, stimulants to the extremities, or even to the whole surface, when the temperature is below the natural standard, are no doubt beneficial; and there is one to which I am particularly partial, and that is the frequent use of a *hot* foot bath—to the water I frequently add ashes or ley, and I think its virtues are thereby increased. While on external applications, I will mention warm poultices,

* I have prevented any increased excitement by giving a full dose of opium at the time of applying the blister.

† The plan, recommended by some, of applying a blister to the whole scalp, in such cases, I have never tried, though I think it *might* be useful; still its advantages are very doubtful and it is liable to several objections.

&c., to the abdomen. These are generally very grateful to the patient and have a good effect in soothing the irritation of the bowels. I generally use a simple poultice made of corn meal, or mullein wrung out of hot water.

Stimulants and Tonics.—My notice of these remedies shall be very brief, because I have used them but little, and because my design is, to confine myself principally to the results of my own experience in the treatment of this fever. Stimulants may sometimes, doubtless, be used to advantage, and in some cases where there is great prostration, they are no doubt positively demanded; but the cases which have fallen under my observation have seldom required a resort to these remedies; the persistence of abdominal or cerebral excitement, or both together, contraindicating their use. I have sometimes used them as a dernier resort, under the circumstances mentioned in connexion with *external* stimulants, but never with the least benefit. I think, when used at all, it should be done with great caution, and if the usual evidences of their favorable action, such as diminution in the frequency of the pulse, &c., are not soon manifest, they should be discontinued. The above remarks will apply mostly to the other class of remedies—tonics: they cannot be used until convalescence is fully established and then they are of doubtful advantage. There is one important article of this class which deserves farther notice, and that is—*Quinine*. I have tried the sulph. quinine, in all stages of typhoid fever, and in doses ranging from 1 gr. to 20 grs., with as strong prejudices in its favor as *any one could possibly* have, yet I am sorry to say that all my expectations have been disappointed, and that I have never seen the *least* benefit from it: generally it seemed to be neutral, acting about like so much chalk; but sometimes its effects were manifestly injurious. I never resort to it now as a febrifuge, unless there be a *very decided* remission—as a tonic, other and less costly articles will answer as well.

Astringents and some other remedies.—As I said in my previous article, I have seldom been annoyed by that troublesome complication—diarrhœa; I have always been able to control this without the use of astringents, by giving small doses of

calomel combined with opium or Dov. powders. Should this not be sufficient, or should the opium be contraindicated, I would prefer the acet. plumbi, the nit. silver or pure tannin, to all other astringents. The balsam copaiva and turpentine emulsions I have prescribed in some cases of diarrhœa and intestinal inflammation, and was pleased with their effects. I have seen some cases in which the tenesmus was considerable; the intestinal disorder assuming a dysenteric character; in this class of cases, from their analogy to others, in which I have witnessed the happiest effects from it, I would recommend an enema of the nit. silver, 10 grs. to the oz. of water. In conclusion, I would repeat my former declaration, viz: that we have no specific remedy, or established mode of treatment, but that the treatment must be varied according to the peculiar circumstances of each case, &c. The plan of treatment then, which I have sketched, must be considered as that which my experience and observation have taught me to regard as the most *generally* applicable; while I have not entirely omitted the contra-indications that may arise, and the corresponding modifications in the application of the remedies noticed. It was my design in accordance with my promise, to "conclude with a general summary of my experience" in the use of the remedies noticed above, but I deem this unnecessary, as I have alluded to this sufficiently in treating of each separate remedy.

ARTICLE XXXV.

CLINICAL REPORTS, No. II.

On a case which presented difficulty in diagnosing between Acute Laryngitis, and Foreign Body in the Larynx. By D. C. O'KEEFFE, M. D., of Penfield, Ga.

The notes on the following case are presented as they were penned during its progress.

Feb. 6th, 1851. A child aged two years was suddenly seized with dyspnœa about 4 o'clock, P. M., while out at play. A record made three hours afterwards thus describes his condition:—Appearance healthy, face flushed, countenance anxious, indicative of great distress; respiration embarrassed, difficult,

sonorous and loud—audible at a distance of three or four yards; abundant discharge of transparent glairy mucus from the throat, the accumulation of which in the larynx causes emesis; some thirst, deglutition somewhat difficult, and excites cough. As often as the air-passages become burdened with mucus, emesis ensues, and affords temporary respite from dyspnœa; but the difficulty again returns and is relieved in the same way. Has distressing paroxysms of coughing, during which the danger of suffocation seems impending, and generally terminate in a discharge, more or less abundant, of frothy mucus. The constitutional disturbance does not amount to more than would be produced by the distressing commotions of emesis and coughing; nor is there any functional disorder save those connected with respiration.

The history received from the child's parents shed but little light on the cause of the difficulty. It is reported that the child was out at play, came in the house screaming, foaming at the mouth, and discharging an abundant secretion of frothy transparent mucous. His mother, under the impression that he had drank strong lye, gave him sweet milk in quantities sufficient to cause vomiting. Of this conjecture she could adduce no stronger evidence than his being where it was, other minor corroborating circumstances were mentioned, but so flimsy as to be unworthy of note.

Another opinion, resting exactly on like foundation, asserted that a cotton-seed had got into his wind-pipe; so that so far as the previous history of the case goes, the effect of the conflicting statements on my mind is neutral.

In this perplexity of opinion, I administered ipecac freely in order to disgorge the stomach of its contents, and incidentally disengage the foreign substance from the larynx, if that should be the nature of the case; for I must confess that the examination made of the fauces by candle-light, rendered still more difficult by the struggles of my little patient, gave but little certainty to my opinions. Emesis is exceedingly difficult to excite, having taken a large quantity of a strong mixture of ipecac, and but little matter yielded from the stomach, among which are pieces of undigested chicken eaten since accident. Vomiting affords temporary relief, but the mucous accumula-

tion in the air-passages soon returns, and the respiration becomes again embarrassed. Having ordered a dose of calomel and Dov. powder, and directed the ipecac given if dyspnœa returned, I left him for the night.

Friday morning, second day.—Rested partially, and spent the night under symptoms such as have been described; no action from bowels, respiration less embarrassed, but still difficult; some improvement in all the symptoms—got up this morning as usual and played with his toys for a short time.

The throat was now examined more thoroughly than heretofore. The mouth and tongue, as well as the tonsils and velum are perfectly healthy; there is a circumscribed slight redness behind and above the tonsils, better entitled to the epithet engorgement than inflammation; heat of surface normal; pulse somewhat accelerated. The only deviation from health consists in the function of respiration, which is still sonorous and loud, so as to be audible at some distance. While pressing upon the base of the tongue with a spoon-handle, a thick somewhat opaque mucous is seen bubbling out of the glottis, quite different from the transparent discharge already referred to.

In view of the absence of inflammation in the fauces, and a want of correspondence between the pectoral signs and constitutional symptoms, I pronounced it a case of foreign body in the air-passages, and advised consultation. Three physicians saw the case with me, one of whom was of the same opinion with myself, while the other two differed from the diagnosis already expressed in favor of its being a case of acute laryngitis brought on by contact of the caustic with the laryngeal mucous membrane. Notwithstanding the difference of opinion in the causation of the disease, there was unanimity in the treatment, which consisted in giving olive oil until it should operate, with occasional doses of ipecac.

Friday night.—The dyspnœa having become distressing, I was again sent for. The embarrassed respiration returned late in the afternoon with some fever; the oil has operated once, and but little effect from the ipecac—the same want of susceptibility to the action of emetics persistent. Administered ipecac to emesis, and had him put in the warm bath with some benefit to the dyspnœa.

Saturday morning, third day.—Met in consultation with the same gentlemen. The child spent a very restless night, but is better this morning; respiration less difficult; no fever, nor difficulty in swallowing; tonsils slightly enlarged, but redness and congested appearance of fauces diminished; discharges large quantities of opaque viscid mucus from the rima glottidis while the tongue is being pressed upon by a spoon-handle—a result which ensues as often as the examination is made. The act of deglutition excites a slight cough.

The calm reflections of twenty-four hours resulted in no change or modification of the opinions expressed at our last meeting; there is still contrariety of opinion in our etiological views—it is still foreign body in the larynx on one side, and laryngitis on the other. It was determined by all to give ipecac *ad emesim*, which was done; vomited tolerably freely in our presence, ipecac to be repeated three times during the day, and the warm bath used as many times; gum arabic water as a drink. Not unmindful of the aphorism of the Coan sage, “*quæ medicamenta non sanant, ea ferrum sanat*,” tracheotomy was suggested as appropriate in the event of the danger of suffocation becoming imminent, but was deemed advisable to be adopted as a last resort—a procrastination frequently destructive of the ends in view.

Saturday night.—The danger of suffocation becoming augmented, I was summoned again to see him; respiration embarrassed all day, but has undergone a change for the worse since about 4 o'clock, other symptoms the same as I have described them at the night visits. Not being in greater danger than I had seen him, I administered nothing, but ordered ipecac given, and the use of the warm bath, if the breathing should get any worse. The child's parents, alarmed at the persistence of the disease, and being cognizant to our postponement of tracheotomy until the condition of the patient should become perilous, were importunate in urging its performance, and threatened to dismiss from the consultation such as were known to be averse to it, and have others called in their place.

Sunday morning, fourth day.—Saw the case alone and found it much better, after 12 o'clock at night, having discharged a quantity of mucus, the breathing improved, and is now less

embarrassed than it has been since the beginning of his illness. Ordered castor oil given until it operates, and mucilaginous drinks continued.

Monday morning, fifth day.—Had no return of dyspnœa since last visit; rested well; has had three evacuations from bowels; respiration still improving—the loud sonorousness of inspiration is supplanted by the sub-crepitant rhœnchus. The countenance is dull and anxious; pulse frequent 130 per m.; heat of skin moderate. Ordered poultices to chest, and hive syrup every two hours.

Evening visit.—Find him much better; has vomited two or three times; no fever; no signs of thoracic disease now. Ordered castor oil to-night, and hive syrup repeated less frequently.

Tuesday morning, sixth day.—Find him still improving; no fever; respiration easy; seems to be free from indisposition; is feeble and coughs a little when he drinks.

February 19th.—Seven days since last visit; his father informs me he considers him well, and adds that he coughs a little sometimes when he drinks water. He has continued free from any trace of the disease up to date—September 1st, 1851.

REMARKS.—The difficulty of diagnosis in the above case is one of its chief points of interest; neither the inception, progress, termination or treatment modified, in the least degree, the conclusions arrived at on the first consultation. The laryngitis theory found favor principally in the circumstance that all the morbid phenomena exhibited in the case are found to exist in acute inflammation of the laryngeal mucous membrane, which is doubtless true; and it is equally true that acute inflammation of the same membrane will, and always does, produce important phenomena, which did not exist in the case under notice. If the record only presents the *out-lines* of laryngeal inflammation, but exhibits a *full representation* of another morbid condition, is it not logically fair to decide in favor of that whose picture is the more perfectly drawn? In order to show the difference between the symptoms detailed in the record, and those of acute laryngitis, I will adduce the latter from the August No. of the London Lancet.

The subject was a tall, thin, delicately formed Bengalese aged

30. Symptoms: "countenance anxious; breathing impeded and hurried; voice husky and thick; inability to protrude the tongue, which was, as far as could be seen, dry and very much coated; constant spitting; deglutition appeared impossible; pressure over larynx and trachea caused intense pain, and the whole of the trachea appeared swollen; pulse 100; skin hot and dry; bowels confined." At 12 o'clock, after some active treatment had been instituted, the record says:—"Much the same; large quantities of mucus running from his mouth; deglutition still difficult; voice gone." Two days after:—"Dangerously ill; breathing hurried; sense of suffocation; countenance expressive of great suffering; large quantities of mucus clinging to, and dropping from mouth; larynx tender; voice a mere whisper; any attempt to look into the mouth caused great pain; every attempt to swallow caused spasm and the rejection of the fluid through the nose; unable to lie down."

This case, under active and judicious management, recovered, notwithstanding the alarming symptoms it presented. It will be seen that many important features of the above description have no parallel in the record: e. g.:—inability to protrude the tongue; a dry and coated state of that organ; *constant* spitting of thick, tenacious mucus; difficult deglutition—apparently impossible; larynx and trachea painful on pressure; hot and dry skin; large quantities of mucus running from mouth; loss of voice; deglutition causing spasm and rejection of the fluid through the nose; inability to lie down. Now, how could the case in question have been acute laryngitis when it lacked so many of its essential elements? It is proper to state that the œdematous variety of laryngitis is selected for comparison because the case we have been considering, if laryngitis at all, can be classed no where else, on account of the great disturbance in the respiratory function that existed throughout its course. It would be difficult to pronounce a case laryngitis that lacked so many of its important phenomena. There was no greater constitutional disturbance than ought to result from an imperfect aeration of the blood; there was not that restlessness, nor starting from sleep that are found in acute laryngitis, the patient could sleep sometimes from morning till evening. The inspection of the fauces, and external pressure on the

larynx, gave no indications of a local inflammation; the appearance of the tongue was normal. How could a disease, considered the most alarming and intractable, pass through all its stages without exhibiting unmistakable signs of inflammatory action?—a disease so rapid in its progress as sometimes to terminate fatally in eight hours, requiring treatment eminently perturbing and energetic. Indeed the treatment pursued in the case was a severe commentary on the diagnosis that favored laryngitis. If an acute inflammation of a vital organ was thought to exist, and that inflammation was exceedingly rapid in its progress and intractable in its nature, why was the child's safety compromised by putting him on a course of inefficient palliatives? The absence of inflammation in the mouth and fauces ought of itself apparently set the question of laryngitis at rest; for we would reasonably expect to find the mouth and fauces the seat of disease after drinking boiling water, or a substance such as has been suspected in the present case, rather than the larynx and trachea. But Dr. M. Hall assures us that the "effects of this accident (swallowing boiling water) are not, as might be supposed, *à priori*, the symptoms of inflammation of the œsophagus and stomach, but of inflammation of the glottis and larynx, resembling those of *croup*."

It is only necessary to recapitulate from the record the evidence in favor of its being a case of foreign body in the air-passages, the nature of which is not known. 1st. There was not more constitutional disturbance than ought to result from an impure state of the blood from faulty aeration. 2d. There was but little difficulty in swallowing as seen in the circumstance that pieces of chicken had been eaten since the accident; deglutition excited a slight cough which constituted the principal difficulty in swallowing. 3d. The paroxysmal character of the disease; the remissions amounted almost to intermissions from suffering—during the interval of the paroxysms, there was oppressed breathing with rattling in the throat. It will be seen that at morning visits there was always an amelioration of symptoms, while the dyspnœa returned at night approached and lasted a variable length of time. It may be asked what has become of the foreign body, and how account for the subsidence of the distressing symptoms? About 12

o'clock on Saturday night (vide record) large quantities of mucus had been discharged with marked relief to the dyspnoea, which continued to improve from that time. The foreign substance (cotton-seed or pebble) (!) was expelled then, or is yet embedded somewhere in the substance of the respiratory apparatus.

There are numerous instances on record of foreign bodies dislodging themselves from mucous canals after they have become enveloped in mucus without any artificial interference. This principle of nature ridding parts of the presence of foreign substances is designated the stimulus of relief, and is considered of such importance by some surgeons that they would never attempt opening the trachea while the foreign body is capable of mobility. The second clause of my answer, viz:—that if the *origo mali* had not been expelled from the air-passages, it is still embedded somewhere in the substance of the respiratory apparatus, finds ample corroboration in the annals of medicine.

A Monk got a cherry-stone in his trachea, and was seized with alarming symptoms of suffocation forthwith. He soon obtained relief and slept two or three hours; for a whole year he did not feel the least inconvenience from it. At the end of this time he suffered from cough and fever, and evacuated a stone as large as a nutmeg, composed of tartarous matter, to which the cherry-stone had served as a nucleus. A copious purulent expectoration followed the discharge of the foreign body, and the patient died consumptive some time afterwards.

A case occurred to Desault, in which a cherry-stone was lodged in one of the ventricles of the larynx; the patient would not consent to an operation, and died in two years of laryngeal phthisis. (Vide Cooper's Surg. Dict.)

The following cases from Ranking's Abstract, July, 1849 though recorded as instances of foreign body in the œsophagus, are, in the editor's opinion, in all probability, cases of foreign body in the air-passages. A young man aged 16 years swallowed an ear of grass, was immediately deprived of the power of speech, and breathed with such difficulty that he appeared to be suffocated. Shivering and fever supervened, and all the symptoms of a formidable disease of the lungs. Active remedies were employed. On the seventh day of the disease, a

tumor the size of an egg appeared between the sixth and seventh true ribs, causing acute pain. Suppuration followed, and the abscess opened of itself on the thirteenth day, when after the discharge of much fetid matter, the mother of the patient withdrew a body which turned out to be the ear of grass. The young man completely recovered. A child of six months, swallowed an ear of grass with which he was amusing himself. The accident was followed by a violent coughing, which, however, did not favor the descent of the body, because the awns were so disposed that they should have made it re-ascend. In the posterior part of the chest, considerable inflammation took place, and terminated near the fifth rib in an abscess, in which the ear of grass was found.

A girl of one year swallowed an ear of corn,* (wheat) which was immediately followed by violent coughing. On the fifteenth day, the patient discharged fetid pus by vomiting; and on the same day, appeared a tumor on the right side, near the superior ribs. The surgeon felt fluctuation, opened the abscess and withdrew the ear of corn, which presented itself first, and then followed much matter. Air used to come through the opening. The child recovered perfectly in five weeks.

In several Nos. of the London *Lancet* for this year, are recorded cases, showing how foreign substances, such as pins and needles, may be retained in the tissues of the organs for an indefinite length of time without producing any injurious effects. It is now seven months since the occurrence of the accident in the case under notice, and we have no guarantee that the child is out of danger; time, which "overthrows the illusions of opinion, and establishes the decisions of nature," can alone determine whether or not evil consequences may ensue.

ARTICLE XXXVI.

Surgical Cases. By WM. NEPHEW KING, M.D., of Roswell, Ga.

The following cases have presented themselves to us in a region of country not very prolific in surgical practice. In the treatment of most of them, we have been assisted by an accomplished friend, Dr. Chas. T. Quintard. As it is a matter of

* In Great Britain and Ireland, the term corn is used in reference to wheat.

some interest to collect the surgical statistics of the various parts of our State, we offer them for publication, without farther preface :

CASE I. *Fracture of the bones of the forearm, with extensive laceration of the soft parts about the wrist.*—A. H., aged 10 years, in attempting to “strip a card” in the factory, had his hand caught in the wheel, and his arm drawn in between the two revolving cards, stopping the machinery. The band was slipped and his arm removed. We saw him in twenty minutes after, and found him in the following condition:—There was great laceration of the soft parts above and at the wrist; rupture and laceration of the radial and ulnar arteries; together with fracture of both bones of the forearm. The flexor carpi radialis, palmaris longus, supinator radii longus, had been with the integuments torn by the teeth of the cards, a number of which were found in the muscles of the palm of the hand. The integuments had been torn down and the palmar aponeurosis completely destroyed. There was no very great hemorrhage; ligatures were however applied to the arteries, the ends of the bones placed in apposition, and *cold water* dressing prescribed. Although the destruction of the soft parts had been very great—muscles, tendons, nerves and arteries being destroyed—the patient progressively advanced toward a cure. He now has a very partial use of his fingers. There has been no perceptible increase in the use of the hand since the accident, now upwards of eighteen months.

CASE II. *Lithotomy.*—B., æt. 7 years, had been troubled with the usual symptoms of stone in the bladder, since *his third* year. *Two years* ago he was sounded by a medical man, who detected a calculus,—his general health good. On the 10th July last, assisted by my friends, Drs. Henry F. Campbell, of Augusta, and Dr. Quintard, we proceeded to perform the operation for his relief. The patient having been placed under the influence of chloroform, the *bilateral* operation was performed with the lithotome: the stone was seized with a pair of small forceps and readily extracted—it measured in its longest diameter, *three and two-thirds* inches, and in its shortest *two* inches,

and is composed of oxalate of lime. The patient did well and in a few days was up and about.

CASE III. *Compound comminuted fracture of the leg—Amputation.*—G. L., æt. 17, of anæmic habits; in falling from a peach tree, a distance of about 8 feet, fractured both bones of the leg. The fibula was fractured transversely, about six inches above the ankle joint; the tibia was fractured also transversely, about three inches above the joint, and forced through the flesh. The accident happened two miles from his residence, and we saw him about six hours afterwards. The anxiety of his parents induced us to attempt to save the leg, and after chloroforming the patient, the bones were placed in apposition and the leg dressed. But the next day it was evident it could not be saved, and the friends of the patient acquiesced in an amputation. Assisted by my friend Dr. Quintard, and Mr. B. King, the usual flap operation was performed, just above the knee—very little blood was lost and but *one* ligature applied. The stump was dressed and brandy administered, at intervals, until the patient had recovered from the shock of the operation. Three grains of quinine with a fourth of morphine were given. The patient continued restless for some time and gradually sunk into a troubled sleep. During the next day the patient was supported by brandy and quinine. Finding his bowels constipated, a dose of oil was administered, which did not act during the next twenty-four hours; at the end of which time an injection was given and repeated several times. He finally had a very good evacuation of the bowels, and there passed a quantity of scyballæ and water-melon seed mixed in the fœcal matter; after which he seemed quite easy, and having taken a Dover's powder, fell into a sleep. He did not pass a very comfortable night,—in the morning his pulse was weak and quick—gave him brandy and quinine. The stump looked very well on the inner side of the flap, and was healing by the first intention; but the external part looked pale and presented no appearance of union, on the contrary seemed inclined to slough. He continued about the same until the eighth day, when his pulse began to flag and become weak in spite of the treatment which had been contin-

ued up to this time. The stump became œdematous and the swelling passed up as high as Poupart's ligament by evening. Next morning it had continued above the hypogastric and iliac regions, involving a portion of the scrotum. A camphorated liniment was now applied, bowels were opened and brandy and quinine given every hour. Saw him at four in the afternoon: found him sinking rapidly; the swelling continued up as high as the ensiform cartilage. He now complained of intense heat and pain, particularly in the right iliac region. Every thing was rejected by the stomach, even a tea-spoonful of chicken water—hiccough finally supervened and our patient sank into the arms of death, retaining the full possession of his faculties to the last.

It would no doubt have been proper to have amputated in this case at *once*; but the prejudices of those with whom he has to do, often come between the physician and what he conceives his duty. It is a little remarkable that a fall of apparently so trivial a character should have produced such terrible injury. This may perhaps be particularly accounted for by the peculiar condition of the patient's system. He was thin and spare, having very little color, and scrofulous looking hair—there was but little vigor of body: about a year ago in walking through the Factory in which he worked he fell and fractured his arm: this was also a transverse fracture. His bones were extremely brittle and the external plate of the tibia was inordinately thin. Although the circumstances were very unpropitious for an immediate amputation, if we were to be placed in similar circumstances, we should adopt it as the only chance of saving the patient's life. We amputated above the knee, as we found some bruises just below it.

CASE IV. *Injury of the leg and foot.*—A laborer at a saw-mill got his foot entangled in the wheel whilst attempting to remove a block that had been caught under the wheel of the mill. He was standing in the "race-way," up to his arm-pits in the water, at the time of the accident. By a turn of the wheel, as soon as he had removed the obstacle, his foot and leg were drawn in by the current, effectually *stopping the motion of the wheel*. He was removed only after the shaft had been cut

and the wheel lifted out. We saw him some time after the accident, he was in the following condition:—The anterior surface of the foot and leg was very much torn and lacerated; some of the tendons were torn loose. The dorsal artery of the foot was ruptured, producing considerable hemorrhage, but it had been arrested by a compress and application of cold water. The lower portion of the *tibia* was *entirely exposed* save the periosteum about three inches up, the skin flaccid, and muscular attachments entirely torn loose—the ligaments at the ankle were left intact, however, save the anular ligament. All of the anterior surface of the foot and leg, and also the lateral portion, were more or less torn and bruised. After removing the bandage, &c., we found that there was little or no hemorrhage, save venous. The coagulum having been formed sufficiently strong to close the mouths of the bleeding vessels we did not ligate them. I clipped off such of the tendons and muscles as were hanging loose. Upon examining the wound, the probe struck against some foreign body, which proved to be fragments of wood, a part of the bucket of the wheel, lodged behind the *tendo achillis*. They were extracted, followed, however, by a considerable flow of blood. I continued the examination until I had removed every fragment. I now ordered a large elm poultice to be applied, enveloping the foot and leg in the same, and administered a dose of sulph. morph.

In the morning the wound was dressed, removed some other fragments of wood, cut off bits of loose muscle, skin, &c., and applied cold water dressing. This was continued until the third day, when sloughing of the bruised parts began. This process was encouraged by poulticing. The sloughing being confined entirely to the bruised portion, and the line of demarcation being perfectly distinct, we felt no uneasiness at its extending up the limb.

No interference was interposed—we waited until nature had done her work, and then removed the dead portions. The patient, as soon as the sloughing process began, was put under the use of wine and quinine. The wound now was dressed with pulv. lignia and a small portion of quinine to act as a stimulus to the healthy parts and as a purifying agent. Small and firm granules were gradually developed, and the case progressed rapidly to a cure.

CASE V. *Tumor of the Knee*.—P. I. G., of plethoric habit; presented himself with a tumor on the inner side of the right knee. It had been growing for two years, and when he came under our notice it had attained the size of a goose egg, producing some pain and great inconvenience in locomotion. The patient was chloroformed, the tumor removed and found to be adipose in its nature. Two small arteries were ligated—the wound dressed with sutures and straps and although it was impossible to keep the patient confined or quiet, it healed very rapidly.

PART II.

Eclectic Department

Typho-Mania. By A. V. WILLIAMS, M. D., late Visiting Physician to the New York City Lunatic Asylum, on Blackwell's Island, and President of the Medical Board of the Emigrants' Hospital on Ward's Island.

To the Editor of the American Journal of Insanity:

DEAR SIR—The following copy of a communication, written, as I am assured, away from home at the bed-side of a patient, in fulfilment at the last moment of a deferred promise, was received by mail, while attending the late annual meeting of the Association of Medical Superintendants of American Institutions for the Insane; and whether wholly verified by time and further observation or not, the views it contains, though very hastily expressed, were deemed by the members of the Association present, to have an important bearing upon the interesting form of disease to which they relate, and I send it to you for publication, for the consideration of absent members and others.

It may be mentioned here, as pertinent to Dr. Williams' views, that Dr. Brigham, in his work on the brain, expresses his belief, that that organ "is primarily affected" in idiopathic fever, and that the primary disease is "probably functional," and appears to consist in such a "loss or change of vitality or natural energy of the brain, that it ceases to supply the other parts of the system with their due proportion of nervous fluid or energy." Now if the "delirium and disease of the bowels" in typhus and typhoid fevers are "consequences of a primary affection of the brain," as Dr. B. thinks, is it not reasonable to infer that, as one of the protean variations which almost every

disease will at different times assume, the delirium may show itself as almost the first evidence of indisposition, and, becoming more intense and protracting itself longer than ordinary, supersede the development of intestinal lesions and the usual course and many of the usual phenomena of typhus?

C. H. N.

BLOOMINGDALE, New York, May 20th, 1851.

My dear Doctor:—As you request, I will write you some account of that form of Insanity, which in our conversations and in my reports, I have denominated Typho-mania, from the striking typhoid character of the physical symptoms it exhibits and the corresponding treatment required for its cure. I do not, however, consider it a new form of disease. It was strongly placed before me whilst acting as Visiting-Physician to the New York City Asylum on Blackwell's Island. It prevailed amongst Irish Emigrants newly arrived, who had been exposed to the influence of Ship Fever, and suffered from the privations of a long sea voyage.

Every one who has had charge of an Asylum, for any length of time, will recollect to have had patients brought to him in a state of raving, incoherent madness, when the physical sufferings were so great as to strike the attention more forcibly even, than the mental condition. The bodily disease or prostration is so great that the question we ask ourselves is, will the patient live? As far as my knowledge extends, this form of insanity is not described in any of the systematic works on mental disease, but it is occasionally alluded to in Reports. Dr. Bell has written an interesting paper upon it, and Dr. Ranney described it in a paper on the Insanity of Foreign paupers, which was read before the Association of Medical Superintendents of Insane Asylums, at its annual meeting in 1850.

In the year 1848 a number of these cases were admitted into the City Asylum on Blackwell's Island, and the large majority of them were females recently landed. From a patient of this class, typhus was introduced into that Institution, and the same year the same thing happened at the Bloomingdale Asylum. These circumstances, together with the appearance of the disease, tended to confirm me in the view that the exciting cause of it was typhus poison. You suggested, as the reason why Dr. Bell had seen more of this malady than the other members of the Association, that he lived in a region where typhus prevails. If the typhoid fevers of New-England are identical with the typhus of Europe (our Ship Fever), which is held by high authority to be the case, your conjecture may be correct.

Since my connection with the Emigrants' Hospital on Ward's Island, I have seen this disease in its commencement.

The patient would perhaps exhibit mere irritability and become quarrelsome in the wards, when I have been asked to see him, and have detected the real state of the case, and despatched him to the Asylum, where the disease would run its course. One remarkable feature of this disease is, that when patients recover from it, they do so completely, and not by the gradual return to reason, as in ordinary mania. When this has not been the case, I have learned from some patients after a more protracted recovery, that they have labored under mania before.

In the early stage of the disease there will be heat of skin, quick pulse, furred tongue, fœtid breath, the odor of typhus, red eyes, generally constipated bowels, loss of appetite and great thirst. The mental symptoms are those of some persons in a state of intoxication verging towards stupor. They talk wildly and incoherently with general hallucinations, without any fixed delusions. If you arrest their attention, they will answer correctly but impatiently, and hurry off in their wild frolics, hallooing, shouting, singing and tearing their clothes. If drink is given, they gulp it down in the same hurry, and if they can be prevailed upon to take food, swallow it without mastication, bolting it like a famished dog. The mental excitement does not subside with the heat of the skin and febrile disturbance, but sometimes when the skin is cool and the pulse small and feeble, they will rave most. In these cases, if depletion should be resorted to, I doubt not the result would be fatal.

With regard to treatment I have only to observe, that it has been in accordance with the views I entertain of its nature, and has been that adapted to typhus fever, except as it might require modifications in particular circumstances. After the first stage I have found great benefit from morphine; keeping the patient under its influence, combined with tonics, until it run itself out. Tartar-emetic ointment applied to the neck and between the shoulders, has also been of great service.

In the autopsies I have witnessed, the sinuses and veins of the brain and its membranes have been loaded with dark blood; there has also been a congested state of the veins of the abdominal viscera. The brain itself has sometimes been softened, and there have also been effusions into the cavity of the cranium.

I do not recollect a case of typho-mania that ended in dementia. Three years since, a lady, after returning from a visit to her friends in Connecticut, was attacked with typhus fever, and in the second week of the disease that peculiar form of insanity set in, from which in a short time she completely recovered.

I think Dr. Bell remarks, that in some of his patients there

was a suicidal propensity, and that by starvation. This I have not observed, their aversion to food arising from the want of appetite and absolute loathing. In the worst cases they could be induced to swallow food, but in the rapid manner I have before mentioned.

If opportunity offers, I will examine Peyer's glands, to see whether they are affected in this disease. Dr. Ranney states that he has never seen petechiæ, although I do not consider the eruptions essential to constitute typhus. They are generally present, but frequently so few in number that they may escape observation.

I hope this hasty sketch may excite the members of your Association to further investigation.

Yours truly, A. V. WILLIAMS.

CHAS. H. NICHOLS, M. D.,

Physician to Bloomingdale Asylum, New York City.

Startling Facts from the Census.

MESSRS. EDITORS—In the interesting article on the increase of our population, published in your paper of June 12th, you remark in the words of *The Commercial*: "Either the free colored population are voluntarily emigrating at a rate beyond what is generally supposed, or there is something in their social condition that is entirely inimical to their physical prosperity."

Many arguments might be adduced to prove that the latter, and not the former alternative is the real fact. And as a remarkable illustration of this truth, I send you a copy of a few items from a statistical table which I compiled some years ago from the U. S. Census of 1840, and published in a country newspaper, without obtaining much notice, although it exhibits, in a most striking light, the amazing prevalence of insanity and idiocy among our free colored population over the whites and the slaves.

It is a matter of regret, that the U. S. Census of 1840, groups both these classes of unfortunates together, as if they were involved in one and the same calamity. And it is also to be regretted that there is no discrimination of the prevalence of these maladies among the free blacks and the slaves. The writer made an effort to have these imperfections obviated in taking the late census in 1850; but he has reason to apprehend, from what he has seen of the returns, that his feeble voice did not engage the attention of the "Commissioners" who were

entrusted with the responsible duty of preparing the forms; though they solicited suggestions from those who felt an interest in the subject.

The census of our own State, taken in 1845, carefully distinguishes between idiots and lunatics; from which it appears, that their ratio in the State of New York is about 4 to 5, or more, nearly 16 to 21, on the whole population; but it makes no distinction between the white and colored population in regard to the existence of these maladies. It is obvious, however, from the following schedule, that there is an awful prevalence of idiocy and insanity among the free blacks over the whites, and especially over the slaves. Who would believe, without the fact, in black and white, before his eyes, that *every fourteenth colored person in the State of Maine is an idiot or lunatic?* And though there is a gradual improvement in their condition, as we proceed west and south, yet it is evident that the free States are the principal abodes of idiocy and lunacy among the colored race.

STATISTICAL TABLE FROM THE U. S. CENSUS OF 1840.

STATES.	Total White populat'n	Insane and Idiots.	Proportion	Total Color'd popula.	Insane and Idiots.	Proportion.
Maine.....	500,438	537	1 in 950	1,355	94	1 in 14
New-Hampshire...	284,036	486	1 " 584	538	19	1 " 28
Massachusetts.....	729,030	1,071	1 " 662	8,669	200	1 " 43
Vermont.....	291,218	398	1 " 731	730	13	1 " 56
Connecticut.....	301,856	498	1 " 606	8,159	44	1 " 185
Rhode Island.....	105,587	203	1 " 520	3,243	13	1 " 249
New-York.....	2,378,890	2,116	1 " 1,108	50,031	194	1 " 257
New-Jersey.....	351,588	369	1 " 952	21,718	73	1 " 293
Pennsylvania.....	1,676,115	1,946	1 " 861	37,952	187	1 " 256
Delaware.....	58,561	52	1 " 1,126	19,524	28	1 " 697
Maryland.....	317,717	387	1 " 821	151,515	141	1 " 1,074
Virginia.....	740,968	1,052	1 " 704	498,829	381	1 " 1,309
North Carolina....	484,870	580	1 " 835	268,549	221	1 " 1,215
South Carolina.....	259,084	376	1 " 689	335,314	137	1 " 2,440
Georgia.....	407,695	294	1 " 1,387	283,697	134	1 " 2,117
Ohio.....	1,502,022	1,195	1 " 1,257	17,345	165	1 " 105
Kentucky.....	590,253	795	1 " 742	189,575	180	1 " 1,053
Louisiana.....	158,457	55	1 " 2,873	193,954	45	1 " 4,310

In the preceding list, I have aimed to give a view of this subject on the territory occupied by the thirteen original States. I have added Ohio and Kentucky merely to show that the same contrast between the old free and slave States exist in the new. Ohio and Kentucky, though contiguous to each other, and of nearly equal age, exhibit the same amazing difference. In the former, there are just 10 colored persons who are idiots or lunatics, where there is one in the latter.

In Louisiana, where a large majority of the population is colored, and *four-fifths* of them slaves, there is but 1 of these

poor unfortunates to 4,310 who are sane. In fact, the want of sense or reason appears to be a rare visitation upon those who are held in slavery. This is an ample theme for the speculations of the physiologist and the moralist.—[*N. Y. Observer.*

Punctured Wound of Abdomen—Wound of Liver: Recovery.

By FREDERICK G. LEROY, M. D., Resident Surgeon.

Wm. Craddock, æt. 27, New York, seaman, admitted into the New York Hospital, July 4th, 1851, (Dr. Post in attendance,) with a penetrating wound of the abdomen, caused by the premature discharge of a horse pistol, from which he was endeavoring to withdraw the ramrod. The rod passed between his hands, and struck the abdomen a little to the right of the median line, about an inch below the common cartilage of the false ribs on that side. Upon introducing the finger, and tracing the course of the wound, the liver could be distinctly felt, and on passing its extremity over the surface of the organ, a laceration of it was detected that corresponded with the external opening. From the nature of the injury, its precise depth could not be ascertained. Patient had on at the time of the accident, a common colored shirt, with a red one beneath it; the fragment of red flannel was removed by the physician who attended him previous to his admission into the hospital. The portions of the colored garment were supposed to have been carried in with the rod. Has vomited several times since the accident; at present complains of no pain, but has some difficulty in respiration. There was a slight venous hemorrhage from the wound. Pulse natural.

Treatment.—Edges approximated by means of a suture. Cold water dressing. R. Tr. Opii. M. xxx. P. M. R. Pil. Opii. gr. i. q. 2 h.

July 5th. Patient passed a comfortable night, after having taken two of the above pills. Bowels being confined, ordered an enema containing Ol. Terebinth ʒs; wound covered with lint saturated in m. g. a., and emplas. vesicans over this as a prophylactic. Strict diet.

July 7th. There is slight tympanitis; the bowels remaining constipated, was directed to . Cal. grs. ii. q. 2 h. until they were removed. Camph. cataplas. to abdomen. The calomel vomiting him after the second powder, it was discontinued: R. Haust. Effervesc.

July 12th. General condition favorable; no symptom of an urgent character having presented since the last note. At present there is no febrile action, has a natural stool, &c. The

slough is separating from the edges of the wound, which is closing up with healthy granulations. To day, removed several pieces of cotton, which upon examination proved to be the missing portions of the colored shirt.

July 20th. Has had several paroxysms of pain over the region of the liver, and in the right shoulder, which he states to have been of a tearing character, and most intense in the evening. Respiration labored, and 27 per min. Pulse alternating between 72 and 80, without any undue excitement. *R.* Opiates Camph. poultice to abdomen, and emplas. vesicans., over the seat of pain; under the influence of which remedies, all the unpleasant symptoms gradually subsided.

July 23d. Several chills have occurred in the last two days, general condition very unfavorable, countenance haggard, hot surface, labored inspiration, cough, with pain in the side. Pulse 92, frequent and hard, slight dulness on percussion. *R.* Tart. Ant. et Potass. grs. $\frac{1}{4}$ q 2 h. Emplas. vesicans. to side.

July 24th. After taking one dose of the antimonial solution, patient became so much prostrated, as to require stimulants, and upon reaction being in a great measure established, *R.* Pul. Ipecac. gr. ss. m. g. a $\frac{3}{4}$ ss. as a substitute for the above solution. Still complains of pain in the side, and difficulty on inspiration. Auscultation reveals an indistinct crepitus. *R.* Emplas. vesicans. over the posterior surface of the lung.

July 30th. All the thoracic symptoms have subsided, with the exception of a slight uneasiness in the right side. Chills continue; has had as many as four in the twenty-four hours. After some of these attacks patient is very much prostrated, and at times quite flighty. For these the sulphate of quinine has been freely given, commencing with grs. x., and gradually increasing up to the present time, when he takes grs. xxv. in the day. The wound has entirely healed.

Aug. 4th. Has had no chills for four days past. Quinine has been continued to the present date, when it is directed to be gradually diminished daily. The whole surface of the trunk is covered with suppurating boils, that occasion considerable constitutional irritation; upon their subsiding, patient's whole system seemed to undergo a marked change for the better, and he commenced gaining in flesh.

Aug. 16th. Since last date, an abscess has formed, and pointed in the original wound, opened spontaneously and gave exit to a greenish discharge, pus apparently tinged with bile; upon its ceasing, the wound again closed, and he was discharged cured.

The present case possesses a peculiar interest, from the serious nature of the wound, and the entire absence of any report

of cases strictly identical with it in the different Journals, and surgical authorities that have been consulted. One of its most prominent features was the greenish discharge from the abscess, some of which was collected and placed in the hands of a chemist for analysis; but no satisfactory return could be elicited from him, although particularly requested to give it a careful examination. That this discoloration was effected by bile, is a fair conclusion, and one, I think, the history of the case justifies. What office nature performed in its happy termination, or how much should be attributed to the treatment employed, is a question for conjecture. But, that opium exerted a beneficial influence in this, is indicated by the pulse; and its efficacy in wounds of a similar nature will be allowed, by all surgeons, who have given it a trial. Cases of rupture of the liver occur, though they are rare, and are generally the result of fracture of the ribs, or some heavy weight passing over the body. But these lesions are not ascertained until revealed by an autopsy. A case is reported in the *Medico-Chirurg. Review*, New Series, vol. xxiv., p. 296, in which a cavalry officer was thrown from a horse, and the animal trod upon his chest with his hind feet. He survived the accident eight years, experiencing constant pleuritic attacks. Before his decease he fell under the observation of the physician who had attended him after the accident, who feeling an interest in the case, made an examination, and found a rupture of the right lobe of the liver, throughout its whole length and substance, from its anterior to its posterior border. The cicatrix was large, broad, and of a cartilaginous character; occupying the fissure beneath this, were biliary concretions. Two cases of wounds of the liver are recorded in the case books of this Hospital, though no symptom presented, by which they were recognized as such previous to death. One, was that of a boatman, admitted with a penetrating wound of the abdomen, inflicted with a butcher's knife. Patient lingered twenty-two hours. Autopsy disclosed an incised wound of the left lobe of the liver, three inches in length and one and a quarter in depth. The second, was a cartman who had received a gun shot wound of the abdomen, midway between the umbilicus and ensiform cartilage. Survived the accident twenty-four hours. On making an examination, the left lobe was found to have been lacerated by the shot, and the coats of the stomach perforated.—[*New York Medical Times*.

Spontaneous Combustion.

The question of spontaneous combustion being involved in a trial, recently, at the capital of the Grand Duchy of Hesse Darmstadt, and scientific opinions on the point conflicting with each other, the Judge directed Profs. S. Bischoff, and J. De Liebig, to be summoned, in order, by their investigation and testimony, to resolve doubts as to the probability of the occurrence of such an event.

A report of the case appeared in the *Archives Générales de Médecines*, Paris, which has been translated by a correspondent of the Western Journal of Med. and Surgery. The details of the case are interesting, but we are obliged to omit them on account of the space which they would occupy.

We give the oral report of these distinguished Professors on the subject of spontaneous combustion, aside from the particular facts in the trial referred to. It will be seen that they are convinced of the impossibility of such an occurrence.—[*Editor Buffalo Med. Journal.*

The first recorded case of spontaneous combustion, said Mr. Bischoff, dates back about one hundred and fifty years (that of Miliet a woman of Reims, in 1725.) By human *Spontaneous Combustion*, we understand that a man has been more or less burned, without our being able by external circumstances to explain the burning. Then it is that we say, this man has burned not by aid of external combustibles, but within himself (spontaneously;) an expression, which, implying an entire theory, seems to be incorrect. How much better it would be to say: we do not know how this person died, but, in saying that he died from spontaneous combustion, we only substitute an explanation quite as absurd. Thus the doctrine of spontaneous combustion, which has crept into the science, is the result of ignorance, and not of scientific research or experimentation. Upon what authorities does this pretended fact rest? The forty-five or forty-eight cases which have been published, relate to individuals burnt either where there was too little, or an entire absence of, combustible material, to account for the combustion. With the exception of two cases of complete and four of partial consumption by fire, the victims of which survived, no one possessing scientific knowledge, no witness of any sort, has been present during the accident. The first case of complete spontaneous combustion, was witnessed and described by a chamber-maid and an unknown person, and the second by a stranger. In the four cases of partial combustion, it is very evident that the first one was caused by burning sulphur, which the burnt person spread upon his clothing in the endea-

vor to extinguish the flames with his hands: the second was the case of a young girl of Hamburg, upon whose hands there appeared a flame which was followed by vesicles; but this fact though reported by a distinguished physician, was not witnessed by himself, or by any other person. Most probably then, these vesicles were the result of some disease, or perhaps of a burn intentionally made, for the purpose of attracting attention, or gaining admission into the hospital. The third case, which is very generally known, relates to a priest, named Bertoli, and was reported in 1786, by an Italian surgeon. Any unprejudiced person, who reads the account of this case, must be convinced that that individual was burnt by fire communicated to his clothing from a lamp. The fourth is still less worthy of confidence; it relates to a blacksmith named Reynaleau: a narration unconfirmed by the slightest evidence, full of contradictions, and winding up with a characteristic trait, that holy water only was able to extinguish the flames. Not one of these cases has ever been reported immediately after its occurrence by any one of recognized authority. Physicians have arrived in several instances after the catastrophe; and some of the cases have been subjected to a judicial inquest, and have the appearance of reality. But for an observer accustomed to criticism, and to the requirements of rigid observation, the reports which we possess are far from presenting the guaranties indispensable to an enlightened investigation. Phenomena such as those of spontaneous combustion, require a methodical observation, which the cases mentioned do not possess the slightest trace of. In none of them was there even an autopsy made, still less a serious, scientific investigation, or chemical analysis. In all the cases of spontaneous combustion which have called forth judicial or medical inquests, we constantly see an exhibition of levity, ignorance, prejudice, credulity, and very often of culpability. At the epoch when most of these facts were reported, science itself was not sufficiently far advanced to supply the lights necessary for an analysis of what was observed. As to the numerous cases that have been reported from hearsay, they are based upon the authority of the school-master, the curate, or the village mayor, but have never given rise to an inquest of any kind. A recent and very remarkable instance will demonstrate how, gradually, these accounts become introduced into the annals of the science. M. Bischoff here cited the case of pretended spontaneous combustion, published by the *Gazette des Tribunaux*, and reproduced in the *Journal des Debats*, for February 24th, 1850. The case was that of a man who, in a drinking shop, where he had, according to custom, drank deeply, introduced into his mouth in consequence of a bet, a lighted candle;

he was suddenly set on fire from within, and his head and the upper part of his chest were carbonized in half an hour in spite of all the assistance rendered. The death and the effects of the fire were reported to have been verified by two physicians. From information acquired by M. Liebig from different savans of Paris, and especially from the prefect of police, it turns out that the whole account was imaginative, and pure fiction, invented for the columns of the paper which had inserted it.

The question of spontaneous combustion has been treated of, adds M. Bischoff, by Rudolphi and Treviranus, MM. Kopp and Nasse. These savans have investigated and have expended a great deal of labor and of science for the purpose of explaining this phenomenon. But incredible as it may seem, without first assuring themselves of the truth of reports as made, they have admitted them as they were presented. Their explanation only could be important, and this is wanting. We do not deny these cases because we cannot explain them, but because their existence must be based upon explanations which tend to overthrow the laws, heretofore admitted as true and exact, of physics, physiology, chemistry and pathology. I will confine myself to the mention of the fact, recognized by all, that a body containing twenty-five per cent. water, does not take fire of itself, and does not continue to burn when started. Suppose we collect all the solid parts of the body, the bones, the skin, the tendons, the muscles, and put the water contained in the body into a vase; kindle these solid parts and their entire consumption will not afford sufficient heat to vaporize the water. Alcoholic excess, however, we are told, brings about a modification in the human body which renders possible its spontaneous combustion. It is true, all accounts of cases of spontaneous combustion tell us that the subjects were addicted to the abuse of ardent spirits; it is also true that alcohol is inflammable; and why not admit that the body soon becomes impregnated with it, and that, especially when fire is communicated from the exterior, it can burn. Such reflections may be made, but for the naturalist and physician they are valueless. The knowledge which we possess upon the passage of substances from the stomach and intestines into the blood, teaches us that alcohol reaches slowly, and in very small quantities, the sanguineous system, and as the circulation goes on with great rapidity, the alcohol is carried almost immediately into the lungs, where blood comes in contact with the air. There the elements of the alcohol become modified by its combination with the oxygen of the air, forming carbonic acid and water which the respiratory process eliminates from the system. Thus under ordinary circumstances we cannot even prove the presence of

alcohol in the blood, since it is decomposed and thrown off by the lungs. Here it may be thought that alcohol taken abundantly will penetrate the blood in substance and in large quantities, and spread itself throughout the whole system. Observations and experiments by distinguished men have been made upon this subject, but they are contradictory; some pretend to have found alcohol in the blood, and even in the brain of persons addicted to drink and who have died drunk. Percy discovered traces of it in the brains of dogs into whose veins he had injected a considerable dose. But the observations of Percy are in direct contradiction to those of Dr. De Pommer, of Zurich, who found no traces of the spirit in the blood. MM. Bouchardat and Sandras were unable to discover alcohol in any secretion except in the pulmonary exhalation. So far then it remains to be proved that the human body can imbibe alcohol like a sponge; moreover it is, *a priori*, impossible to admit that the body being saturated with alcohol life could continue for a single instant. The coagulation of the albumen, the arrest of the circulation, and the destruction of the nervous system, are the immediate results of the injection of any considerable quantity of alcohol into the blood of an animal. For my own part I am convinced that a dead body does not become combustible from being saturated with alcohol. I have taken parts of a dog into whose arteries I had injected alcohol at 92°, and they would not burn when exposed either to a flame or to the action of carbon; in the latter case only they roasted and carbonized, but even that ceased so soon as withdrawn from the action of the fire. M. Bischoff ended by refuting and ridiculing the stories of flames issuing from the mouths of drunken individuals.

M. Liebig, who, six years since, had given his opinion upon spontaneous combustion, (*Annales de Chimie et de Physique*, 1844, t. 1, page 331,) likewise opposed, for very extensive reasons, the possibility of the facts reported on the subject. It will be borne in mind, said he, that the idea of spontaneous combustion arose at a period when all opinions upon the subject were erroneous. What takes place in combustion was only known some seventy years ago, (Lavoisier.) What is necessary for the combustion of a body, has been known only forty years, (Davy.)

Since the occurrence of the case of Millet, of Rheims, to the present time, some forty-five or forty-eight cases have occurred which are alike in—1st. Always having taken place during the winter season; 2d. The persons attacked have all been drunkards, drunk at the time; 3d. They have most generally happened in countries where rooms are heated by open fire-places, and furnaces of charcoal, in England, France, and Italy; in Russia and Germany, where stoves are principally used, deaths from

spontaneous combustion are very rare ; 4th. There have been no eye-witnesses to the combustion ; 5th. No physician, amongst all those who have attempted to explain these cases, has seen one ; 6th. There is no information as to the quantity of combustible matter consumed ; 7th. Some time has always elapsed from the commencement of the combustion until the body has been found consumed. M. Liebig also argued with great power upon the principal details reported as connected with spontaneous combustion, and upon the explanations given of them. He also demonstrated clearly the error into which the partizans of the spontaneous combustion theory have fallen. The arguments which they employ being deduced, contrary to all logical rules, in the cases in point ; death and the destruction of the body, the cause of which is unknown, being assumed as proof of the truth of the assumed cause. These are explained by the fact of the possibility of spontaneous combustion, the existence or possibility of which is proved by the same cases. This discussion we are disposed to believe does not admit of refutation, and gives a fatal blow to the doctrine of spontaneous combustion.

The trial, which occupied the greater part of the month of March, 1850, concluded by the condemnation of J. Strauss, as the murderer of the Countess of Goerlitz, to perpetual imprisonment. The journals have recently published that he has made a full acknowledgment of his crime. He admitted that going into the chamber of the Countess upon some household duty, and finding no one, he could not, upon seeing that the secretary contained money and articles of value, resist the temptation to steal. The Countess having discovered him in the act, he seized and strangled her with some difficulty ; then having placed the body in an arm chair, near the secretary, he surrounded it with combustibles, to which he set fire for the purpose of concealing his crime.—[*Buffalo Medical Journal*.

Phosphate of Lime in Scrofula and other depraved states of the System. By W. STONE, M. D., Prof. of Surgery in the University of Louisiana.

In the July number of the reprint of the London Lancet, there is an article by Beneke, entitled the Physiology and Pathology of the Oxalate and Phosphate of Lime, and their relation to the formation of cells. The conclusions of the author are based upon careful chemical research and results from the use of the remedy. His researches show that in man, as well as in vegetables and inferior animals, Phosphate of Lime as

well as albumen and fat is absolutely essential for the formation of cells, and he considers that many of the pathological states of the system depend upon a deficiency of this salt. The affections in which it is advised are ulcerations dependent upon a general dyscrasia, and not a mere local affection; infantile atrophy; in those suffering from rickets and consequent diarrhea and tuberculous diseases, particularly of the lungs in the early stages. I was favorably impressed with the article, and being encouraged by the results of the Practice, I am induced to relate a few cases by way of calling the attention of the Profession to it, believing great improvement may be made in the treatment of diseases dependent upon vice of nutrition.

CASE I. Slave Bob was admitted into my Infirmary early in July, with a disease of his nose. Two large fungous growths, one on each side of the nose, barely separated by a strip of sound skin in the centre, of about one inch in diameter, extended nearly to the corners of the eyes. The cavities of the nose were filled by a similar growth, and the disease was making its appearance in the roof of the mouth. His general appearance was bad, and not unlike that of a dirt eater. He complained of pains in different parts of the body, but not much at the seat of the disease, and he had an indolent swelling on one of his feet which finally softened down, and on being opened, discharged a thin matter and broken down tissue, leaving an ill-conditioned ulcer. I had to rely upon him for his history, which must necessarily be imperfect. He said the disease commenced four months previous in the nasal cavities and gradually made its way through. An examination showed that the bones had been absorbed—the mass bled freely, and upon pressure a thick cream-like pus appeared, and some of it resembled softened tuberculous matter. Pulse feeble and frequent, and digestion bad. I do not know what particular treatment he had been under, but he appeared to be slightly under the influence of mercury, and I put him upon the use of the hydriodide of potass—cut off the fungus externally, and extracted as much as was practicable from the nasal cavities with polypus forceps and used a lotion of the sulphate of copper. No perceptible improvement followed, and on the first of August I put him upon the use of cod liver oil, but his digestion continued bad, had acid eructations which he thought was worse when he took the oil. The phosphate of lime was added eight grains three times a day, and he soon began, for the first time to improve. His color began to return—the local disease began to assume a better appearance. Local treatment was disregarded, and the oil and phosphate of lime has been continued up to this time.

His color is now of a shining healthy black. The fungus is even with the surrounding skin. Cicatrisation is taking place, and the fungus has disappeared from the nasal cavities, so that he breathes quite freely through them. Those having confidence in cod-liver oil, may attribute the favorable change to it alone, but I would say that no favorable change took place until the lime was given, although it had been given sufficiently, I think for a fair trial. The oil may supply one deficiency, and the lime another; but my object is not to theorise, but to draw attention. Bleeding, leeching, cups and gum water, on the one hand, and tonics, stimulants and opium on the other, are sufficiently well understood, but I believe that Chemistry is yet to assist us and enable us to relieve many of those undefinable maladies that depend upon vices of nutrition, either hereditary or acquired, which cut off so many before the natural decay of the system takes place.

CASE II. Miss —, aged 24, had been in delicate health for some time, without suffering from any particular disease. In May last, a dry cough commenced, and loss of appetite followed, etc. But to make it brief, as it is but a common case, I saw her about the middle of June, and found the upper part of both lungs filled with tubercles, in some places beginning to soften. Her cough was almost incessant, expectoration slight, consisting of viscid mucus, streaked with pus, and occasionally with blood; pulse a hundred and twenty, much emaciated, and her menses had ceased. She had fever in the evening, and exhausting night sweats. I ordered cod-liver oil, together with a soothing cough mixture, for temporary relief, and to procure rest, which she could not get without. This course afforded some relief, but the appetite did not improve, and I could not say that any marked improvement had taken place. About the first of July I gave the phosphate of lime, in addition to the oil, and in a short time there appeared to be some improvement in the appetite; the sweats began to leave, and her color gradually to return. The same course has been continued up to the present time, and she says she feels better than she has for two years. Her cough is in a great manner gone; she has gained considerable flesh, and has for the last two periods menstruated more naturally than for two years previously. There could be no doubt as to the precise nature of this case, and I am free to allow full credit to the oil, but I am confident that the lime was equally useful. The patient, who knows nothing of the medicine, spoke of its good effects. If the theory upon which its beneficial effects are based is correct, it ought to be an admirable assistant to the oil. I do not pretend that this patient is effectually cured, but it must be admitted that the result of the treatment is highly

encouraging. It was a case of unmixed phthisis, that might have been expected to terminate in the course of a few months.

CASE III. A child of Mr. W., aged about seven years, had been laboring under a derangement of his bowels something over a year, and had been treated by very excellent physicians, with only temporary benefit. I saw him first in July, during one of his bad spells, as it was termed. I will not be so tedious as to give all the symptoms of the case, or the treatment that had been pursued. Suffice it to say that he was emaciated very much, but there was no evidence of any serious organic lesion, and no decided appearance of a scrofulous taint. Dyspeptic diarrhea is a term as applicable as any one term to his case, though at times he seemed to digest tolerably well; but there was no assimilation or appropriation of his food. He was at first put upon the use of hydriodate of potash, in an infusion of gentian, without any change, and finding that he suffered from acidity, and added the phosphate of lime in doses of six or eight grains, three times a day, which he is still using. I saw him a few days since, and learned from the parents that he had had no new attack, and that his bowels had been steadily improving, and is gaining flesh and strength rapidly. The parents, who are highly intelligent, attribute most of the benefit to the phosphate of lime.—[*N. O. Medical Register*.

A modification in the Operation for Hare-lip.

M. Coste, chief surgeon of the Hôtel Dieu of Marseilles, has been endeavoring to devise the means of obviating the ugly notch which too often remains after the operation for hare-lip. M. Coste states that the modification proposed and practised by M. Malgaigne is only applicable in double hare-lip, and that in the simple deformity M. Malgaigne's method produces an unsightly prominence.

The author in simple hare-lip, (which lies generally on the left side,) has succeeded in avoiding the notch altogether, by cutting a horizontal flap in the red part of the lip on one side, and a kind of half mortise on the other. In paring the margins of the fissure, he takes off more substance than is generally done; the flap and mortise are well secured by twisted suture, and by one of the diminutive spring-forceps called "serre fines;" one transverse needle is placed a little higher up, and no application whatsoever made, so that the progress may be more accurately watched. M. Coste has thus succeeded, upon a little boy, twelve years of age, in completely avoiding the above-mentioned notch.—[*London Lancet*.

Remarkable Migrations of a Pin and Needle through the Body of a young lady. By NAPOLEON B. ANDERSON, M. D., of Louisville, Ky.

On the 20th of April, 1849, Miss Catharine M——, æt. 19 years, in a fit of laughter, accidentally swallowed a large brass pin and a medium sized needle. No pain attended the passage of these bodies into the stomach, nor was any felt until after the expiration of about the third week, at which time a warm, pricking sensation was first felt in the cardiac orifice of the stomach, which position it maintained for the space of three months, when it gradually changed, and seated itself in the lower lobe of the left lung. In this situation it remained for some nine months, without any disturbance to the organ of respiration in which it was felt, with the exception of occasional cough and slight hemoptysis. During this period, the pain gradually moved to the glenoid cavity of the scapula, and was experienced at the insertion of the deltoid muscle, in which situation considerable pain was the result of elevation or rotation of the arm. From this point it moved to the arm pit, when the arm had to be carried horizontally, and no elevation, rotation, adduction, or abduction, could be performed without excruciating pain; the inner part of the arm turning very black, from the infiltration, I suppose, of blood into the surrounding parts. Pressure upon the parts, produced no material change in coloration, nor was there any unusual amount of sensation or numbness in any part of the discolored portion, with the exception of the region in which these foreign bodies were situated. The arm remained in this condition, with no material changes, until December, 1850, when the pain and uneasiness moving from the arm-pit, towards the articulation of the ulna and radius with the humerus, settled in the belly of the biceps flexor muscle, forming there a dark spot the size of a half dollar, and very sensitive to the touch. An emollient poultice was applied for twenty-four hours, when fluctuation indicated the use of the knife. A quantity of bloody pus was discharged, and the needle and pin were extracted from two different apertures, about half an inch apart. The pin was dark, but the needle was bright, and had undergone no material change. Alteratives were used, and in ten days from the extraction of the bodies, the lady had perfect use of her arm, and has continued to do so ever since.

During the period, from the swallowing of these substances until their removal, the constitution was not disturbed in the slightest degree, except the cough and hemoptysis spoken of; and this continued only as long as those articles were passing

through the lungs, after which the symptoms disappeared. The lady underwent no treatment during their migration from the mouth to the arm, with the exception of a purge when she first swallowed the articles, and anodyne embrocations afterwards.

These pointed bodies appear to have travelled side by side over the entire route from the mouth to the point at which they were extracted, and must, in their course, have passed through the stomach, diaphragm, lung, pleura, among muscles and bloodvessels, before reaching the parts from which they were extracted. The points of each article presented at the incision made, and must, I suppose, have thus passed the entire distance.—[*Western Journ. of Med. and Surg.*]

Water Dressings.

We condense from the *Bul. Gén. de Thérap.* the following notice of a Thesis read before the Parisian Faculty of Medicine, upon the therapeutical effects of water, by M. Amussat, Jr.

The author first discusses the temperature at which water is most conveniently employed as a surgical remedy. He thinks that cold water should be rarely used, but in the majority of cases it should be luke-warm. According to M. Amussat, luke-warm water will be most advantageously used in simple inflammations, erysipelas, burns, ulcers, gangrene, simple and contused wounds, gun-shot wounds, &c. M. Amussat makes three divisions of the modes of applying water, by dressing, irrigation and immersion. There are, he says, three objects to be attained in water dressings—1st, the pus should be allowed to pass out freely; 2d, the moisture should be continuous; 3d, all evaporation should be prevented, in order that the part may not be chilled. He fulfils these indications by means of four pieces of cloth. The first piece is pierced with holes to allow the pus to escape; the second is a common piece of linen or cotton cloth moistened in warm water—this is to absorb the pus; the third piece is tinder prepared without saltpetre or gun-powder; the fourth piece may be made of any impermeable tissue. If there is an abundant suppuration, the dressing should be frequently changed. The dressing should not be discontinued suddenly, but by degrees. M. Amussat thinks this dressing far superior to poultices, but less powerful than irrigation or immersion.

Magnesia as an antidote for Poisoning with Copper.

M. Roncher, in an article upon this subject, in the *Gazette Médicale de Strasbourg*, draws the following conclusions, from experiments he has made :

1st. That calcined magnesia will arrest entirely the symptoms of poisoning with copper, if it be administered sufficiently soon after the copper has been taken.

2d. That the dose of magnesia necessary to neutralise the salt of copper, is 8 grammes of magnesia to 1 of sulph. copper.

3d. That as magnesia prevents the formation of the greenish, soluble salt, it is quite probable that it will act as an antidote to all the salts of copper.—[*Revue Médicale*, Aug. 1851.]

Solution of Lac a Substitute for Collodion.

As a substitute for collodion, Dr. Mellez recommends a solution of powdered shell-lac in highly-rectified spirit. The solution when cold, becomes gelatinous, and is used by joiners for polishing furniture. Spread on taffeta or linen and applied to the skin, it shows all the properties of collodion. It is impenetrable to the air, water, fat, and the organic secretions ; it does not irritate the skin, and can be employed instead of dextrin for fractures. Wounds heal remarkably quick when dressed with this solution.—*Lond. Pharm. Journ.*

Removal of Carbonic Acid from Cellars and other places where it has accumulated.

Aubergier proposes to remove carbonic acid from cellars and other places, where, from fermentation or other causes, it has accumulated, by sprinkling about *liquor ammoniæ*; this combines with carbonic acid to form carbonate of ammonia, and fresh air rushes in to fill up the space produced by the condensation of the acid.—[*Ibid.*]

Iodine.

An account has been published by M. Chatin of a series of experiments on animal and vegetable substances, with a view to ascertain the amount of iodine that enters into their composition. All vegetables appear to contain more or less of this element, and particularly water-cresses. Wine is much more rich in iodine than water, milk richer in the element than wine, and asses' milk more rich in this respect than that of cows. Eggs contain a large portion of iodine. A hen's egg weighing an ounce and a half, was found to contain as much iodine as a quart of milk from the cow.—[*Ibid.*]

Miscellany.

Reply to "REMARKS" contained in the April number of the Western Journal of Medicine and Surgery, over the signature of "B."

Having just returned from Europe, after an absence of five months, I find that my Report of "a case of Urinary Calculus, attended with peculiar circumstances, and treated by Lithotrity," which appeared in this Journal last April, has been made the subject of criticism in the Western Journal of Medicine and Surgery (published at Louisville, Kentucky,) and that these Strictures elicited a reply from an esteemed friend, (in the July number of this Journal,) which has, in its turn, been followed by a rejoinder in the Western Journal, of August. A sense of duty to myself as well as to science, demands of me a sacrifice of feeling while I obtrude upon an enlightened profession the following pages.

With a view to render my comments upon the "Remarks" of my critic more intelligible, I will append them to each of his paragraphs in the order in which these appear :

"REMARKS.—This certainly presents 'certain peculiar features,' both in anatomy and Surgery, and we are utterly at a loss to understand some of them. The fault may be ours, but there can be no wrong in stating the difficulties.

"1st.—It is somewhat remarkable that a phymosis should have created so great a resisting power in the prepuce as to dilate even the ureters. This strikes us as a very remarkable peculiarity. The wonder is increased considerably when we find that notwithstanding the ureters were thus dilated so as to permit the passage of a stone of novel dimensions, the urethra, which should have synchronised liberally in the dilatation of the ureters, was so little inclined towards anything of the kind, that it stopped the stone which had fallen through the ureter! The extravagant dilatation of the ureter is inexplicable; but, assuming the claim as a fact, the dilatoriness of the urethra is rather marvellous."

The reader will perceive that, according to this paragraph, I am charged with having alledged that the phymosis "created so great a resisting power in the prepuce as to dilate even the ureters," and that "notwithstanding the ureters were thus dilated so as to permit the passage of a stone of novel dimensions, the urethra, which should have synchronised liberally in the dilatation of the ureters, was so little inclined towards any thing of the kind, that it stopped the stone which had fallen through the ureter." Such is the meaning of the

paragraph, bereft of the epithets, "remarkable," "wonder," "novel," "extravagant," "inexplicable," and "marvellous."

Now, if the reader will look over my Report, he will find no foundation whatever for such a charge. No where have I said or inferred that the resisting power of the prepuce was so great as to dilate the ureters and to permit the passage of the stone. It is distinctly stated in my first paragraph, that the patient was thirty years of age when I operated upon him, and that he was but twenty when circumcised by Dr. Banks; thus leaving him entirely free from any phimosis or difficulty in urinating for a period of ten years before I saw him. Having stated that the phimosis had been removed ten years before I saw the patient, and more than nine years before the entrance of the stone into the bladder, my Report cannot be so perverted as to make it appear that I ever expressed or even entertained the views to which "B." objects. I again beg the reader to refer to my report.

It is true that I found a calculus in the bladder, and that I reported the patient's narrative as "establishing conclusively" (in my opinion) "that he did know the precise moment at which the stone came into the bladder." The fact that the stone appears to have come down the ureter, cannot be accounted for by "B." otherwise than by supposing this duct to have been dilated by the resisting power of the prepuce, and he therefore endeavors to make it appear that such was my representation of the case. It does not seem to have once occurred to the critic that a calculus may be formed in the pelvis of the kidney, pass into the ureter, and be gradually propelled forward by the urine, thus dilating the ureter in its progress. Nor does he appear to have ever heard of a case in which a calculus after thus passing down the ureter, had been refused admittance into the urethra! He forgets that whereas the ureters are comparatively passive in their resistance to dilating agents, the urethra is essentially different—and also that the column of urine emerging from the kidney would necessarily press forward a body situated in the ureter until it entered the bladder, if possible; but that the calculus might remain in this reservoir without being necessarily forced into the urethra.

Again: according to "B." the "urethra should have synchronised liberally in the dilatation of the ureters." This is a new doctrine, for the originality of which the critic is unquestionably entitled to credit. A case in which the urethra "would synchronise" with the ureters in a dilatation consequent upon the descent of a renal calculus would indeed be "remarkable," "extraordinary," and "marvellous." Whether or not the urethra should "synchronise liberally

in the dilatation of the ureters" occasioned by phimosis, is not the question before us, since in this case as reported, no phimosis had existed for more than nine years before the period at which the calculus is alledged to have passed into the bladder.

"2d.—The statement of the patient that he "heard something drop," and therefore knew the exact moment of the entrance of the calculus into the bladder, seems to have made a profound impression upon Professor Dugas, for he unhesitatingly gave credence to the statement. The patient may be excused for thinking that a calculus could fall from the ureter into the bladder, but we have some difficulties in our faith. The ureters enter the *bis fond* of the bladder, very obliquely, and a stone would have to fall *up* in falling from the ureter into the bladder. And then when we remember the pathological truths of Mr. Aldridge, which seem to show that the oxalate of lime is not secreted in the kidneys, when we remember that there is no kind of evidence that the ureters in this case were dilated even in the slightest degree, and that the passage of a mulberry calculus through the ureter would have made a man feel a multitude of other things besides the falling of the calculus, we must remember that we have before us what may be called the difficulties of faith."

I have no right to complain that "B." has not as much faith in the statement of the patient as I have, who know him to be an honest and intelligent gentleman. If all the circumstances detailed in the second paragraph of my report are not deemed by "B." sufficient to establish "conclusively the facts that he did know the *precise moment* at which the stone came into the bladder," the fault is not mine. I will not stop to correct an error of quotation, nor to return thanks for "B.'s" sapient anatomical and philosophical suggestions. I am still, however, credulous enough to believe that a calculus may emerge from the ureter suddenly and with sufficient force to occasion just such a sensation as "that of a buck-shot allowed to drop into a bag." I do not know any form of expression by which my patient could have imparted a more accurate idea of his sensation, and it is therefore I gave his own language.

Will "B." "remember" that the patient was relieved of the phimosis nine years before he experienced this sensation; that "from that time his health improved rapidly, but he continued subject to occasional paroxysms of severe nephritic pains, which now became confined to the left side;" and that this state of things continued until the middle of April last (1850), when, *although in good health*, he felt a calculus drop into the bladder?" If, as intimated by "B.," the calculus did not come from the kidney, why did the patient never ex-

perience any symptom of it in the bladder, until the day upon which he felt the sensation in question? Why were all the symptoms of calculus in the bladder so well marked *after* the sensation?

"B." experiences "what may be called the difficulties of faith," when he "remembers the pathological truths of Mr. Aldridge, which *seem* to show that the oxalate of lime is not secreted in the kidneys," when he "remembers that there is no kind of evidence that the ureters in this case were dilated even in the slightest degree" and, finally, when he remembers "that the passage of a mulberry calculus through the ureter would have made a man feel a multitude of other things besides the falling of the calculus." But "B." should also "remember" that whatever the "truths of Mr. Aldridge may *seem* to show," Prout, Bird and Brodie, to mention no others, recognize the existence of calculi of oxalate of lime in the kidneys. Brodie says that "a patient may void one of these calculi and never void another, or he may void a second after the lapse of many years. In one instance, however, in examining a body after death, I discovered as many as five or six *in one kidney*." (p. 225, from Chelius, v. 3, p. 226.) One fact of this kind is worth more than a thousand conjectures or theories. As to the "multitude of other *things*" that "B." thinks the patient ought to have felt "besides the falling of the calculus," I would only remark that all surgeons of experience know that the sufferings occasioned by the descent of stones from the kidneys or by their escape from the ureters into the bladder vary infinitely and that the passage of even rough ones is sometimes effected without any pain whatever.

"3d.—We feel some difficulty about the dimensions of the calculus. We have seen between two and three hundred specimens of calculi, and have heard from various other collections, and we have neither seen nor heard of any calculus, except this one in Georgia, that was just one inch in length, and a half inch in thickness. These dimensions are such a wide departure from that uniformity of proportion found in calculi, that we think there must be some mistake in Professor Dugas's measurements. There must be a want of accuracy. Did it not strike the Professor that the growth of his specimen was altogether too rapid for a case of oxalate of lime calculus? There seems to us a wonderful celerity in every branch of this case."

In reply to the "difficulty" which "B." feels "about the dimensions of the calculus," I will merely suggest that it is not very surprising that he has not seen calculi of all the dimensions they may assume. By a coincidence, which may appear to "B." somewhat singular,

Dr. H. F. Campbell, of this city, reported in July last, a case of Lithotomy in which the dimensions of the calculus, (which was of oxalate of lime), approximate very closely to those indicated in my case. "On measurement, it was ascertained to be of the following dimensions: longest diameter, one inch and three-tenths; shortest diameter, seven-tenths of an inch." (see South. Medical & Surgical Journal, July, 1851.) Prof. Gross, of Louisville, the weight of whose testimony will not be denied by "B.," in his recent and valuable work upon the Diseases of the Urinary Organs, thus expresses himself:

"Most urinary calculi originate in the kidneys, from which they descend into the bladder." (p. 341.) In relation to their form, he says: "Vesical calculi are commonly of an oval form, but occasionally they are round, spherical, or even cylindrical. Other varieties of form are sometimes seen, as the conical, pyriform, cubic, triangular, pyramidal, gourd-like, polygonal, and the tetrahedral. Sometimes the concretion is thin and flat, like a coin, lenticular, semilunar, or in the shape of a mushroom, a kidney, a mulberry, a bean or a heart. Again, it may be large and bulbous at the extremities, and narrow at the middle, like a dumb-bell. * * * *In fact, there is no end to the grotesque appearance of these foreign bodies.*" (p. 353.) And yet, "B." objects to my case because "the dimensions are such a wide departure from the *uniformity* of proportion found in calculi."! I leave it with the reader to determine whether it is more probable that there was a "mistake" in my "measurements" than that "B." has not seen stones of all the above forms and dimensions.

With regard to the time occupied in the growth of my "specimen," I have not expressed any opinion, nor do I know the source from whence "B." has derived his belief that it was "altogether too rapid for a case of oxalate of lime calculus." "There seems to us a wonderful celerity in every branch of this case" of fault-finding.

"4th.—The calculus in this case was 'oxalate of lime,' and the stone was crushed with Heurteloup's '*brise pierre*,' at two sittings, on two consecutive days, and the fragments were allowed to be passed off during the night. This is certainly the most remarkable achievement yet effected by Heurteloup's instrument. It is enough to excite the envy of Civiale, and put an end to the lateral operation. If a calculus of oxalate of lime, one inch long, and a half inch thick, can be utterly crushed in two sittings, in two successive days, so that no vestige of it is left, what apology can there be for cutting instruments for lithotomy? We have seen various efforts with Heurteloup's instrument, and have been sometimes surprised with the result,

but this success in breaking down, in two sittings, a stone of oxalate of lime, of the size of the one recorded by Professor Dugas, certainly takes the lead of all achievements we know of in lithotrity. We have seen vesical stones of oxalate of lime removed by the lateral operation after lithotrity had failed, and in which the most persistent efforts with the drill for many sittings had failed to make any more impression than if it had been used on a piece of Syenite. But if the improved apparatus of Heurteloup can break up at two sittings, a mass of oxalate of lime, and remove it entirely in two days, lithotrity is making rapid strides, and M. Roux is an accredited prophet, when he says: 'lithotrity has assumed her function, and no surgeon hereafter will attain sufficient experience to reach the highest degree of adroitness in lithotomy.'

"We suppose these new claims of lithotrity will come before the American Medical Association, and if they receive the endorsement of that body, we may expect to see renewed evidences of the envy felt by European surgeons for the rising reputation of American Surgery, and we shall hear them again denouncing American surgeons for a proneness to exaggeration. B."

The two last paragraphs of "B.'s" "Remarks" fully illustrate the spirit in which they were indited. Their unworthy imputations and insinuations cannot provoke me to any special notice of them. Regarding personalities in scientific controversies as decidedly in bad taste, I have endeavored to avoid noticing any thing that might savour of unfairness on the part of my critic.

When I published my case, I must confess that I did not anticipate for it so much notoriety. I thought that it presented "certain peculiar features" not unworthy of record, and endeavored with as much brevity as possible to draw up a faithful report. The features I deemed most interesting, are: 1st, the existence during twenty years of a phimosis attended with an almost complete closure of the preputial orifice, and which seriously implicated the general health of the patient before he applied for surgical relief; 2d, the recurrence of occasional attacks of nephritic pains during ten years after circumcision, which pains finally became confined to the left side; 3d, the accurate indication by the patient of the "precise moment at which the stone came into the bladder; 4th, the passage of the stone into the bladder just after micturition; and lastly, the circumstance that the stone entered the bladder *three months* after the last nephritic attack. But it had not occurred to me that these peculiarities presented any thing incredible, nor even extraordinary. I merely regarded them as furnishing an interesting illustration of facts, which, although already within the domain of science, are not of very fre-

quent occurrence. I have not had leisure to look over authorities on the subject, but I do not recollect an instance in which the knowledge of the precise moment at which the stone came into the bladder, is so well established. Such may, however, be on record, and yet the addition of this case can do us no harm.

That the stone came into the bladder just *after micturition* is not surprising; but it would be interesting to know whether such is *usually* or only *rarely* the case. It may therefore be well to direct attention to this point more forcibly than has heretofore been done. The fact that the distention of the bladder occasions a compression of the portion of the ureters engaged between its laminae, whereas this compression ceases with the subsidence of distension, would lead us *à priori* to infer that calculi rarely, if ever, enter the bladder when filled with urine, and that they would be most apt to do so just after micturition, when the flow of urine through the uncompressed orifice of the ureter would exert most favorably its propelling influence upon the stone. It is doubtless owing to this compression of the vesical extremity of the ureters that calculi are so often arrested at this point and that they sometimes remain fixed here. In the case before us, it would seem probable that the calculus had remained at this point about three months,—the period which elapsed from the last nephritic attack to the time at which the stone is indicated to have passed into the bladder.

With regard to the operation by which the patient was relieved, I did not, nor do I now see in it any thing miraculous, or in anywise calculated to provoke the envy of our transatlantic brethren. It is certainly flattering, however, to find that any one, even in our own country, considers it a "remarkable achievement."

L. A. DUGAS.

P. S. The subjoined letter from Dr. Banks, has just been received, and may perhaps satisfy "B." that the patient has been entirely relieved of his calculus.

L. A. D.

"GAINESVILLE, Oct. 23, 1851.

"*Dr. Dugas:* Dear Sir,—I have not been able to see Mr. Bell since the receipt of your letter, as he lives about eighteen miles from town. I saw him shortly after he returned from Augusta, and he then complained of a slight soreness in the neck of the bladder. I have seen him several times since then, and he has uniformly expressed himself as being entirely relieved from the disease. His general health and appearance is much improved. I saw his brother on yesterday, and he confirms the above statement as to the entire restoration of John L. Bell's health.

"Truly yours,

RICHARD BANKS, M. D."

Physicians' Society for Medical Observation of Greene and adjoining Counties.—We take pleasure in recording the organization of a new auxiliary Medical Society, and doubt not that it will prove highly beneficial, as well as agreeable to its members, if kept up with proper spirit. We hope to derive from its influence valuable contributions.

GREENESBORO', GA., Oct. 6th, 1851.

Pursuant to previous notice, the physicians of Greene and adjoining counties, met in Greensboro' on the 1st Monday in October, for the purpose of organizing a Medical Society *exclusively* for mutual improvement, and as auxiliary to the Georgia State Medical Society.

Present:—Drs. J. F. Foster, F. W. Cheney, R. S. Williams, A. H. Randle, W. L. Alfriend, C. M. Park, R. S. Callaway, Samuel Hall, Benj. Rea, Thos. P. Janes, H. H. King, J. E. Walker, D. C. O'Keeffe, R. Olive, R. S. Massey, E. V. Culver.

On motion of Dr. O'Keeffe, Dr. CHENEY was called to the Chair, and Dr. REA requested to act as Secretary.

After a few appropriate remarks by the Chair, explanatory of the objects of the meeting, and urging the importance and utility of medical organization—on motion of Dr. O'Keeffe, a committee of five was appointed by the Chair, for the purpose of drafting a Constitution and By-Laws, consisting of Drs. Randle, Alfriend, Callaway, Rea and O'Keeffe. The committee retired and after a short interval, signified through their chairman that they required further time for the discharge of the duty assigned them.

Dr. Alfriend wished to see a uniform Fee-Bill adopted by the Society, and urged upon the meeting its importance and advantages. Dr. O'Keeffe opposed the measure, on the ground of its general adoption being impracticable, and as exerting an injurious influence against the prosperity and perpetuity of the Society. Dr. Cheney opposed it for the same reasons.

On motion of Dr. Randle, the vote of the meeting was taken respecting it, and decided that the Society recognize no fee-bill.

On motion, the meeting adjourned to meet again on the third Monday in October.

F. W. CHENEY, M. D., Chairman.

B. F. REA, M. D., Secretary.

—
GREENESBORO', GA., Oct. 20th, 1851.

The physicians met, pursuant to adjournment, and organized by calling Dr. FOSTER to the Chair, and Dr. JOHN E. WALKER to act as Secretary.

The minutes having been read and confirmed, Dr. O'Keeffe proceeded to read the Constitution, By-Laws and Rules of Order, drafted by the committee appointed for that purpose at the previous meeting. Each section having been considered and voted on separately, they were finally adopted after considerable discussion.

On motion of Dr. O'Keeffe, the meeting resolved itself into the "*Physicians' Society, for Medical Observation of Greene and adjoining Counties, Ga.,*" which is to be the name and style of the society.

On motion, the officers of the meeting were requested to act as officers of the society until an election be had, which being held, the following gentlemen were elected :

James F. Foster, M. D., Greensboro', President ;

W. L. Alfriend, M. D., White Plains, 1st Vice-President ;

F. W. Cheney, M. D., Penfield, 2d Vice-President ;

D. C. O'Keeffe, M. D., Penfield, Secretary.

J. E. Walker, M. D., Greensboro', Treasurer.

On motion of Dr. H. H. King, the Society adjourned to hold its first regular bi-monthly meeting on the first Monday in November next.

JAMES F. FOSTER, M. D., Chairman.

J. E. WALKER, M. D., Secretary.

Dr. Isaac Hays' Circular.

SIR—The Committee of publication respectfully submit to you, as a member of the American Medical Association, the following statement :—

The whole amount received from the assessment for 1851 has been about six hundred dollars, which, with the balance in the treasury at the last report (four hundred dollars), makes one thousand dollars.

It is estimated that the cost of vol. IV. will be about seventeen hundred dollars ; the expense of printing the Reports being one thousand dollars, and of the prize essay, with the necessary illustrations, nearly seven hundred dollars.

This last, which was awarded the prize of one hundred dollars, and which the association ordered to be published in the Transactions, is a paper of great merit, exhibits extensive research, and is illustrated with numerous beautiful drawings, a number of them colored. The publication of this essay will do credit to the Association, and tend to elevate the scientific character of the American Medical Profession. But the committee are without the means for that purpose, and they appeal to the Association to furnish them. A large number of copies of the three volumes already published remain unsold, and, if the members will complete their sets, and use their influence to extend the sale of these volumes, the required sum may be readily raised.

The committee call the attention of the Association to the terms upon which the published volumes are now furnished to members, or to societies which have been represented in the Association.

Either of the first three volumes separately (in paper covers)				\$1 50
A complete set in three volumes (paper covers)				4 00
do.	do.	do.	(cloth)	5 00
Single copies of vol. iv. (to permanent members)				2 00
Three	do.	do.	do.	5 00

In all cases the amount must be remitted to the Treasurer of the Association, Isaac Hays, M. D., Philadelphia.

Your earliest possible attention to the above is earnestly solicited, to avoid great delay in the publication of the forthcoming volume of the Transactions.

ISAAC HAYS, M. D.,

Chairman of the Com. of Pub. Am. Med. Ass., and Treasurer.

PHILADELPHIA, August 1, 1851.

American Medical Association. Prize Essays.—At the meeting of the American Medical Association held in Charleston, S. C., in May last, the undersigned were appointed a Committee to receive and examine such voluntary communications on subjects connected with medical science, as individuals might see fit to make, and to award a prize to any number of them not exceeding five, if they should be regarded as entitled to such a distinction.

To carry into effect the intentions of the Association, notice is hereby given, that all such communications must be sent, post-paid, on or before the first day of April, 1852, to George Hayward, M. D., of Boston, Mass. Each communication must be accompanied by a sealed packet, containing the name of the author—which will not be opened unless the accompanying communication be deemed worthy of a prize. The authors of the unsuccessful papers may receive them on application to the committee at any time after the first of June, 1852; and the successful ones, it is understood, will be printed in the Transactions of the Association.

GEO. HAYWARD, Boston.

J. B. S. JACKSON, “

D. H. STORER, “

JACOB BIGELOW, “

USHER PARSONS, Providence, R. I.

Boston, Aug. 20, 1851.

An Example of Ancient Superstition.

To the Editor of the Stethoscope:

ACCOMACK C. H., Va., Aug. 13, 1851.

DEAR SIR.—I send you for publication the following curious scrap of antiquity, which I met with some months ago, while searching the ancient records of this country. It has seemed to me to deserve a place in a medical journal, as a very apt illustration of a medical superstition, among the most remarkable of those with which the history of our science abounds.

Those who have paid attention to the history of medico-legal science are aware, that in former times and for a long period, it was a belief universally entertained, that the wounds of a murdered man would bleed afresh, hours and days after death, on the body being touched by the murderer ; or that some other change, not less wonderful, would occur in the condition of the corpse. This superstition being received as an undoubted truth, it was very naturally turned to account in judicial investigations, for the purpose of detecting the guilty and acquitting the innocent. Dr. Dunglison, in his work on Physiology, refers (in the chapter on Sympathy, &c.) to several instances in which this kind of ordeal was resorted to ; and others may be found in Beck's Medical Jurisprudence, in the chapter on Persons Found Dead. Sir Walter Scott, who has made one of the leading incidents of " St. Valentine's Day " turn on the practical application of this test, in a case of murder where the other evidence was defective, denominates it the "*bier-ordeal*," or the "*trial by bier right* ;" and speaks of it as having been " often granted in the days of our Sovereign's ancestors, approved of by bulls and decretals, and administered by the great Emperor Charlemagne in France, by King Arthur in Britain, and by Gregory the Great, and the mighty Achaius, in this our land of Scotland."

The confidence in the proof thus elicited seems to have been great, inasmuch as it was regarded to be " the pleasure of Heaven, by some hidden agency which we cannot comprehend, to leave open this mode of discovering the wickedness of him who has defaced the image of his Creator." This trial by " bier-right," in truth, originated in the same notions of supernatural agency and direct divine interposition which gave birth to the ordeals of fire, of water, and of battle. They were all regarded in the light of appeals to Heaven for the discovery of truth.

The facts of the case, of which the old records of our county court furnish the history, are briefly these : On or about the 12th of January 1680, one " Mary, the daughter of Sarah, wife of Paul Carter," gave birth to an illegitimate child, which was born alive, but died soon after its birth. It was buried the next day by Paul Carter and his wife in an old house, where it remained until the end of February, when it was removed to the garden. The parties above named, who were the only persons present at the birth and burial of the child, testified that they endeavored to preserve its life, that it received no violence, and that it was decently buried. But suspicions of foul play having arisen, a *jury of twelve matrons* was summoned, according to the custom of those times, to investigate the case. They acted, it seems, in the capacity of a coroner's jury, and their verdict, as copied *verbatim et literatim* from the record, is as follows :

" Wee y^e subscrib^{rs} being sworne to vew y^e body of a dead bastard child confest by Mary y^e daughter of Sarah Carter to be borne of her body w^{ch} said child we caused to be taken out of the ground in the garden where it was very shallow put in then we caused Sarah

the wife of Paul Carter & mother of the said Mary to touch handle and stroake y^e childe in w^{ch} time we saw no alteration in the body of y^e childe Afterwards we called for Paul Carter to touch y^e s^d child & immediately whist he was stroaking y^e childe the black and settled places above the body of the childe grew fresh and red so that blud was redy to come through y^e skin of the childe we also observed the countenance of the said Paule Carter to alter into very much paleness, the childe also appearing to us to be very much neglected in severall respects as to y^e preservacon of such an Infant, & we doe conclude if y^e child had violence it was by y^e throat w^{ch} was very black and continued so through other places w^{ch} were black altered to red & fresh collered to w^{ch} we subscribe our hands this first day of March 167 $\frac{9}{10}$."

[Here follow the signatures of Mary Hill, Margaret Jenkins, Matilda West, and nine other women, and that of "Wm. Custis, coroner."]

In view of the preceding verdict, and the other evidence in the case, the grand jury presented Paul Carter, who was supposed to be the father of the child, for wilful murder, and he was committed to appear for trial before the governor and council at the next general court. As for Sarah, his wife, notwithstanding her having passed so successfully through the ordeal of the touch, some very suspicious circumstances in the other evidence led to a similar presentment against her. The final result of the case, and the degree of importance attached by the general court to the facts stated in the verdict of the jury of matrons, I have no means of learning, as there is no reference to the trial in the records of this county.

Considering the length of time the child had been buried, and the season of the year, the foregoing is one of the most singular examples of this superstition that I have met with. Somewhat analogous to it is the case quoted by Beck from Hargrave's State Trials, in which the body was disinterred thirty days after death and on being touched by one of the accused, "the brow of the dead, which was before of a livid and and carrion color,"—"turned to a lively and fresh color," &c. But much more marvellous phenomena were sworn to have been observed in this case, viz: the sweating of the brow, the opening and shutting of the eye, the repeated motion of the finger, and the dropping of blood from it.

At the present day, it seems at first view inconceivable that superstition should ever have obtained such a complete mastery over the popular mind, that persons apparently of good faith were ready on frequent occasions to bear testimony to such facts, and others as ready to believe them. In our own case, above cited, we have the concurrent testimony of twelve persons under oath. Yet this strange superstition, like many others which have long been received and acted on by the ignorant, has an unquestionable foundation in truth. The history of the cadaveric changes wrought by putrefaction furnishes the key to it. It is well known that the blood, though it generally coagulates in the vessels soon after death, is rendered fluid again by putre-

faction, in which condition it may be caused to flow from wounded blood vessels, by any motion or change of position. It is also known that the blood, thus rendered fluid, may escape from incisions spontaneously, in consequence of the developement of gas by putrefaction in the heart and large blood vessels. Devergie, in his description of the phenomena of this "gazeous putrefaction," (*Med. Legale*, vol. i., p. 166,) remarks that thereby "the heart is emptied of blood, as well as the large blood vessels. The decomposed blood is driven into all the superficial veins and the general capillary system; hence the veins of the surface become distinct like bluish lines, visible externally as if those vessels had been injected; hence too, that reddish coloration of all the white tissues, as the cellular tissue, the parietes of the trachea and alimentary canal, of which the external aspect may then simulate the appearance of inflammation." In this passage Devergie gives a very satisfactory explanation of the phenomena observed in the case of the child above mentioned, after its body had been exposed to the air for a certain period.

It is very easy to conceive that in any particular case one or more of these cadaveric phenomena might coincide with the presence or the touch of an individual whom other circumstances might prove to be the murderer; and in an age when the general tendency of the popular mind was towards a superstitious belief in the marvelous, as affording evidence of divine interposition, it would require but few such coincidences to establish a general law, and furnish sufficient ground for constituting an ordeal for deciding upon the guilt or innocence of persons accused of murder. Such is, no doubt, the true history of the origin of this "trial by bier-right."

Craving your pardon for the length to which I have allowed this communication to run,

I am, very respectfully, &c.,

L. S. JOYNES.

Hindu Remedy for Sterility.—The following is copied, merely as a matter of curiosity, from one of the medical works received from India a short time since, and already alluded to in this Journal. Take powder of bidari or anuloka, mixed with honey or ghee, and eat the testes of a goat, roasted and prepared with salt, ghee and long pepper. The preparations of maskulai, with sugar barley and wheat. Eat the eggs of crabs, crocodiles or turtles, properly prepared. Flour prepared with ghee and milk, rubbed on the feet with oil mixed with crocodile's eggs—the flesh of rats, frogs, and the eggs of sparrows. The patient, in the meanwhile, is to drink fresh milk with sugar, honey, and swallow the powder of Swayangupta, with ekuruka.—*Bost. Jour.*

Tea and its Adulterations.—The "Analytical Sanitary Commission," in London, to which reference was made some months since in this Journal, is still pursuing its analyses of various articles of food and drink. The different kinds of tea and their adulterations have occupied a prominent place in its investigations. A very brief sum-

ming up of researches respecting this article is contained in the following extract. Much curious information concerning the culture of the tea plant is contained in the Commission's report in the *Lancet*, which we may hereafter copy.

"The chief points ascertained with regard to *black tea* are—

"1st. That the principal black teas—namely, the Congous and Souchongs, arrive in this country, for the most part, in a genuine state.

"2d. That certain descriptions of black tea, as Scented Orange Pekoe and Caper, are invariably adulterated, the adulteration in general consisting in the glazing of the leaves with plumbago or black lead; the Caper likewise being subject to admixture with other substances, as paddy-husk, Lie tea, and leaves other than those of tea.

"3d. That several varieties of a spurious Caper, or black gunpowder, are prepared, which consist of tea-dust, and sometimes the dust of other leaves, and sand, made up into little masses with gum, and faced or glazed with plumbago, Prussian blue, and turmeric-powder; in some cases these imitations are sold separately, but most frequently they are used to mix with and adulterate the better qualities of Caper—viz. those which are made of tea faced with lumbago only.

"With respect to *green tea* the principal conclusions are—

"1st. That these teas, with the exception of a few of British growth and manufacture, from Assam, are invariably adulterated—that is to say, are glazed with coloring matters of different kinds.

"2nd. That the coloring matters used are in general Prussian blue, turmeric-powder, and China clay, other ingredients being sometimes but not frequently employed.

"3rd. That of those coloring matters, Prussian blue, or ferro cyanide of iron, possesses properties calculated to affect health injuriously.

"4th. That in this country there is really no such thing as a green tea—that is, one which possesses the natural green hue considered to characterize that kind of tea.

"5th. That green teas, and more especially the Gunpowders, in addition to being faced and glazed, are more subject to adulteration in other ways than black teas, as by admixture with leaves not those of tea, with paddy-husk, and particularly with Lie tea.

"That Lie tea is prepared so as to resemble green tea, and is extensively used by the Chinese themselves to adulterate gunpowder tea; it is also sent over to this country in vast quantities, and is employed for the same purpose by our own tea-dealers and grocers."—[*Ibid.*

New Lebanon; its Physic Gardens and their Products.—The beautiful valley of New Lebanon, situated about 30 miles east of the Hudson river, in the State of New York, and noted for its attractive watering place, the resort of many pleasure-seeking travellers in the summer months, has long been celebrated for its gardens devoted to the culture of medicinal plants, with a view to the supply of apothecaries, druggists, and others in all parts of the United States. For a long time this

business was solely in the hands of the people called "Shakers," who originated it as a regular pursuit, and who yet are largely concerned. During the past summer, whilst on a visit to the valley of the Hudson, we accepted an invitation from Mr. Henry A. Tilden, to visit his gardens and laboratory situated in the township and village of New Lebanon, where he and his brother conduct an extensive business in the culture, drying and packing of plants, and the preparation of medicinal extracts. The Messrs. Tilden informed us that they have about forty acres cultivated under their immediate superintendence, somewhat in the following arrangement: 9 acres in Taraxacum, 2 in Conium, 3 in Hyoscyamus, 3 in Belladonna, 3 in Lettuce, 3 in Sage, 2 Summer Savory, 2 Stramonium, 2 Burdock, and Dock, 1 Marjoram, 2 Digitalis, 2 Parsley, Poppies and Horehound, 1 Aconite and Balm. The remainder are occupied with Basil, Button Snake root, Blessed Thistle, Borage, Coriander, Feverfew, Hollyhock, Hyssop, Larkspur, Lovage, Marsh-mallow, Marygold, Mugwort, Mountain Mint, Southern Wood, Tansey, &c. The narcotics, especially the Hyoscyamus and Belladonna, require a rich soil, and they exhaust the land rapidly. These last attain a height in many instances of five feet, but in general from three to four. They are liable to be preyed upon more or less, at all seasons of their growth by insects and worms peculiar to each, to such an extent in some instances, as to destroy the crop. Conium maculatum grows spontaneously in all that region of country, having become naturalized. It is seen along the roads, and in fields that have been abandoned for a time, attaining often the height of six feet, and presenting a striking object to the eye, by reason of its subdivided foliage. For this reason, the Messrs. Tilden do not cultivate this plant very extensively, but depend largely on that of spontaneous growth, which they gather from the country many miles around, as far as the Vermont line, and in Massachusetts. It is probable that the Conium obtained in this way is really more active, weight for weight, than the cultivated, being less succulent. We noticed the Valeriana officinalis growing with great luxuriance, and as high as five feet, although its culture has not as yet been much extended. Besides the varieties cultivated, large quantities of indigenous plants are purchased from collectors in the West and South, which are required in their business.

Their factory or laboratory is an extensive, oblong, three storied building, in the basement of which is a powerful steam engine which performs the double duty of propelling the powdering apparatus, and of driving a double acting air pump connected with their vacuum evaporators.

The recent plants intended for extracts are brought to the mill from the gardens, reduced to a coarse pulpy state by a pair of chasers, and subjected to a powerful screw press to extract the juice. This is clarified by coagulation, strained, and the pure juice introduced into the large vacuum apparatus, holding several hundred gallons, where it is concentrated rapidly to a syrupy consistence, at a temperature varying 110°—130°, almost entirely free from the deteriorating influence of the atmosphere. In the construction of this apparatus, they have had

a view to great extent of tubular steam-heating surface, so as to be able to accomplish the very large amount of evaporation their business demands. The finishing apparatus is analogous to the vacuum pan of the sugar refiners. We witnessed the operation in progress with the thermometer standing at 112° F. They make annually about 8000 pounds of extracts from green plants and roots, consisting chiefly of Conium 2000 lbs., Dandelion 2000 lbs., Lettuce 1200 lbs., Stramonium 500 lbs., Butternut 800 lbs., Belladonna 500 lbs., Hyoscyamus 500 lbs., and so on. These extracts in the aggregate according to Mr. Tilden's estimate are derived from about 300,000 lbs. of green material, and require the evaporation of more than 20,000 gallons of juice.

Besides these, a considerable amount of extracts are made from dry materials, both foreign and indigenous as Gentian, Rhubarb, Chamomile, Mayapple, Horehound, Cohosh, etc. They are also about engaging largely in the manufacture of extract of Liquorice from foreign root.

In the powdering department they run burr stones and chasers, and use bolting and dusting apparatus. They powder large quantities of material on contract, besides that for their special business, amounting annually to from 50 to 60,000 pounds.

In the herb department, the quantity of material handled is very large. The plants are brought from the gardens into a large room in the factory building, where a number of girls are employed in picking them over to remove other plants accidentally present, and separating the decayed parts and the stems when desirable. They are then placed on hurdles, and exposed in the drying room till properly desiccated. Two presses are kept in operation, by which 2000 pounds of material are sometimes pressed in a week, and about 75,000 pounds per annum, including near three hundred varieties of plants.

At the time of our visit, thirty men and five girls were engaged in the several departments of their establishment.

When we consider the large amount of extracts of important drugs prepared in vacuo, which are thus thrown into the market to replace the former crude products, obtained by boiling down the juices, etc., in open vessels with a naked fire, according to the old method, we cannot but believe that much good will accrue to the medical practitioner in the increased power of these agents. The Messrs. Tilden have, thus far, been *directly* beneficial to the medical interests of the country. But they have also been indirectly useful by inducing their neighbours, the Shakers, from motives of competition, to adopt the vacuum pan, in lieu of the open boiler, in the preparation of their extracts. We have some few observations to make in reference to the medicine-producing department of this remarkable people, who received us kindly during a hurried visit whilst sojourning in their beautiful valley, but we are compelled to defer them till our next issue.—[*Journ. Pharm.*

Medical Coroners.—Over and over again, the propriety of having all coroners medical men, has been urged upon the appointing powers ;

but all the presumption is that they have too much business with the living, to interfere with old established errors that only concern the dead. In England, the functions of a coroner are discharged with ability, by persons of the best medical preparation for understanding both the laws of the land and those governing organized beings. In France, too, and over the continent generally, to put any other than a physician into that office, would be considered absolutely absurd, and an insult to the people. But how is it in our country, the boasted seat of intelligence? Hardly a coroner among us belongs to the medical profession. In the first place, only one, in a town or city, is at all necessary, and he should be centrally located. The city of London has but a single coroner, Mr. Wakeley, the surgeon; and Westminster another; and yet they have a population of 2,400,000. In Paris, every dead body found, is removed to the dead house, where the coroner calls an inquest. In Boston, its 130,000 inhabitants have three coroners appointed to them. A simplification of this unnecessarily complicated system of rival coroners, where only one, a gentleman of scientific attainments, is required, would be not only economical, but satisfactory, as the truth, in regard to the causes of death under circumstances of suspicion, would be more certainly obtained.—[*Boston Med. and Sur. Jour.*]

Eating Pork.—The New Hampshire Shakers have abandoned pork, as food, and they are not without good reasons for doing so. Moses understood the injurious effects of swine's flesh, which he learned of the Egyptians, and therefore interdicted it in his judicial character. The Egyptian priests, who were both philosophers and physicians, ages upon ages before the birth of the Jewish law-giver, had gained an insight into the constitution of man, which, transmitted through the Israelites, diluted and corrupted as it may be, still exerts a powerful influence on all modern systems of legislation. Whatever was unclean in the Mosaic catalogue of edibles, is still thought to be unfit for human food, with the exception of swine. It is an anomaly that the one article, more abhorred than all others in the Levitical code, should become a favorite dish with us American Gentiles. Scrofulous affections, if not generated, are thought to be aggravated by pork; and the measles has been charged to its use. The hog is omnivorous, and more uncleanly than any other animal domesticated for economical purposes—a fact in itself sufficiently strong to deter the Jews from using the meat. Let those who are possessed of the information, show how much more we suffer from certain cutaneous and glandular diseases than the people of countries where pork is not used for food. We never saw a single swine in the whole of Egypt or Syria. The old prejudice, or the ancient interdiction, appears to influence the public sentiment in those countries. With these views, we doubt not that the Shakers will be gainers in health, and perhaps in longevity, by eschewing pork. Lard oil and stearine, in domestic economy, are invaluable articles; and when the demand for them requires all the swine raised, it will be a happy circumstance for the people.—[*Ibid.*]

Mortality of Intemperance.—At a meeting of the Statistical Society of London, June 6th, a paper was read by F. G. P. Neison, Esq., from which we extract some data, in regard to the longevity of persons who are habitually intemperate. The mortality of this class of persons is shown to be frightfully high. In 6111·5 years of life, to which his observations extended, 357 deaths had taken place; but if these lives had been subject to the same rate of mortality as the general population of England and Wales, the number of deaths would have been 110 only, or less than one-third. At the term of life from 21 to 30, the mortality was upwards of five times that of the general community; and in the succeeding 20 years it was above four times greater. An intemperate person of 20 years of age has an equal chance of living 15·6; one of 30 years of age, 13·8; and one of 40 years, 11·6 years; while a person of the general population of the country would have an equal chance of living 44·2, 36·5, and 28·8 years respectively. The influence of the different kinds of drinks on the duration of life was also shown; beer drinkers averaging 21·7 years, spirit drinkers 16·7 years, and those who drank both spirits and beer indiscriminately, 16·1 years. The average duration of life after the commencement of intemperate habits, among mechanics and laboring men, was 18 years; traders and merchants, 17; professional men and gentlemen, 15; and females, 14 years only. [*Ibid.*]

Substitute for McMunn's Elixir of opium.—The following is extracted from an article by E. Dupuy, New York, in the August number of the Philadelphia Journal of Pharmacy. Opium, 3x.; water, q. s.; alcohol, 95 per ct., $\frac{3}{4}$ iv. The opium is to be made into a thin pulp with water; the mixture allowed to stand in a cool place 48 hours, then transferred into an elongated glass funnel containing filtering paper; a superstratum of water equivalent to the bulk of the whole mass is added. When 12 ounces of liquid have been filtered, the alcohol is added to the filtered solution. The proportion of opium is the same as that in Tinct. Opii of the U. S. Pharmacopœia.—[*Ibid.*]

Death of Sylvester Graham.—Most of our readers in this part of the country are familiar with the name of this individual, who some years since made himself notorious in urging upon the community the system of light and exclusively vegetable diet to which his name was given. He has lately died at Northampton, in this State, at the age of about 50. The Gazette, of that place, states that his health had been gradually failing for the last year, and he had suffered much from rheumatism in his hands and feet. "A post-mortem examination disclosed no disease in the system, which, in the opinion of the medical examiners, was sufficient to produce his death; and the immediate cause of his decease is thought to be the use, contrary to the advice of his physician and friends, in the extreme exhaustion of the system, of Congress water and a tepid bath."—[*Ibid.*]

BIBLIOGRAPHICAL NOTICES.

We are indebted to the publishers for the following works received during our absence, and regret not having room to notice them more at length at present. We hope, however, to be able to do some of them more justice hereafter.

Elements of General and Pathological Anatomy, presenting a view of the present state of knowledge in these branches of science. By DAVID CRAIGIE, M.D., F.R.S.E., &c., &c. 2d edition, enlarged, revised, and improved, pp. 1072. Philadelphia: Lindsay & Blakiston. 1851.

The first edition of this work appeared in 1828, and established the reputation of Dr. Craigie as a faithful observer. The present very much enlarged edition shows that the author has kept pace with the advances of science. By combining in the same work a study of the tissues in both their normal and pathological conditions, much of the monotony which would otherwise attend it is obviated, at the same time that the acquisition of correct knowledge is facilitated. We cannot too highly recommend it.

Special Anatomy and Histology. By WM. E. HORNER, M.D., Prof. of Anatomy, University of Pennsylvania, &c., &c. 8th edition, illustrated with anatomical figures, in 2 vols. Philadelphia: Blanchard & Lea. 1851.

Prof. Horner's Anatomy has been so long and so favorably known to the profession, that it is barely necessary to state that the present is a much improved edition, in order to secure for it the liberal patronage it merits.

A Practical Treatise on the diseases and injuries of the Urinary bladder, the Prostate gland, and the Urethra. By S. D. GROSS, M.D., Prof. of Surgery in the University of Louisville, &c., &c., with 106 illustrations, pp. 726. Philadelphia: Blanchard & Lea. 1851.

The learned author informs the reader that "the object of this work is to present, in a systematic and connected form, a full and comprehensive account of the diseases and injuries of the urinary bladder, the prostate gland and the urethra." So far as we have been able to look into the work, we have every reason to congratulate the profession that it has been prepared by one so well qualified to do justice to it. It is the only American monograph upon the sub-

ject, and cannot fail to occupy a place in the library of all medical readers.

Urinary Deposits : their diagnosis, pathology, and therapeutical indications. By GOLDING BIRD, A. M., M. D., F. R. S., &c., &c. 2d American edition, from the 3d revised and enlarged London edition. pp. 337. Philadelphia: Blanchard & Lea. 1851.

Dr. Golding Bird has been long known as one of the most successful investigators of urinary deposits. In the work before us he gives a faithful exposé of the physiological origin and physical properties of urine, and of the chemical pathology of the various elements of this secretion, followed by remarks on the therapeutical employment of remedies influencing the functions of the kidneys. This work is essential to all practitioners, who feel it their duty to be well acquainted with the resources of their art.

Lectures on the Eruptive Fevers ; as now in the course of delivery at St. Thomas' Hospital, in London. By GEORGE GREGORY, M. D., &c., &c. 1st American edition, with numerous additions and amendments by the author, comprising his latest views. With notes and an appendix, embodying the most recent opinions on exanthematic pathology ; and also statistical tables, and colored plates. By H. D. BULKLEY, M. D., &c, of New York. pp. 379. New York : S. S. & W. Wood. 1851.

The work of Dr. Gregory commends itself to the practitioner, not only by the success it has met with in England, but also by the importance of the subjects upon which it treats. Small-pox, measles, scarlatina, erysipelas and vaccination, require so frequently our attention, that we cannot be too well posted up in relation to them. The additions by Dr. Bulkley are both valuable and interesting.

A practical Treatise on the diseases of the Lungs and Heart, including the principles of physical diagnosis. By W. H. WALSH, M.D., Prof. of the Principles and Practice of Medicine, &c., in University College, London, &c., &c. pp. 512. Philadelphia: Blanchard & Lea. 1851.

Diseases of the chest are becoming more common in the Southern States as intermittent and remittent fevers diminish in frequency. We therefore esteem this publication as quite opportune, independently of its well known intrinsic value. A thorough knowledge of the principles of physical diagnosis may be obtained here, and cannot be dispensed with in the present state of our science.

On diseases of Menstruation and ovarian inflammation, in connexion with Sterility, pelvic tumours, and affections of the Womb. By E. J. Tilt, M. D., &c., &c. pp. 286. New York: S. S. & W. Wood. 1851.

We are much pleased to find Dr. Tilt's views reprinted in our country. He has treated with much originality subjects of growing interest, and his book cannot be read without great profit.

Letters to a candid inquirer on Animal Magnetism. By WILLIAM GREGORY, M. D., F. R. S. E., Prof. of Chemistry in the University of Edinburgh. pp. 394. Philadelphia: Blanchard & Lea. 1851.

No subject has met with more decided hostility on the part of physicians who have not studied it, than Animal Magnetism; yet of all classes of society, physicians are those whose vocation furnishes them most frequently with facts beyond their ability to explain, and which they nevertheless are compelled to admit. Scepticism in science is a duty—but so is unbiassed and candid inquiry. The Letters of Dr. Gregory appear to be written in the proper spirit and should therefore be read.

The Laws of Health, in relation to mind and body: a series of Letters from an old practitioner to a patient. By L. J. BEALE, M.R.C.S. pp. 295. Philadelphia: Blanchard & Lea. 1851.

This very sensible production is well calculated to enlighten the non-medical public, to whom it is addressed, upon a subject of all others the most interesting. If it were carefully read by thinking men, it would tend very materially to correct a multitude of vulgar errors and to lessen the success of charlatanism.

The Physician's Prescription Book; containing list of terms, phrases, contractions and abbreviations used in prescriptions, with explanatory notes, &c., &c. 1st American, from the 10th London edition, pp. . Philadelphia: Lindsay & Blakiston. 1851.

This little book contains much useful matter to the student who is not well acquainted with Latin, and who wishes to become familiar with the phraseology of prescriptions. We believe, however, that the use of our vernacular in writing prescriptions would diminish the number of accidents which continually occur in putting them up. We can see no good reason for mystifying by abbreviation and dog latin, what ought to be made as plain as possible.

ERRATA.—Page 516 (Sept. No.) 11th line from bottom; instead of "*Synochû*," read *Synochâ*; and 2 lines lower down, instead of "*synochus*," read *synocha*. Also, page 521, 3d line from bottom, instead of "*creeping and jerkey*," read *creeps or jerks*.

SOUTHERN MEDICAL AND SURGICAL JOURNAL.

Vol. 7.]

NEW SERIES.—DECEMBER, 1851.

[No. 12.]

PART FIRST.

Original Communications.

ARTICLE XXXVII.

Contribution to the History of Typhoid Fever. By H. R. CASEY, M. D., of Appling, Ga.

On the 20th day of April last I was called to see Ben, a negro man, aged 26, whom I found to be in a low state of fever: skin hot and dry; pulse 140; vomiting a dark greenish slime, with diarrhœa constant and wasting; tongue redish on the tip and edges and cupped on the top; eyes sunken, though of a peculiar brilliancy; great tenderness of abdomen; tympanitis and gurgling on pressure over the right iliac fossa. On enquiry into the previous history of the case, I learned that the "boy had been sick for three weeks, having a paroxysm of fever every day, but apparently of a light kind, rising about noon of each day, and continuing on through the night; that in the forenoon he would be free of fever; that he did not complain of any pain, only of great weakness, and that from the beginning of his attack." Not thinking there was much the matter with the boy, his owner had not called in medical aid, but had treated him himself, for a mild, though obstinate, attack of intermittent fever. He had given him repeated doses of calomel at night, and castor oil in the morning, and quinine during the intermission. Persisting in the treatment, and thinking the disease would ultimately yield to the remedies instituted, the case went on, overriding the mercurial and the great anti-periodical, until twenty-one days had passed, at

which time his symptoms became suddenly alarming. I was called in, and found him as above described. For the past dozen years I have had my commission, and have been fulfilling its requirements as occasion demanded; but in all my intercourse with the sick I had never seen a case of typhoid fever, nor had I ever met with a case identical in all its bearings with the one before me. For four or five years of my practice, my field of operation was in the south-western part of the State, in the counties of Baker and Lee, where bilious remittent and intermittent fevers are to be seen in all their protean forms, from the mildest quotidian to the algid or pernicious variety: the other years of my medical pilgrimage have been in the more interior of the State; but, as before observed, typhoid fever had been to me a "thing unseen." Yet from what I had read of this fever I immediately diagnosticated the case before me as one of genuine typhoid fever, and my prognosis was unfavorable, as I considered the boy in the last and fatal stage. The diarrhœa and vomiting being the most urgent symptoms, presenting the indications first to be met, I ordered small doses of morphine in gum water; blisters to the abdomen and between the scapulæ; glysters of starch and laudanum *pro re nata*.

21st. Found the patient in nowise improved by treatment: the disease hurrying him on to the world of spirits. Fever had not abated since last visit; delirium had supervened during the night, and its ravings were still to be traced in his low mutterings; feet and hands cold; abdomen enormously distended and hot, with great meteorism, tympanitis, and gargouillements. Ordered brandy and carb. ammonia; sinapisms to the extremities; hot mustard pediluvia. With this treatment he seemed to rally a little. Gave him to eat chicken broth with rice: he took a little, closed his eyes in sleep—and into that sleep from which there is no awaking.

I was sent for the next day to open an abscess on a negro's leg, and took occasion to step into the room to see the corpse, and was surprised at its appearance. Twenty hours had passed away since the departure of the spirit from the body, and yet there it lay with the blood still boiling or bubbling up from his mouth and nostrils, until a stream some 4 to 6 feet

had stained the table on which he was laid out, coming, I suppose, from the stomach, or possibly from the pulmonary vessels. From his eyelids to his toes, his skin was stretched to its uttermost. A few hours before his death, on examining the temperature of the different surfaces, I had felt and seen the boy all over, and the tegumentary tissue was soft and yielding, with the exception of the abdominal covering. Now, such was the extent of the distension that no indentation could be made. No post-mortem was allowed.

On the 22d, was called to see case 2d. Sam, aged 16, who was attacked the evening before with fever. Pain in the head. Upon examining Sam, I was led to the belief that typhoid fever had set in as an endemic on this plantation. The fever, in this case, did not seem to be very high, though his pulse was full, and presenting in a remarkable degree the double beat spoken of by some writers; tongue red on the tip and edges and furred in the centre, the red palpillæ appearing through the white coating. Upon examining his bowels, I found them but slightly distended, without gurgling. I instituted a general plan of treatment, which was put into practice in this, and in all the subsequent cases. I treated them on the expectant method; not expecting to cure the fever, but husbanding the energies of the system, reserving special treatment for the complications that might supervene in individual cases. My plan was to commence with an emetic dose of ipecac, and often full emesis was procured—to continue it in nauseating doses for a day or two; opiates at night. Seneka tea to follow the ipecac; Port wine, with a nutritious and non-excrementitious diet.

23d. Called to Sam, and found him with more fever than at last visit, and complaining of pain in his bowels; some tenderness. Had fifteen or twenty leeches applied to his abdomen, and warm poultices to succeed them. Gave him Port wine, with 10 grs. Dover's at night. This case ran its course in fourteen days, without any threatening. His fever would rise every day from 12 to 4, and continue on through the night, when there would be an intermission of some four to six hours.

Case 3d, was shown me while attending to the above. Eliza, aged 19, was attacked with fever; pain in the head; great thirst; tongue similar in appearance to the last. Ordered, an

emetic of ipecac ; and after its operation, to continue the nauseant in small portions, alternated with spts. mindereri. I shall not give (as I write from memory alone) the full particulars of each day's observation. This proved to be a very long and obstinate case, the fever not running its course under forty days. There was in this, and in case 2d, an eruption on the face, neck and chest. Her bowels, in the course of her attack, took on diseased action ; tympanitic and gurgling, with thin, watery evacuations. Leechings, fomentations, Port wine, and opium, corrected this state of things. She had a paroxysm of fever every day for forty days. There was then a subsidence of all febrile action for thirty six hours : then a return of fever, which made its appearance daily for five days. After this, she convalesced slowly.

Case 4th. Edmond, aged 15, was attacked with fever. Pain in the head ; tongue red and furred. This case was the only one whose bowels were constipated. His owner had given him, before I saw him, a dose of calomel, and afterwards oil ; but his bowels, up to the time I saw him, were still unmoved. I ordered a glyster of salt and water, which had the effect. This case was treated with ipecac, seneka tea, and anodynes at night. The eruption was out as thick as possible. This case was obstinate, the fever continuing for four weeks, having every day one paroxysm.

Case 5th. Abraham, aged 19, became a victim to the fever, which, however, was of a mild type, running its course in seven days.

Case 6th. Betsey, aged 16, was attacked with fever. Pain in the head ; tongue redish and furred ; bowels loose and tympanitic. Gave her an emetic of ipecac, and kept it up in nauseating doses for a day or two. Found her on one of my visits complaining of a sharp pain in her left side ; pulse quick, though not full nor corded. Thinking there might be a pleurisy supervening, I cupped her over the affected side, and applied a blister. Fearing a diarrhœa, I could not treat the case as I would have done ordinary pleurisy. I gave her small doses of ipecac, alternating with it the acetate of ammonia. Upon the drawing of the blister the pain and cough measurably gave way, and she seemed to be doing well on the morning of

my next visit. About 3 o'clock that day diarrhœa set in heavily. I was sent for in haste, but, from other engagements, did not see the patient until night. I then found her entirely prostrated from the wasting aqueous discharges, and her appearance indicating her in *articulo mortis*. She was delirious, trying to get out of bed; pulse small and quick; extremities cold. Had sinapisms applied to hands and feet. The head symptoms I looked upon as sympathetic, the coma and delirium not dependent upon either inflammation or congestion, but rather upon loss of nervous energy. I ordered opium in chalk mixture; blister to the abdomen; but of no avail—she succumbed that night.

Case 7th. Mehaley, aged 13, was attacked with fever. Pain in the head; furred tongue. Ipecac was given her, and subsequently seneka tea. This was a mild case, the fever running its course in five days. She continued to convalesce for five or six days, when there was a relapse. She was again treated as in the original attack, and after nine days of fever she again convalesced.

Case 8th. Ann, aged 17, was attacked with fever. Pain in the head; tongue red and coated; pulse quick and presenting the double or vibratory beat. Gave her an emetic of ipecac, and after the emesis nauseating doses of the same at intervals of three to four hours. This case proved to be a very obstinate one, and gave frequent threatenings of fatality, but ultimately yielded, after five weeks. This woman was of a delicate fibre, of nervo-lymphatic temperament; had been in bad health for some years; had had a severe attack of pneumonia last spring, from which she had not entirely recovered. Being very delicate, I thought she would not be able to stand the prostrating effects of the fever. Her lungs being in a weakened condition, I supposed that the disease, should it search for an organ upon which to spend its violence, would naturally go to the weakest one, and that pneumonia would be the complication and the death of the patient. My fears were in part realized: about the third day of the fever she complained of pain in the chest; had a cough, and expectorated the pneumonic sputa. She was cupped on the spine and blistered; croton oil over the chest, and put upon small doses of ipecac in flax-seed tea. The great

contra-stimulant, the sheet-anchor of safety in pneumonia, I was afraid to resort to, for reasons which are obvious. In some four to five days, these symptoms, to my surprise, subsided, and she seemed to be doing well, and hopes were entertained of her recovery. But these hopes were not long allowed to obtain. She began to complain of pain in her bowels, and upon examination, I found them swollen and tender upon pressure. Ordered leeches, warm poultices, chalk mixture, with Port wine. Despite of these means diarrhœa set in with large watery dejections: she was made to drink freely of persimmon and elm tea; put her upon sugar of lead and opium, and covered her abdomen with a blister. Was sent for in haste to see her, and on my arrival found her passing from the bowels large bloody evacuations, with large clots of blood. I increased the quantities of sugar of lead and opium, and gave her enemas of sub. nit. argent. (8 grs. to the ounce). To this last I attribute the check of the diarrhœa. I am in the habit of using it in all obstinate cases of both diarrhœa and dysentery, and I think I can say with almost uniform success; but here, in this case, I feared a disappointment of its curative powers. But again it proved "a source of life unto life." Her bowels were checked of both bloody and watery discharges in a few hours, and in twenty-four hours thereafter she had a tolerably healthy evacuation. The fever continued on, and in its course a mild pleurisy supervened, which yielded to a blister and warm teas. Her convalescence was slow, but steady.

Case 9th. George, aged 19, was attacked with pain in the head; tongue presenting the same marked similarity that had been and continued to be observed in all the cases, and which, for the sake of brevity, I shall term typhoidal. His bowels took on the anatomical lesion in the course of the fever, but yielded to the lead and opium powders. In seven days his fever left him, and he began to convalesce.

Cases 10 and 11. Daniel and Matilda, (twins,) aged 11, were the next victims to the disease, but of a mild type, the fever continuing for seven days. Case 12. Joe, aged 9. Case 13. Hiram, aged 13. Case 14. Anderson, aged 12. Case 15. Bill, aged 7—were all attacked in the same way, and with but little interruption. The fever ran its course in from five to nine days.

Case 16. Basil, twin brother to Ben, was attacked with fever and pain in the head, and sickness of stomach; tongue typhoidal. This case presented some differences from the beginning—so much so that there was some doubt about his having the fever. He did not take his bed as did all the others, and even went out to work; visited his wife some mile or two distant. He was, however, taken down in a few days thereafter with a violent diarrhœa. I was called in haste to see him: found him cold; delirious; bowels tender and tympanitic. Applied a blister to his abdomen; mustard to his extremities; brandy, with laudanum; but it was too late—his fate was sealed; he died in a few hours. The mother of these twins, like many of the African race, is tinctured with superstition, and she had imbibed the notion that her boys were poisoned, and it is thought that she physicked them for the poison, which may have given inveteracy to their attacks.

Case 17. Martha, aged 26, was the next victim, but the fever was of a mild variety, and left her in seven days.

Case 18. McLin, aged 22, had a severe attack of the fever: his bowels became seriously diseased; great nervous prostration; pulse weak; skin cold; brain much affected; and, upon the whole, gave threatenings of speedy dissolution. But, by the timely use of the remedies which had been used in the other cases, similar to this, he was saved, and recovered in fourteen days.

Case 19. Lewis, aged 22, was attacked with fever. Pain in the head; tongue typhoidal; with loose bowels, and with a dry, hacking cough. When his diarrhœa was controled, ipecac and a blister between the scapulæ removed the cough, and after seven days of fever he convalesced.

Case 20. Amy, sister of the last, was attacked at the same time, and in the same way. She was treated on the same plan; but in her case the fever was of a more obstinate type, and complicated: pneumonia, diarrhœa, hysteria, subsultus tendinum, and bed-sores, were all present, and aggravated her case. The fever continued for six weeks, and finally wore her out. The closing stage was marked by a wild delirium, and in her ravings her spirit escaped from its brittle and shattered tenement.

Cases 21, to 27. Moses, 20; Jane, 12; Adeline, 16; Emeline, 13; Cynthia, 10; Dick, 7, and Cassy, 10 years of age—were all attacked with the fever, and pretty much in the same way. The fever, in all these cases, was of a mild type, and ran its course in about five days.

Case 28. Nelly, aged 10, was attacked with fever. Pain in the head, tongue typhoidal. This proved to be an obstinate case, and the only one of the children that was so. Her bowels became diseased about the third day, and after their adjustment there was a metastasis of the disease to the head, and acute meningitis supervened. This ultimately yielded to leeches and cold cloths to the shorn scalp; but when her fever left her, she seemed to be in *articulo mortis*; pulseless and cold as marble. She was kept on brandy toddy for two or three days, and ultimately recovered.

Case 29. Jackson, aged 20, was attacked with pain in the head; fever; tongue typhoidal; bowels tender, and somewhat tympanitic. Was treated on the same plan as was adopted in the onset, and recovered after an illness of some two weeks.

Case 30. Adeline, aged 16, was the last case I had up to the time when I write. This was not a case of typhoid fever, in its incipency; but after the original disease (severe at first) began to give way, it became clothed in the livery of the prevailing epidemic. This was a case, in its origin, of acute gastritis—the inflammation, after a given time, descended down into the ileum—and the characteristic abdominal lesion became manifest in the tympanitis; gurgling and tenderness over the iliac fossa. This was a very long, tedious and exceedingly critical case; but after continuing for some six weeks, she seems now in a fair way to recover. After the gastritis was subdued by appropriate remedies, and the typhoid symptoms set in, she was treated as all the other like cases were. Diarrhoea and pneumonia complicated this case.

REMARKS.—The above is a hasty and imperfect summary of thirty cases of Typhoid fever, occurring in the family of Maj. T. E. Beall, of this county, and in reporting them for the Journal, I do so with no intention of writing a treatise upon the fever in question, nor of attempting to settle the different

controversial points that have been and still are agitating the medical world. With an experience so limited as mine in the management of typhoid fever, it would be rather assuming in me to attempt to *teach*, when I am only commencing to *learn*. I am only presenting a given number of cases that came within the perview of my own observation—the treatment instituted in those cases, and the success attending that treatment. I will, however, call the attention of the profession to one peculiarity in these cases, and that is the *type* of the fever. Typhoid fever is, and has always been, classed among the continued fevers, and I do not remember ever to have read or heard of it prevailing in any other form. By reference to my cases, we find it of a positively intermittent character, there being generally a decided intermission of from two to six hours. From its decidedly *intermittent character*, I was induced to try the quinine; but no impression was made by it. I shall say nothing of the locality where this fever prevailed, for I place but little stress upon *local* causes in the generation of typhoid fever. The hidden, unseen, intangible miasmata are not culpable for this fever. I am rather disposed to ascribe its cause and prevalence to a peculiar condition of the atmosphere. Nor am I one of those who hold that typhoid is but a modification of bilious remittent fever. Prof. T. D. Mitchell contends for their identity, and he thinks quinine should be as effectually resorted to in this as in other fevers. Now, if the Professor's reasonings are *a posteriori*—that is, if he considers them in the same category, because quinine has a like influence—his teachings are liable to be attacked, and “of a right should be,” for I think I hazard nothing in saying that no practitioner in these days ever resorts to quinine with a view to break up this fever. It has no more effect in arresting a paroxysm, or in severing the chain of febrile action, than so much saw-dust: typhoid fever has a defined course to run, and as well may we attempt to curb the storm, while at sea, as to attempt to cut short or stay in its progress this fever with quinine. The skillful mariner will rather attempt to guide his vessel safely into port, than to raise his feeble hand to still the raging element. It is a fever which makes a great demand upon the *vis medicatrix naturæ*, and the philosophic physician will make his means chime in with this

conservative power. Act upon nature's dictates by giving to our remedies such a direction as will aid, and not antagonize her salutary process—sustain the powers of the system until such time as its natural tendency to restoration brings about recovery. As far as my observation extends, the greatest danger to be apprehended is from the bowels—the supervention of diarrhœa—and a good plan of treatment is to use such means as will have a tendency to prevent such a state of things. With this view, I put my patient (*ab initio*) upon Port wine, three times a-day, and opium at night. I locked up the bowels from the jump—"an ounce of prevention is worth a pound of cure"—using enemas alone, when dejections were thought advisable. I believe I am safe in saying that the mortality was small, in my cases, considering their number. The statistics of France show a mortality of one-third. I consider it a contagious disease. This opinion is in direct conflict with the generally received notion of this fever, both in this country and in France. (I learn from Professor Dugas, who has just returned from a summer tour through Europe, that the French physicians are changing their opinions on this subject, and that he thinks Typhoid fever is now held by a majority of them to be contagious. Two of my cases were married women, having husbands living on plantations some miles from the infected locality. The husbands of these two women (being allowed to visit their wives in their sickness) were both attacked with typhoid fever. In 1847, typhoid fever made its appearance in a certain family in this county. A lady, who was a visitant of the family, paid a visit to her friends in South Carolina. At this time, the family she was leaving were in the enjoyment of perfect health. It happened that the family in South Carolina were suffering with typhoid fever. She returned to her friends in Columbia, and in a few days after was taken with typhoid fever: it spread through the family, and several of them died. Dr. Grisolle is an advocate of the contagiousness of typhoid fever, but concurs with Louis, that it may arise independently of contagion, and with Gendrin, that one attack gives immunity from a second. This last, I believe, is the prevailing opinion of the profession, both of this country and Europe. My last case formed something of an exception to

this rule; yet, as that was not a case of typhoid fever, in its *origin*, it cannot, in all probability, be brought up as an exception to the general rule.

There seems to be a greater prevalence of this fever this year than at any prior time, owing, doubtless, to a *very dry and extremely sultry* summer, succeeding a cold, damp spring. Nor do I know how to reconcile this notion with the fact of its greater prevalence in Northern latitudes. To sum it all in a very few words, I think I can safely say, that as yet we know but little, of a *certainly* of typhoid fever. The ambiguity attached to this knowledge is well expressed by Prof. Darrach, in his "charge" to the graduates of the medical department of Pennsylvania College. In speaking of "medical discernment," the Professor says—"In typhus and typhoid fevers, how difficult it is in their multiform and antagonizing therapeutics, to determine upon the *when*, and the *when not*—the *where*, and the *where not*—the *how much*, and *how little*—the *how often*, and *how not at all*," etc.

In presenting my cases, I have given the ages of each and every one, as there seems to be a contrariety of opinion on this point. Some writers contend that typhoid fever confines itself to individuals between the ages of 10 and 30—others, again, from 15 to 35; while there are a few who say it has no latitudes to its ravages, attacking, indiscriminately, all ages and both sexes. On the plantation where the endemic of which I write prevailed, there were some fifty negroes, ranging from the infant of a few months, to the grey-headed grandsire. Its ravages were confined to patients between 7 and 26 years of age. Mothers with suckling infants were attacked with the fever, while the infants escaped. Now, I don't think I am authorized in saying that typhoid fever is limited in its attack to persons between 7 and 26, because none younger and none older than those ages were attacked: another year and another epidemic might compel me to change my opinion. We should not lay down postulates hurriedly. Dr. E. Parks, (*Medical Times*, vol. xxi. p. 557,) in speaking of the diagnosis of typhoid fever, winds up by saying, "finally, in fixing the diagnosis of typhoid fever in adults, it should never be forgotten that it is most common under 40, and seldom seen above that age. In

old people there are many febrile conditions consequent on latent and subacute inflammation, or some urinary derangement, which may, at first sight, bear some resemblance to typhoid fever; but the age of the patient will be an insuperable argument against the existence of the disease. By reference to my plan of treatment, it will be seen that I have entirely excluded mercury in all its forms. From my knowledge of the effects of this remedy—from the *slow, protracted* course of the disease, its defined course, the diarrhœal proclivity—I feel authorized in entering my caveat against mercury in the treatment of typhoid fever. M. Becquerel, jr., has presented to the French Institute an important memoir on the treatment of Typhoid fever with the sulphuret of mercury, internally, and mercurial ointment endermically. I am not prepared to say how the essay was received by that honorable body.

If the miasmatic fevers of our country are to be displaced by this peculiar fever, it is time that we should “be up and a doing,” seeking to obtain information where it can be found. If we are to have yearly visitations of this fever, it is highly probable that a familiarity with it at the bed-side of the sick will add to our now limited stock of knowledge, and the time *may come* when we can as confidently expect success to attend our treatment in this fever, as we now do in the management of malarious fevers. We have some surety in this, from the fact that ours is a cumulative science—that as years roll around, the science of medicine advances—she sees no climacteric point: that though she may now be basking in effulgency, she yet sees in the far distance a ray of light, still more dazzling—inviting her onward. The science is *progressive*, and in the future there are laid up for the medical philosopher gems now hidden from sight. And this is what, in part, gives beauty to the study of the *healing art*. The idea that we can live on and learn—that our knowledge now, is only a stepping stone to farther advancement, which will as surely follow, as probation is lengthened, fills the mind of the medical inquirer with ineffable delight.

ARTICLE XXXVIII.

Vital Statistics of Houston County. By GEO. F. COOPER, M.D.,
of Perry, Ga.

Vital statistics are to medicine, what mathematics are to the astronomer. Perhaps our calculations are not as precise, and may not attain our object as infallibly as his, yet the same principle is involved, and to us the results are as grand. The most astute observers have long since acknowledged the "numerical system," based upon proper and authentic data, to be the most practicable and reliable means of establishing the average of life and its duration, the salubrity or insalubrity of different localities and other matters most intimately connected with and important to the progress of civilization. Extensive laborers in this field are unfortunately but few,—the work which attaches to it is not enviable, but the reputation which it secures is worth possessing. The works of Bérard, Casper, Tommasini, Robertson and Quetelet, will long live as reminiscences of their industry. The philanthropic efforts of Lemuel Shattuck, Esq., of Boston, to improve the sanitary condition of his commonwealth, will be imperishable; and those of Dr. Simonds, aided by others, of New Orleans, to stay the frightful mortality annually dealt out upon her population, will not go unrewarded. And could the information which pertains to medical science, recorded in the late census, and which is quite accessible, be spread out before the public, developments would be made highly encouraging to our empire State. Dr. Pendleton, of Hancock county, projected a work that I would gladly see consummated in the respective counties of the State, and from which I have derived no inconsiderable aid in the preparation of this paper. Houston county lies between $32^{\circ} 15'$ and $32^{\circ} 40'$ north lat. and $6^{\circ} 30'$ and 7° west long. from Washington, between the Ocmulgee and Flint rivers, below the line of primitive rocks which touch at Macon, the nearest point, I believe. Tertiary formations constitute its geology.* Pine lands compose the largest area of its surface—there is a

* At no distant day I hope to be able to speak of its meteorology from more complete data than I now possess. I hope, therefore, my omission of it now, will be overlooked.

section of mixed growth that is pine and oak, and a ridge stretching almost, if not entirely, from north-east to south-west, through the county, apparently more elevated and possessing I doubt not some interesting geological features; many places abound in lime rock composed almost wholly of fossil shells, many of which are entire, and by conchologists could easily be assigned their respective places. This ridge is well known as "the oaky woods" or "lime lands," and is much the most productive, owing doubtless to the great quantity of calcareous matter present. These rocks are not very hard, and when burned yield largely of pure lime. Most of the pine lands consist of a silicious soil, and are therefore not productive; while some, and particularly those that border upon the streams, have a loamy soil and are indeed the most desirable cultivable lands in the county.

The following table will exhibit at one view the entire population, as to race, sex, age, &c.

Years.	WHITES.		BLACKS.		MULATTOES.	
	Males.	Females.	Males.	Females.	Males.	Females.
under 10	1162	930	1680	1716	83	55
over 10 " 20	775	654	1116	1029	54	127
" 20 " 30	562	620	1033	1048	38	23
" 30 " 40	505	387	465	469	11	7
" 40 " 50	192	190	200	152	2	2
" 50 " 60	124	129	176	197	2	1
" 60 " 70	83	75	91	82	0	0
" 70 " 80	44	48	22	36	0	1
" 80 " 90	7	5	14	14	0	0
100 & over	0	0	1	3	0	0

This gives us, whites, 6492; blacks, 9544; and mulattoes, 406—being seven less than appears upon the census.

The aggregate population as recorded is 16449. It will be perceived that one unusual feature occurs in this table: among the white females, that from the age of 30 to 40 inclusive, there is the diminution which generally occurs from 40 to 50. This is explicable only on one ground—and my acquaintance in the county is not sufficient to verify the suggestion, that is the critical period takes place earlier among the white females than among the negroes. I think this idea would hold good as

far as my observation goes, it being rarely the case that white females are found bearing children over the age of 40. Between 40 and 50 they get the ascendancy and maintain it. After the age of 50, particularly after 70, a sudden decrease occurs, which can only be explained by the fact of the county having been settled comparatively of recent date. There is no doubt, that as the county grows older, the number of these ages will be swelled so as to compare favorably with the older settled sections of the State. It will also be noticed that a large number of slaves are under the age of 30, which is presumed to arise from the fact that, a large portion of our slave population is a purchased one. This does not appear among the whites, and is therefore rendered more plausible. The males are predominant in both of the pure races, while in the hybrid the females are quite in the majority; a fact noticed by Dr. Pendleton, and which he intimates may have some bearing upon an interesting physiological fact—alluding, I imagine, to their procreation. In regard to extreme longevity, the blacks take precedence; while from 60 to 80 the whites have much the largest ratio. One white attains 60 in every 41, while the blacks only 1 in 57, and mulattoes 1 in 136. Whites who attain 70, nearly 1 in 70, and blacks only 1 in 168. There are only 3 mulattoes in the county who have attained 60, and only 1 at 80.

We have among the whites, 12 octogenarians, (7 males and 5 females,) which gives us 1 in every 543. There are 28 black octogenarians, (14 males and 14 females) giving them predominance, being 1 in 354. Not a single centenarian is recorded among the whites, and only 4 among the blacks—1 male and 3 females—giving them 1 in 2481. We should not overlook the recent settlement of the county in connexion with this part of the table.

Whether or not this extreme age is attained alone by the original Africans, I cannot say; I believe, however, the idea would be upheld by investigation.

The number of marriages among the whites was 43. The number of births 166—giving an average of 3.86 births to each marriage, or 3.7 per cent., which, as well as I remember, will compare favorably with England and some other European

countries. The number of black births was 223—making the aggregate of births 389. This will give us among the whites 1 birth to 39·3 persons, and 1 to 44·5 of the blacks; and 1 to every 42 or 2·36 per cent. of the whole. Notwithstanding the circumstances of the blacks, in the main, are much more adverse to gestation, and perhaps three abortions occur among them to one among the whites, yet the difference in the per centage of births is small: with proper management, I believe they would transcend the whites almost two to one. It is often to be seen upon large quarters, where there are thirty, forty, and even fifty negresses, scarcely a birth, and at most, not over three or four occur during the year. I think I know some planters who really do not desire the negro women to bear children, because they cannot perform the same amount of labor. If men would reflect, they might see in the end they become their own despoilers; and those who attempt to frustrate nature's great design, and do not encourage the increase of their own property, should not possess it.

There were 201 deaths in the county for the year ending the first day of June, 1850: of that number, 114 were males and 88 females, 72 were whites, 125 blacks and 4 mulattoes—making a per centage among the whites of 1·10, or 1 in 90·6; among the blacks, mulattoes included, 1·29, or 1 in 76·9. For females, the ratio was 1 in 80, or 1·10 per cent.; for males, 1 in 73·5, or 1·35 per cent. For the whole population, it was 1 in 81·7, or 1·22 per cent.—leaving us an increase of population by births alone of 2·36 per cent.

The following table will exhibit the number and ages of those who died:

under 5				100
over 5	"	10		12
" 10	"	15		13
" 15	"	20		11
" 20	"	25		11
" 25	"	30		4
" 30	"	35		4
" 35	"	40		6
" 40	"	45		5
" 45	"	50		7
" 50	"	55		1
" 55	"	60		9
over 60				
under 65				8
" 65	"	70		3
" 70	"	75		0
" 75	"	80		2
" 80	"	85		3
" 85	"	90		1
" 90	"	95		0
" 95	"	100		1

Thus it will be seen that half of our mortality occurs among those under 5 years of age, and much the largest proportion of them are blacks; which may be easily explained, in consequence of the same care not being taken of them and the same comfort being provided. The farmer should look well to this matter, as he is loser and would be profited in an amendment. That our climate or locality is inimical to the child, I have yet to learn; but really mothers would not be induced from the above table to make a nursery of Houston county. I am much inclined to the opinion, that the major part of this mortality may be justly attributed to that prevalent, deprecable belief, that "old women know more about children, than doctors." It might be expected that the mortality of the blacks would exceed that of the whites; for they are, perhaps, as susceptible to predisposing, and certainly more exposed to exciting causes, and they are not unfrequently crowded together in illy ventilated houses, which too often become foci of disease, and as often in shanties which do not protect them from the cold and vicissitudes of winter. I noticed in Dr. Pendleton's paper the mortality among the whites was only 0.76, while among the blacks it was 1.35, a disparity much above ours. The excess of our white mortality over his may be, perhaps, in part, explained upon the ground, that in the older settled portions comfort and ease are studied much more than here, where all is absorbed in that insatiable desire to make cotton and buy negroes.*

The following is the order of deaths in each month:—January, 18; February, 29; March, 15; April, 17; May, 28; June, 8; July, 16; August, 12; September, 24; October, 15; November, 7; December 11. The following will show the order as to seasons:—Spring, 60; summer, 36; autumn, 46; and winter, 58; clearly showing winter and spring to be, by considerable odds, the most fatal seasons—a fact which, for the last few years, accords with the observation of all. These seasons, once the most salubrious and enjoyed, are now looked to with much apprehension and dread. The average age of whites, at death, was 20 years; of the blacks, 16.6; and mulattoes, 10.

* Efforts were made to obtain information touching the mortality of the adjacent counties, but were unavailing.

The number of the latter class, compared with the others, being too small to afford reliable data. The great mortality among children will account for this limited average, as it will be seen by reference to statistical tables, either of Europe or this country, that as low down as our county sounds to many of our up-country brethren, our mortality is by no means discouraging.

The diseases occurred as follows, having been alphabetically arranged :

Apoplexy,	1	Fever, typhus,	1
Bronchitis,	6	“ inflammatory,	1
Brain, dropsy of	1	“ child-bed,	5
“ congestion of	1	Gravel,	1
“ inflammation of	1	Influenza,	3
Bowels, inflammation of	15	Injuries,	18
Croup,	4	Intemperance,	3
Cramp Colic,	2	Lungs, congestion of	1
Cholera Infantum,	1	Old age,	4
Convulsions Infantile,	4	Pleurisy,	2
Dentition,	6	Pneumonia,	27
Diabetes,	1	Pertussis,	4
Dropsy,	15	Rheumatism,	1
Dysentery,	3	Scrofula,	1
Erysipelas,	1	Suicide,	1
Fever,	9	Phthisis Pulmonalis,	3
“ congestive,	5	Unknown,	35
“ bilious,	1	Worms,	6
“ typhoid,	3		

We find 12 deaths to have occurred from attacks upon the brain and nervous system, 6 from dentition, 27 upon organs of digestion, 15 from dropsies, 1 from eruptive fever, 20 from idiopathic fevers, 1 from disease of fibrous tissue, 1 from glandular, 18 from injuries, 2 from intemperance, 4 of old age, 50 organs of respiration, 1 suicide, 2 urinary organs, unknown 35, and 5 peculiar to women. More than half which compose the diseases of the organs of respiration are pneumonia, which has caused our citizens to look with so much apprehension to the winter season, 18 of which I think occurred in one militia district. In this instance, latitude seems to have lost its influence in the production of that class of diseases which it usually exhibits. The unknown stand out in bold relief, and is certainly

no argument that all have physicians, for doctors usually name disease, whether right or wrong. The ugliest feature is, that our mortuary records are blotted with 3 deaths from intemperance. That class of disease, zymodic, which constitutes the standard by which we are enabled to compare different localities, and their relative salubrity does not occupy such a prominent stand in our catalogue of diseases as to frighten us, and I presume there are not many sections which have the advantage of us. In the county, there was one deaf white, 3 deaf and dumb, 1 blind, 1 insane, and 11 idiotic. Among the blacks, there were 6 blind, and 9 idiotic. Making the total of 1 deaf, 3 deaf and dumb, 7 blind, 1 insane, and 20 idiotic. The blacks suffered most from blindness, evidently from exposure. Among the idiots, 5 belonged to one family, and 2 to another, thereby swelling our number by hereditary transmission, and not incidental causes. Out of the whole number, 17 were males.

ARTICLE XXXIX.

A notice of Dysentery, as it prevailed in Gordon and Cass Counties. By J. S. WEATHERBY, of Palmetto, Ga.

Having noticed in the Sept. No. of the Journal, a communication from Dr. Campbell, of Augusta, on Dysenteric fever, and observed that he failed to obtain reports from localities where it prevailed, I have drawn up a sketch of an endemic, that I with others, attended last summer, in the counties of Gordon and Cass. This will of course be brief, as I have to depend entirely upon memory.

The locality in which it first commenced and evinced more malignancy, was in Gordon county, on and between the creeks Salaquoy, Pine-log and Cedar. The whole of this region of country was some years since very subject to bilious fevers, but for the last two or three years had been remarkably healthy, with the exception of sporadic cases of typhoid fever. The spring and summer were remarkable for their heat and drought. The endemic commenced about the 1st of June, and might be truly called an awful scourge, sweeping over the country with a baneful and noxious influence. Scarcely a family in the re-

gion of its prevalence escaped a visitation from this unwelcome guest; and many, particularly the very young and very old, succumbed to it.

Symptoms.—It was generally ushered in by slight pain and tormina, in the lower portion of the abdomen, with frequent desire to defecate. The discharges during the first day consisted of feces mixed with a little mucus. These symptoms gradually grew worse, the pain more intense; and on the second or third day a slight chilliness was followed by fever, which would generally be continuous. The abdomen at this time usually became tender, and evacuations attended by the most excruciating pain, would occur at intervals of from fifteen to thirty minutes. The evacuations consisted of bloody mucus, and sometimes pus, resembling very much the contents of a large boil that had been bruised. The tongue was generally coated with a thick whitish or brown fur, and was frequently cold at the tip. The head was very rarely affected. The pulse was generally full, and from 80 to 100 per minute. The skin generally remained harsh and dry until the latter stage, when in fatal cases it became suffused with cold, clammy perspirations. The duration of the disease was from ten to fourteen days, though in some cases it proved fatal in less than ten days, and in other cases recovery did not commence before the twentieth day. I can say very little of the pathology of the disease, as I made no post-mortems; yet I think the onus fell upon the large intestines, and that the inflammation frequently extended to the whole alimentary canal.

The cause of the disease must have been atmospheric, as it made its appearance in many places and at nearly the same time; the people, however, contended strongly for its contagiousness. When called to a patient in the first stage of the disease, if the pulse was full and strong, with febrile excitement, the lancet was used freely and always with benefit; after which the bowels were freely acted upon, either with calomel and ipecac, blue-mass, or castor oil and ipecac. Sulphate of morphia was given to allay pain. After the bowels had been acted on, Dover's powder was given every two or three hours; warm fomentations to abdomen; injections of slippery-elm tea, starch and laudanum, &c., &c. In what might be called

the 2d stage, if the pulse did not warrant bloodletting, I gave calomel and Pulvis Doveri, night and morning, followed by castor oil and ipecac. After the bowels were acted on by the oil, pulvis Doveri was given in oft and repeated doses. I also gave nitrate of silver injections in this stage. In the last stage, if the disease progressed thus far, medicine did little good. Stimulants and nitrate of silver injections were the principal remedies. With this treatment, out of fifty or sixty cases, I lost five or six.

ARTICLE XL.

A Case of Double Lithotomy. By WM. J. JOHNSON, M. D.,
of Fort Gaines, Ga.

We make the following extracts from a letter received from Dr. Johnson, dated Nov. 1st, 1851. The patient, Mr. Givins, was 18 years of age, and had been affected with symptoms of stone from early life. Dr. J. first saw him in August last, and advised an operation as soon as possible. It was, however, only on the 8th October that he repaired to the Doctor for relief.—[Ed.

At 10 o'clock the next day, (9th Oct.,) I repaired to Franklin, where I was met by my brother, Dr. Wm. G. Johnson, and Drs. Hilburn and McKewen, surgeon-dentists, who kindly administered the chloroform, while, with the assistance of my brother, I undertook the operation—and truly a formidable operation it proved to be. Having secured the patient upon a table, in the usual manner, I proceeded to perform the lateral operation. The operation progressed finely and satisfactorily until I encountered the stone, when, from its large dimensions, I found it impossible to extract it through the perineum. I then made a great many ineffectual attempts to crush it with the forceps, but all my efforts proved abortive, and I was compelled, after a consultation with my brother, as a dernier resort, to perform the high operation. The calculus was removed, the wounds dressed in the usual manner, the patient put to bed, and a dose of pulv. doveri et morphine administered.

His situation for the first twenty-four hours was as comfortable as could have been expected, under the circumstances. He had scanty urinary discharges through the tube left in the opening. He slept comfortably, and declared himself more free from pain than at any previous time for the last two months. During the operation the hemorrhage was slight, the patient not having lost exceeding a half a pint of blood. The coats of the bladder were much thickened and indurated, showing that the long presence of the stone in the cavity of the bladder had occasioned a structural change in its parietes, &c. After the first twenty-four hours, symptoms of peritonitis supervened, accompanied with ileus. He had cold surface and extremities, constant vomiting every few minutes, indomitable thirst, tympanitis, &c., obstinate constipation, &c., &c. Notwithstanding these unfavorable appearances the patient lived some eighty-two or three hours. The body of the calculus, I suspect, is composed principally of phosphates; the outer crust, or covering, is exceeding fragile, and the color is a greyish white mixed with red. The calculus was weighed by Col. McAlister, a merchant of Franklin, and found to weigh within a fraction of six ounces. Could I have succeeded in securing all the fragments, I am certain the weight would have been at least six ounces. The following are the dimensions of the stone, taken by a workman in this place, by rule:—Length, 3 inches; longitudinal circumference, 7 inches; width, 2 inches; lateral circumference, $5\frac{3}{4}$ inches.

I have no recollection of having seen any case reported where so large a calculus has been removed in this State.

PART II.

Eclectic Department

On the Physiology and Pathology of the Phosphate and Oxalate of Lime, and their relation to the formation of cells.

By WILLIAM BENEKE, M. D., Resident Physician at the German Hospital, Dalston.—(Continued from p. 473 of this Journal.)

PART II.

Having alluded to the principal facts which afforded a proof of the relation of the phosphate of lime to the formation of cells, in my first communication, there is one point which I have now to insist upon.

It, namely, being a fact, that we are enabled to increase the formation of cells in diseases which evidently show a want of formation of cells, by the administration of phosphate of lime, we must suppose that there exists a want of the latter in individuals who are afflicted with those diseases; and so arises the question—whether it be true, that in diseases which I have shown to be most beneficially influenced by the administration of phosphate of lime, that the phosphate is present in the system in a smaller quantity than it ought to be in the normal state?

With respect to this question, we have first to complain of the impossibility of directly determining the quantity of phosphate of lime which is present in the body. Every one who has directed his attention to the quality of the bones in different skeletons will be acquainted with the great differences of their consistence; however, we are unable to determine or even to estimate these differences during life. One might suppose, perhaps, that the above question could be solved by accurate analysis of the blood; this supposition, however, is also entirely wrong. The blood which is drawn from the veins or arteries does not show the proportion of the materials brought into the system as food; it does not only contain those materials which are necessary for the formation of new substance, but it also contains many parts which result from the wear and tear of tissues and organs, so as to form a compound mixture of nutritious and excrementitious substances. We ought, therefore, to be as careful as possible in making up our results from those analyses. We are very well aware of the increase of fibrin in the blood in cases of acute rheumatism, pneumonia, and other inflammatory diseases, but we have not yet arrived at a certain determination, whether this increase of fibrin is owing to an increased quantity of albuminous substances taken as food, or

to an increased resorption of the fluid which penetrates the cellular tissue of the various organs, and the single constituents of which partly originate from chemical changes of the tissues themselves. And still greater is our uncertainty in judging of the origin of the phosphate of lime which is found in the blood. It is not difficult at all to determine the quantity of phosphate of lime contained in a certain quantity of blood ; but what conclusions can be drawn from such an analysis ? On the one hand the phosphates are brought into the system with the food taken, on the other they originate from the wear and tear of the bones and the muscular tissue ; one part of them is concerned in the formation of new substance, another part is thrown out of the system by the kidneys and the bowels. Supposing now the wear and tear of bones to be abnormally increased, an abnormal quantity of phosphates will consequently re-enter into the composition of the blood, and by analysing the latter we shall find an increased quantity of phosphates in it. The general waste of bones is therefore leading to an increase of the phosphates in the blood, and nothing really would be more erroneous, than to suppose the general quantity of phosphates in the system to be increased, whenever we find their quantity increased in the blood. We cannot therefore avail ourselves of the analysis of blood in order to solve the above question ; but there is another way which will enable us to do so, and this way is afforded by continued analysis of the urine and the fæces. Thereby we shall know what quantity of phosphates is thrown out of the system, and by comparing this quantity with the average quantity of phosphates taken with the food, we shall be very well enabled to judge of a general increase or decrease of the earthy phosphates in the system. However, as it is very likely that the greatest part of the phosphates contained in the excretions of the bowels originate directly from the ingesta, and as it is certain, on the contrary, that the greatest part of the earthy phosphates contained in the urine originate from the wear and tear of the tissues and bones of the system, I have in the first place only directed my attention to the latter, and I have found the analysis of the urine to give a satisfactory answer to the question proposed. It is clear that if in the normal state from six to eight grains of phosphate of lime are eliminated from the economy in twenty-four hours, and elimination of from thirty to forty grains must indicate a general loss of bony substance ; and from the quantities of phosphate of lime found in the urine, we may approximatively determine whether such a loss takes place or not. I do not myself consider the analysis of the urine alone sufficient for a complete solution of the above question ; such a solution absolutely requires also analyses of the fæces and

ingesta ; however it will be seen that, to begin with, those analyses of the urine are sufficient, and that they indicate the way which we have to apply to in our future inquiries. Thus, then, instead of the above question, the question that arises is, whether in those diseases which I have shown to be most beneficially influenced by the administration of phosphate of lime, the earthy phosphates are eliminated from the system by the urine in a larger quantity than in the normal state ? whether different quantities of phosphates are excreted in different diseases generally ? and what quantities these are ?

I am happy to state that I have succeeded in answering this question satisfactorily ; and it really afforded me great pleasure to see the results of the analysis of the urine agreeing in the most remarkable manner with my former observations. I have found that in all chronic diseases which are distinguished by wasting, emaciation, ulcerations of the skin and of the bowels, &c., (such as scrofula, especially rickets, &c.) a much larger quantity of phosphates is removed from the economy by the urine, than ought to be in the normal state ; and even such quantities as one never would have imagined, without entering into these examinations.

Previously, however, I have to say a few words on the mode of examining the urine which I adopted. As in other examinations of this kind, I here also only endeavoured to obtain approximate results, which I believe to be sufficient for general deductions. It seems to me quite indifferent, indeed, whether we know that a man's urine which is passed during twenty four hours, contains ten grains or whether it contains eleven grains of phosphates ; if our mode of analysis is sufficient to show whether a given quantity of urine contains ten, fifteen, twenty, thirty, fifty, or more grains of phosphates, I think this mode affords a way by which we may arrive at positive results. First, I believe it ought to be our only object to obtain such general and positive facts, upon which we can base our physiological and pathological views ; and having become acquainted with these positive general facts, we may enter upon their more accurate examination, though in many cases we shall hardly gain anything else thereby, than a quantity of numbers, more positive and accurate than could be obtained in the other way ; we shall gain other numbers, but no other views. In this way I then proceeded with respect to the phosphates of the urine ; and the following was the mode of examination which I employed.

It is well known that the phosphates of lime, as well as of magnesia, are held in solution in the urine by its acidity, and directly precipitate when the urine becomes alkaliescent by the

addition of alkalies, or ammoniacal by putrescence. This precipitation of the phosphates becomes much more complete when the urine is heated or boiled, and by the addition of a solution of soda to boiling urine we may precipitate the whole quantity of phosphates present. Supposing now we put a small quantity of urine (about two drachms) into a test-tube, boil it, and add a few drops of a solution of pure carbonate of soda, so as to make the urine alkaliescent, and directly we shall perceive whether a small or a large quantity of phosphates is present. By always making use of the same solution of soda (I used a solution of one ounce of dry carbonate of soda in twelve ounces of distilled water,) and of the same sort of test-tubes, we shall farther observe, that by the precipitation of the phosphates, certain and distinct degrees of turbidity are produced in this way in different sorts of urine, and even these degrees are so well marked, and so easily distinguished one from another, that I have considered this way sufficient in order to judge of the quantity of phosphates present in any urine. I therefore marked these various degrees of turbidity by numbers, and so exhibited for myself seven various types, on which I founded my observations. By No. 0, I marked a sort of urine which did not show the least turbidity by the addition of soda when boiled; by No. $\frac{1}{2}$ a sort which became opalescent by the same means; by No. 1, a sort of urine which became opalescent in a higher degree, but so as still to admit of the recognition of objects behind the test-tube, as, for instance, the bars of the window, &c.; by No. $1\frac{1}{2}$ a sort which exhibited an opalescence of such a degree that it was impossible to see through it, or to recognise any object behind it; by No. 2, a sort which became quite turbid by the addition of soda, when boiled; by No. $2\frac{1}{2}$, a sort from which, after a few seconds, proceeded a precipitation of phosphates, when treated in the same way; by No. 3, a sort which directly yielded a precipitation by the addition of soda; and lastly, by No. 4, a sort of urine from which the greatest possible amount of phosphates was precipitated. These numbers, and these significations of them, soon became so familiar to me, that by several most accurate analyses, which I instituted in order to have a test of accuracy, I almost always obtained equal results so as to convince me that this mode of analysis was to be relied upon.

Of course there occurred descriptions of urine which could not with propriety be marked by any of the numbers alluded to; well then, I marked them by $\frac{3}{4}$, $1\frac{1}{4}$, $1\frac{3}{4}$, &c.

In order to know the positive quantity of phosphates which was contained in the different sorts of urine, I made most accurate analyses of each of them, and these analyses the more in-

creased my confidence, as they showed quite similar results to the numbers proposed.

When meeting with alkaline or ammoniacal urine, of course, after having well stirred it up, so that the sediment, if there be any, should be equally distributed throughout the whole, I first tried the degree of alkalescence, and according to this degree I added a few drops of soda-solution or none; and when meeting with albuminous urine, I first precipitated the albumen or boiled the urine, after the addition of the solution of soda, in which case the albumen does not coagulate. The eye may often prove fallacious; however I could never deviate far from the truth, and the error committed could never surmount the quantity of phosphates which I marked by $\frac{1}{4}$, or at the most $\frac{1}{2}$.

In accurately determining the quantity of phosphates present in any of the numbered types, I used the following method: Four ounces of urine are made ammoniacal by the addition of a few drops to ammonia; the precipitation thereby produced is redissolved by acetic acid, and then a sufficient quantity of a solution of oxalate of potash is added; a precipitation of oxalate of lime takes place, and this having subsided, after about twenty-four hours the urine is filtered. Thereby a residue of oxalate of lime and a filtered fluid is obtained. The oxalate of lime is then burned with the filter, and carbonate of lime results. Now, on gently lifting the lid of the crucible, a few drops (3 to 5) of concentrated sulphuric acid are added; the acid is evaporated at about 200° , and then the sulphate of lime is strongly burned. The resulting weight of sulphate of lime multiplied by 0.7647 gives the accurate amount of phosphate of lime, the weight of the filter-ash having been previously deducted. To the filtered urine is then added a few drops of ammonia, and a precipitate results, which consists of a phosphate of ammonia and magnesia, (triple phosphate.) After a few hours this precipitation is collected on a filter, and the residue, burnt with the filter and weighed, indicates the exact quantity of pyrophosphate of magnesia, the weight of the filter-ash having been here also previously subtracted. By summing up the quantity of phosphate of lime and of phosphate of magnesia, we shall obtain then the exact quantity of earthy phosphates which is contained in four ounces of urine, from which may be easily calculated the quantity contained in one ounce, or in 1000 parts of urine. (This method of analysis has been investigated by Dr. Heintz, at Berlin, and kindly communicated to me by Dr. Boecker.)

The following now are the results of the analyses which I have performed in this way, on which we may depend the more as most of them have been twice repeated. First, the urine, which I marked by 0, always contains a very small quan-

tity of phosphates ; this quantity, however, cannot be detected in the above-described way, but it never seems to exceed the quantity of 0·2000 grains in one ounce of urine. There are many steps between no phosphates at all and of 0·2000 grains ; they require a more accurate study in order to show the preternatural decrease of the quantity of earthy phosphates in the urine. But as we shall consider here only the hypernormal increase of the quantity of phosphates which are excreted in the urine in diseases, I am compelled to waive this discussion at present. I have only to state, that in every case we may consider a urine marked by 0 as containing 0·2000 grains of earthy phosphates, or less ; never more than this in one ounce. With respect to the other descriptions of urine, I should really far exceed the limits of this paper by adducing the results of the single analyses ; they can be seen in a pamphlet which I have lately published in Germany, entitled, “*Zur Physiologie und Pathologie des Phosphorsäuren und Oxalsäuren Kalkes.*” Goettingen, 1850. Here I only beg to state the general results as follows :—

A urine marked by $\frac{1}{2}$ contained nearly 0·250—0·300 grains of earthy phosphates in one ounce ; a urine marked by 1, 0·400—0·450 grains ; a urine marked by $1\frac{1}{2}$, 0·550—0·600 grains ; a urine marked by 2, 0·700—0·750 grains ; a urine marked by 3, 1·000—1·050 grains ; and lastly, a urine marked by 3—4, 1·000—1·3000 and more grains of earthy phosphates. By referring to these numbers we may easily approximatively calculate the quantity of earthy phosphates voided in twenty-four hours, and I am sure we shall never be far from the truth.

After these explanations I have to speak of the different quantities of phosphates which I met with in the urine in different diseases. My observations of course do not extend to all diseases ; it would scarcely be possible to give such accounts in a large number of years, notwithstanding I observed a sufficient number of cases with respect to this point for the deduction of some general results.

First, I have to remark generally, that scarcely any disease occurs, in the course of which we should not sometimes find an increased quantity of phosphates ; that at any rate there exists no disease which does not admit of some hypernormal excretion of phosphates at some one of its periods ; on the other hand we meet at different periods of disease with quite different quantities of phosphates, as, for instance, it often happens that in the first stages of diseases we do not find an increased quantity of phosphates at all, and that at a later period a large quantity is excreted. With respect to this point, and in order to obtain results which can be depended upon, it is therefore indispensa-

bly necessary to examine the amount of phosphates almost every day; we shall never arrive at correct views if we do not attend to this rule.

Secondly, it must be stated as a general result, that the quantity of phosphates excreted does not depend as well upon the nature of the disease itself, as upon the individual afflicted; and if in one case of rheumatism we find, for instance, a large amount of phosphates in the urine, we do not detect any increase at all, perhaps, in another case. This point really is a very important one: it affords the best proof of the general fact, that we are always wrong in speaking of certain diseases as of individuals, or as of well defined and marked never-varying alterations of the physiological state of the body, and that we shall never succeed in obtaining positive results, if we do not direct the most accurate attention to the previous history and the former state of the individual who has become afflicted with any disease—in other words, if we do not individualize disease.

It may be concluded from these short remarks, that it is very difficult to give an account of the excretion of phosphates, generally applicable and absolutely right in almost every case. However, we meet with some pathological states which, generally speaking, very rarely show an increase of the excretion of phosphates; with other affections which always show an increased quantity of phosphates in the urine and even a most anomalous quantity; and with others which are generally distinguished by a slighter, but continued loss of phosphates. To these states and affections I shall now draw attention, and I scarcely know how to give a better explanation of them, than by referring to the numbers above alluded to.

I have met then with urine containing only such a quantity of phosphates as I have marked by 0 or $\frac{1}{2}$ —

1st. In persons who, always showing a good state of health, a normal complexion and colour, and a strong constitution, have accidentally become afflicted with disease or injury, as, for instance, with syphilis, wounds, contusions, &c. As I really considered these persons as nearly healthy, at least for so long a time as the affection remained a local one, I am inclined to view the quantity of 0.1000—0.2000 grains of earthy phosphates in one ounce of urine as nearly the normal quantity. It must, however, be remembered, that it is always extremely difficult to speak of normal states of health, a precise distinction between health and disease being incompatible with our present amount of knowledge, if conceivable at all.

2nd. In the first stages of acute diseases, as, for instance, of acute rheumatism, p \acute{n} eumonia, pleuritis, peritonitis, &c. When these diseases happened in persons who did not exhibit any

other signs of diseased constitution, and had never been ill before, I did not, either during the whole course of the disease nor upon recovery, find an increased quantity of phosphates in the urine. But it was seldom I met with such persons. When, on the other hand, persons were afflicted with acute diseases who never enjoyed good health before, or suffered from dyscrasia of the blood, I almost always met with an abnormal quantity of phosphates in the urine after the acute stage having ceased; there appeared now all the symptoms of the original dyscrasia, and either the convalescence was a very slow one, or emaciation, general weakness, &c., was still increasing; instead of acute tuberculosis, the symptoms of chronic affection of the lungs appeared; instead of acute rheumatism, chronic rheumatism remained. From these very remarkable differences I concluded with certainty, that it was not the disease itself which caused a decrease of the excretion of phosphates, but that this decrease was exclusively dependent upon the acuteness of symptoms, that is to say, the feverish action—a circumstance which I shall refer to in the following parts of this paper. It must also be mentioned, that in some cases of acute disease I met with a quantity of phosphates, as marked by $\frac{3}{4}$ or 1, even during the first periods; in these cases, however, the quantity of phosphates was very considerable after the acute symptoms having ceased.

3rd. In the first stages of typhus fever. Here I always met with a decrease of the quantity of earthy phosphates in the urine a result which, after a great many analyses, seems to be a characteristic one. With respect to the later periods of typhus, however, the same refers to them as I have stated on acute diseases generally.

4th. In some cases of Bright's disease, as well as in some persons who suffered from stenosis of the orifices of the heart, or from insufficiency of their valvules. But some cases also occurred where an absolute increase of phosphates was met with; however, in these cases complications or affections of other organs could be observed; and I am inclined to suppose that the dyscrasia of the blood, which leads to the well known degeneration of the kidneys, as well as the dyscrasia which results from the above-mentioned diseases of the heart, does not cause by itself any increase of the earthy phosphates in the urine.

5th. In the first stages of carcinoma, (but only in these;) an observation the more interesting, that Rokitansky alludes to the preternatural development of the bones, or the proportion of phosphate of lime in persons afflicted with cancer. I myself found, in a post-mortem examination of an individual who died from carcinoma of the lungs, all the cartilages of the ribs ossi-

fied; which was never the case in persons who during life passed for a long time increased quantities of phosphates, and had been afflicted, for instance, with tuberculosis.

I have to add generally, that in all cases where I observed no increase of the phosphates in the urine during *the whole course* of a disease, I likewise never observed emaciation; that is, want of formation of cells; these persons altogether were of strong constitution and showed a remarkable development of the muscles. In these persons blistered surfaces healed far more speedily than was the case in persons who passed a hypernormal quantity of phosphates, the average time being three or four days. Of course in acute diseases emaciation was observed, though no abnormal loss of phosphates could be detected; the same was the case with Bright's disease. In these affections, however, many circumstances concur, which sufficiently account for the waste of tissues—circumstances which do not require any further explanation.

With respect to the quantities of phosphates which I marked by $\frac{1}{2}$, 1, $1\frac{1}{2}$, and even 2, I found them to be present in very different and nearly all sorts of diseases, varying on different days in one case, and always remaining of nearly the same amount in others. These cases proved in a most remarkable manner that it is not the disease itself which causes the excretion of phosphates, but that there must necessarily exist some other cause in the economy, to account for the excretion alluded to. I observed the mentioned quantity of phosphates, that is to say, from about 0.350 to 0.700 grains in one ounce of urine, in chronic rheumatism of the joints or muscles, in the chronic stage of tuberculosis, in the ulcerative stage of carcinoma, in different sorts of chronic diseases of the skin, such as eczema, impetigo, ecthyma, and boils, slight cases of scrofula, syphilis, emphysema of the lungs, and especially in cases of chronic dyspepsia, hæmorrhoids, &c. It is impossible to give any special account of the quantities observed, unless I give a special explanation of every case, an explanation which would too far exceed the extent of these papers; moreover I shall speak about the cause of the excretion of phosphates in the following part of this communication, and we shall become acquainted thereby with those circumstances on which the excretion of the different quantities, in different diseases depends; but I have to state, that the general complexion of patients in these cases rarely surpassed what we call the middle degree and often even did not surpass an emaciated, feeble state of health; these patients do not get fat or muscular, in spite of large quantities of wholesome food; on the contrary, the more they take, the less they gain strength and power; indeed, as long as the excre-

tion of phosphates is abnormally increased, the formation of cells is impaired.

Thirdly, I have to speak of diseases in which I met with the largest quantities of phosphates, such as marked 2, $2\frac{1}{2}$, 3, and 4. These quantities have been found :—

1st. In scrofula and rickets. Especially in children afflicted with scrofula and ulcers, or other diseases of the skin, as impetigo, boils, &c., with diarrhœa and atrophy, I found large quantities of phosphates passed by the urine ; in one case of inflammation in the kneecjoint, in a young man who was afflicted with scrofula, the quantity of earthy phosphates passed amounted to nearly a drachm every day.

2nd. In tuberculosis, especially in those stages where the very acute state and deposition of tubercles in any organ has ceased, a slight fever, however, and the dyscrasia of the blood continues. I did not observe any increased quantity of phosphates during the first attacks of acute tuberculosis, exhibiting the well known symptoms of typhus : but as soon as the localization had taken place and an exudation in the lungs was produced, the general symptoms decreasing in acuteness, the phosphates appeared in the urine in an increased proportion, and soon amounted to 0·8, 0·9, or even 1 grain in one ounce of urine. This being the case, I was rather puzzled at not finding any increase of phosphates during the very last days of some persons dying of tuberculosis. But also this point I shall account for in the following part of this paper.

3rd. In such cases of acute rheumatism which occur in anæmic individuals, and are well known to be accompanied by a remarkable loss of flesh, or by a want of formation of cells. I have mentioned already, that in the acute stages of these cases, I met with no increased proportion of phosphates at all ; but as soon as the acute stage was over, the phosphates appeared, and then rapidly increased to nearly the same amount as I observed it in tuberculosis.

4th. In the last stages of carcinomatous and some other affections, yielding large quantities of pus or ichorous secretions, such as psoas abscess, suppuration of the cellular tissue, &c. In these very cases, the general emaciation of the patients was very remarkable, although readily accounted for, if we take into consideration, that for the formation of cells three substances are required—namely, albumen, fat, and phosphate of lime, and that two of these were evidently excreted from the economy in an immense quantity ; on the one hand, by the suppuration ; by the urine on the other. However I also met with some rare cases in which a remarkable emaciation took place, in consequence of abundant suppurations, without a remarkable increa-

crease of the phosphates having been observed. But these cases could scarcely afford any objection to my other results, the suppuration being so large as sufficiently to account for the emaciation itself. It is self-evident, that in all cases where emaciation takes place, we must not only look at the quantity of phosphates eliminated from the economy, but likewise at the quantity of albuminous and fatty substances which have been taken as food during the course of the affection, which are present at its commencement, and which are eliminated in the form of pus, serous exudations, urea, &c.

With respect to the other cases referred to, it must also be generally stated, that in almost every case the emaciation was a very rapid one during the excretion of large quantities of phosphates, and sores of blisters applied to these individuals scarcely healed at all, or at least not till after a long time. We may provide persons who are afflicted with these diseases with the largest quantities of albumen and fat, but we never shall produce thereby a remarkable increase of tissues or complexion—that is to say, of formation of cells, if we do not diminish at once the excretion of the phosphates by the urine. The disease will not cease as long as their excretion continues, but if a decrease of the phosphates in the urine takes place in an early stage of the affection, we may be induced thereby to believe that the condition is really an improving one. On the other hand, it should be remarked that the nature of the diseases alluded to is a very different one; that therefore, the hypernormal excretion of phosphates is also here independent of the disease itself, and that there must exist some other cause of it, the investigation of which will be the subject of the following part of this paper.

These are the chief points which I have to refer to, with respect to the different quantities of phosphates excreted from the economy in different diseases. The general conclusions are, that a hypernormal excretion of earthy phosphates by the urine is independent of the nature of the disease; that wherever we observe such an excretion, we find a corresponding deficiency of formation of cells, emaciation, and loss of strength; but that, finally, this deficiency of formation of cells is not always exclusively caused by a hypernormal loss of phosphates; that, on the contrary, it is often only the result of fever, of suppuration, of want of nourishment, or of any other loss of materials necessary for the regeneration of tissues and organs.

There is one point more, which I have to direct the attention to. It having been well known for a long time and more accurately established by the inquiries of Dr. Bence Jones, that the quality and quantity of the urine are very different at different times of the day, I also examined the quantities of phos-

phates voided at different periods in twenty-four hours. It is not here the place to explain, that by repeating the very interesting examinations of the urine, published by Dr. Bence Jones, in the *Philosophical Transactions*, Part II. 1849, p. 235, I did not obtain quite the same results as this able physician respecting the decrease and increase of the acidity of the urine; I only have to remark with respect to the phosphates, that this quantity also is very varying at different periods of the twenty-four hours. But, from a great many analyses of this kind, I could not detect any certain regularity in the excretion of the phosphates; it may only be stated that generally in persons who are known to pass hypernormal quantities of earthy phosphates during twenty-four hours, in one sort of urine, out of three or four sorts passed after dinner, we find the largest quantities of phosphates; and that likewise generally one specimen of urine, out of three specimens passed in the morning, contains a large quantity of them; the other ones showing either a decrease from, or an increase to, the highest amounts, or no hypernormal excretion at all. This being a fact, we shall never be able to judge of the quantity of earthy phosphates present in one ounce of urine of any person, unless we examine the whole quantity which is passed during twenty-four hours. A quantity of urine, passed at a certain time of the day and examined with reference to the phosphates, will always lead to wrong conclusions respecting the whole quantity of phosphates excreted during twenty-four hours. I have carefully attended to this circumstance in my observations, so as to make them trustworthy in this respect; it should never be neglected by others entering upon similar investigations.

To sum up now the results of the first and second part of these communications, I have shown in the first that, supposing a sufficient quantity of albumen and fat to be present, the produce of cells evidently increases by the administration of phosphate of lime; that, on the other hand, by this administration we may promote the cure of diseases which show a deficiency of formation of cells; and that especially in scrofulous affections the administration of phosphate of lime has often proved most beneficial. On the other hand, in the second part, I have established the fact, that in nearly all chronic diseases, where we observe a loss of flesh, emaciation, and general weakness, a hypernormal quantity of phosphates is always excreted from the economy by the urine, and more especially in those cases where the administration of phosphate of lime proved most beneficial. Perhaps it might be supposed that these quantities had been increased by the phosphate of lime taken as a remedy; but this is by no means the case; on the contrary, my observa-

tions prove that, even during the administration of phosphate of lime, the quantity of earthy phosphates in the urine often decreases, supposing a proper treatment in other respects to be employed. Well, then, the harmony of the results of the above two parts is so striking, that we scarcely can admit of any doubt in their truth, and the physiological as well as the pathological importance of the points alluded to is so apparent that it does not require any further explanation. We know that the phosphate of lime is indispensably necessary for the production of cells; we know that in a great many diseases the phosphates are excreted from the economy in very abundant quantities by the urine; and we know even that in these diseases the formation of cells is deficient. Shall we have any doubt that by substituting the quantity of phosphates excreted by the urine, or by removing the cause of their excretion itself, we must afford a great benefit to persons who are afflicted with the diseases alluded to?

There remains one difficult point, which I have to refer to. In the way which I relied upon in determining the quantities of phosphates in the urine, I precipitated the phosphate of magnesia, as well as the phosphate of lime. This having been shown microscopically and chemically, the question arose, whether the results which I spoke of with respect to the phosphate of lime would not require an amendment? It is true the proportions between phosphate of lime and phosphate of magnesia are very different in different urines; however, I rarely observed the quantity of magnesia to exceed the quantity of lime; on the contrary, it was more often found less. I therefore concede, without any hesitation, that the exact quantity of phosphate of lime could not be ascertained in the manner which I adopted; but generally we shall not be far from the truth in supposing half the quantity of earthy phosphates present to consist of phosphate of lime, this being generally below and very rarely beyond the real quantity. I must repeat with respect to this point the above-given remark, that I have only looked for approximative results, and that I believe them sufficient for the conclusions which I have drawn. All these relations demand a very accurate revision; nor can I refrain observing that the proportions between phosphate of lime and phosphate of magnesia, in different diseased persons are highly interesting, so much so as to recommend their most accurate study and exact analysis. In expressing this opinion, I refer, for instance, to a communication in the *Annales de Chimie et de Physique*, Juin, 1849, t. xxvi, 3e serie, entitled, "Recherches sur les Causes du Goitre et du Cretinisme, par M. T. Grange;" but I am compelled to waive the discussion for the present, it being rich enough in itself to form the subject of a special treatise.—[*London Lancet*,

Case of Destruction of the Lower Jaw and of a portion of the Face, under Homœopathic treatment. Novel Operation.
By Prof. F. H. HAMILTON, M. D.

Martin Neuman, 7 years old, was attacked on the 10th of August, 1849, with a mild dysentery. The family were German and sent for a German Homœopathist, who gave him at once small pills which "looked and tasted like sugar!" also a powder and a solution.

Within seven days from the time the medicines were commenced, salivation began, and small ulcers appeared upon the inside of the mouth, upon the gums, &c. Three days after, the ulceration had extended so rapidly that the lower lip was nearly separated, and in a day or two more it fell off entirely. Three months later the greater portion of the lower jaw came away in one piece, being two and a half inches long and including the whole diameter of the bone with its corresponding teeth. The bone and teeth are now in my possession.

It is a coincidence somewhat remarkable, that the sister, Amelia, several years older, was ill in the same way and at the same time (it was during the prevalence of the cholera in this city), and took medicines from the same man, viz: solutions, &c., and within one week she was severely salivated also, and her mouth became ulcerated, but no destruction of bone or of the soft parts ensued.

In January, 1850, the lad was brought to me by his father. The lower jaw was then reproduced through the whole extent of that which had been destroyed, but the teeth were of course not replaced: nor was there a vestige of a lower lip, and even the bone was thinly and imperfectly covered with integument. His condition was distressing in the extreme, since he could masticate only with great difficulty, and his saliva was constantly pouring upon his chin, excoriating his face and neck, and saturating his clothes.

First operation for the restoration of the lip. Jan. 14, 1850, in the presence of the class at the Medical College, I abraded the upper edge of the skin corresponding to the lower lip, to the extent of a quarter of an inch each way from the centre; from either extremity of this horizontal incision I cut perpendicularly about one inch, and then starting from the lower end of these incisions, I carried the knife outward and downward to the left, and outward and downward to the right, one inch and a half. The two lateral pieces thus marked out, were now dissected from the jaw and slid upward and drawn together with sutures above the central piece; the lower edge of the lateral pieces thus united were stitched also to the upper and abraded edge of the central piece.

The object in leaving a central piece attached to the jaw, and uniting the lateral pieces above it, was to prevent the lateral pieces, which were to constitute the new lip, from drawing down again by the contraction of the wound below. The plan was original, I believe, and proved successful. The new lip, however, became, in process of time, through stretching and shrinking, insufficient, and I made a second operation to increase the depth of the lower lip, and prevent more effectually the saliva from dribbling from the mouth.

Second operation, Aug. 28, 1850, at my office, in the presence of Drs. Samuel Carey, Camp, and others. My mode of procedure was entirely new, and as I believe, has established an important principle in this class of operations. The operation was as follows: A single incision was made just under the chin, extending along the lower edge of the inferior maxilla about three inches from side to side. All the integument comprised between this horizontal incision and the upper edge of the lower lip, was now raised from the bone, and the entire mass slid upward until its lower edge was made to correspond with a line just below the upper border of the jaw. Here this edge was made fast to the *periosteum*, by several interrupted sutures. The gaping wound below was left to close by granulation. The result has been that adhesion occurred between the lower edge of the flap, thus secured, and the periosteum, and no disposition was afterwards shown in the flap to draw downward as the wound cicatrized; but on the contrary, the skin from below, that is, from under the chin and the neck, was somewhat drawn upward, and thus between the formation of new skin and contraction from the skin below, the wound closed.

The new principle established is that *by attaching the skin directly to the PERIOSTEUM its displacement by cicatrization, and contraction, is prevented*. Every one who has operated for the restoration of the lower lip will see the advantages which this plan offers. There is nothing to which the upper, free border of the new lip can be attached, and there is consequently nothing but the mere transverse tension of the lip, to prevent its descending as cicatrization progresses below. This tendency I sought to avoid in the first operation by leaving a central piece untouched and adherent to the bone, and then bringing the new lip above it. But this procedure requires a sacrifice of a portion of the transverse diameter of the lip, and is often wholly inadmissible; and always objectionable, if the same end can be attained by another mode. This new mode, as we have demonstrated, prevents the sliding downward, without sacrificing any portion of the lip. These remarks are applicable especially to cases of *complete* loss of the lip. Where

only a portion is lost, various other methods of supplying the deficiency may be practiced; as by stretching the lip, or sliding from the cheeks, or even by an operation of "torsion" from the cheeks.

This idea originated in having observed elsewhere the capacity of periosteum to form skin. I have several times proved, contrary to the often repeated doctrine, that skin may form *de novo*, independent of old skin: as where there has been an extensive destruction of the integuments over a bone—where the parts have been torn away or have sloughed quite to the periosteum, and consequently no old skin could have been left from which the new could form except at the edges; yet in the very centre of this broad ulcer new skin has sprung up like an oasis, and gradually spread outward in all directions. But this has always been where the periosteum was actually exposed, which first becoming white and spongy, has soon shown itself to be the nucleus of a new skin—in fact it has become *itself converted into skin*, remaining ever afterwards depressed, immovable and adherent to the bone at that point.

The result of the case of the lad Neuman, is that he has a lower lip, sufficient to cover the gums and a part of the bodies of a set of artificial teeth which our ingenious dentist, Dr. Harvey, has made for him. The lip is narrow, for we have not yet been able to prevent the contraction and rolling in of the upper edge as it heals, but it would certainly have been much narrower, or entirely lost if the adhesion to the periosteum had not been effected.

I will not omit to say that by the constant effort to use the lower lip, or perhaps simply by the lapse of time, the lip has very perceptibly lengthened in its vertical diameter during the last six months.—[*Buffalo Med. Jour.*

Case of Eclampsia Nutans, or "Salaam Convulsions." By
E. C. BIDWELL, M. D., Keene, O.

IN March, 1849, Mr. Newnham published an article on this interesting disease, containing the details of four cases—all that were then known to be on record. The next year, Dr. Willshire reported another case. These two essays comprise, so far as my knowledge extends, the bibliography of the disease. Of these five cases, the last only had a favorable termination, the other four issuing in idiocy or death.

Soon after meeting with Mr. Newnham's essay, a case of the disease came under my own observation; and though differing but little from the cases therein reported, I have thought it

worthy of record, since every case of a malady which has occupied so small a space in medical literature, and, at the same time, of such grave importance, possesses an interest and value which cannot attach to mere repetitions on more familiar subjects.

At the age of three months, the subject of the present report suddenly lost the power of motion. She was afflicted at the time with very obstinate costiveness; further particulars not recollected. She was treated with cathartics, &c., and was promptly relieved. About the same time, some of her relatives began to suspect her of deficient intellect; but with the above mentioned exception, she seemed to enjoy uniform good health; and the parents profess to have noticed nothing wrong with her till she was almost six months old. At that time she was observed, on waking in the morning, and three or four times during the day, to bow or drop her head forwards convulsively. This movement was repeated several times in the course of one or two minutes, and the series repeated three or four times every day. These paroxysms were accompanied by suffusion of the eyes, most marked in the night. There was no expression of pain, and there was, apparently, a momentary loss of consciousness. There was not, at this period, any unusual heat of the head; no flush of the face at any time, which was generally quite pale. The feet were habitually cold; the bowels always costive, and the appetite always good.

With the exception of several slight remissions, of a few weeks perhaps, the peculiar convulsions increased progressively in frequency and intensity. When she was one year old, the paroxysms recurred very frequently, almost always soon after waking from sleep, and at other times also, with thirty or forty convulsions in rapid succession in each, and accompanied with a cry as if from pain. By this time it had become evident that her mental development was very much retarded, if not wholly arrested. She had the appearance of a dull child of seven or eight months; but her general health seemed unaffected, and she was able to walk at thirteen months.

Subsequently, her growth was slow, and the intellect evidently retrograded. Gradually, the morbid movement increased in extent; from the slight nod first described, it became a true oriental "salaam," in which the head was drawn suddenly quite down to the floor, with such violence that the forehead and lips were continually bruised and lacerated from the injuries received. At this period, there was but one convulsion at a time, lasting but an instant, and attracting no attention from the little sufferer herself, except when attended by severe injury. This was repeated many times during the day.

Still later, epileptiform fits were superadded, less frequent, but individually of longer duration. Towards the end of her second year, she was hopelessly epileptic and idiotic. At twenty-six months, she died, after a short illness, with febrile symptoms, and a great aggravation of those peculiar to her case.

For the last few months all treatment was discontinued, previous to which time she had passed through the hands of several different practitioners, regular and empirical; had been blistered behind the ears, and on the neck; pustulated with tartar emetic; bathed with warm, and showered with cold water, separately and consecutively; had taken tonics, cathartics, vermifuges, alteratives, specifics, and what else I am unable to say, all with no sensible effect, unless, possibly, that, while suffering palpably from the drug disease, the morbid motions were less conspicuous. Perhaps none of the temporary amendments or remissions, already noted, could be fairly attributed to any system of medication to which she was subjected. The parents think, that one remedy, and one only, had a decided effect on the disease: the decoction of yellow dock, with a small quantity of bichloride of mercury, was taken for a considerable period, during which the habitual costiveness was entirely obviated, and for the same period the characteristic symptoms were ameliorated. The costiveness and the convulsions returned together immediately on its discontinuance.

The principal points of the disease, as exhibited in this case and in most of those previously reported, may be recapitulated thus; the diagnostic nodding or bowing of the head; its occurrence soon after sleep, though it also occurred at other times; the subsequent accession of another form of convulsive movement, intercurrent with the original; the permanent injury to the intellect; and finally, its irremediable character, and fatal result. In one important particular only did it differ from the most of those—there was no paralysis.

I have been thus minute, at the risk of being tedious, in reporting this case, upon principle, believing that clinical reports are valuable very nearly in proportion to their completeness. Doubtless, the reader will consider the space better occupied with these details than with any speculations or hypotheses of mine, which are therefore omitted.—[*New York Journ. of Med.*

On Eclampsia Nutans. By Dr. FABER.

Dr. Faber relates two cases of this curious affection. The *first* occurred in a girl, æt. 3, who though pale and weakly had not suffered from any decided disease, until three months before, when she complained of headache and sleepiness, began

to squint somewhat, and sometimes to nod her head towards the left side. This nodding action was at first continued only for a few minutes three times a day, during which the head was making constant salutation-movements. After a while the attacks increased in frequency, and were fearfully violent. The child was much disposed to sleep; and became, on waking, convulsed in the extremities, this passing on to complete epilepsy. She was backward in mental development, and had an idiotic expression of countenance. The *second* occurred in a boy, æt. 6, who showed good capabilities for instruction up to the commencement of his sixth year, when he fell into the water. He remained in bed several days after in a drowsy state, and never again so lively and quick. After a while he was observed to nod his head for two or three minutes, and this several times in the course of the day, the motions being sometimes so rapid that eighty could be counted in a minute. They commenced at first slowly, like real salutations, but gradually increased in quickness, when the child would fall back in a passion. During the time they continued, his face was distorted, and great fatigue was induced. He was aware when the attacks were coming on, and his consciousness continued during their prevalence. He was pale and feeble, and had acquired a peculiar, stupid look.

The reporter inquires whether this is a partial chorea or a peculiar form of spasm, and whether it is dependent upon morbid conditions of the brain or spinal marrow. The most careful examination of his two cases did not enable him to decide. No pain or tenderness about the head or spine were discoverable, and nothing abnormal in the general condition. Various applications and medicinal substances were resorted to with but very little success—iron seeming the most useful among them.—*Brit. and For. Rev.*, from *Schmidt's Jahrb.* vol. lxxvii, p. 213.

Eclampsia, and its relations to Albuminuria in Lying-in Women.

M. Depaul read a note on a case of eclampsia, and submitted observations, of which the following were the conclusions:

1. Convulsions of an epileptic character, are rarely seen in the first four months of pregnancy. The case related is therefore interesting, as having occurred at the end of three months, in a person who previously had not been subject to epilepsy or any other nervous affection.

2. It is erroneous to state that the prognosis of eclampsia is more favorable in proportion as gestation is less advanced, or

according as labor, if it have commenced, is distant from its termination.

3. This opinion is founded upon an inference not drawn from facts, viz., that the paramount indication of treatment is depletion of the uterus.

4. The dangers which threaten, the fœtus consist in the modifications which the maternal blood undergoes, and in the disturbance of the uterine circulation; the death of the fœtus not unfrequently occurs in the course of a convulsive paroxysm. The fœtus resists the causes of destruction more certainly in the earlier than in later months when it is nearer to its perfect state.

5. The best treatment of these cases is full general depletion.

6. Neither paleness of the countenance, smallness of the pulse, nor the presence of albumen in the urine, are contra-indications of depletion.

7. Albuminuria is more frequently met with in pregnancy than in any other physiological state.

8. Without denying the possibility of nephritis in a pregnant woman, the presence of albumen in the urine must generally be regarded merely as a functional derangement. This is confirmed by the results of autopsies.

9. This albuminuria cannot be regarded as the cause of the convulsion, since the latter has often preceded the former.

[*Medical Gazette.*

[The following article, translated for this Journal from the *Revue Médicale* (of Paris), offers a new field for observation and reflection. It bears the impress of a philosophic mind, and well deserves the attention of the profession. It is high time to abandon the vagaries which have so long obtained in relation to the causes of disease and to direct our attention to something more tangible and comprehensible. The works of M. Raspail, in France, and Dr. J. K. Mitchell, in our country, have done much already to direct attention to the subject.]

On the Nature of Viruses.

Dr. Hameau's work upon the Nature of Viruses and the laws which govern their action in the economy, was made the subject of a Report by M. Londe to the Academy of Medicine of Paris, from which we draw the following details :

According to M. Hameau, poisons, miasmata and venoms, affect the economy instantaneously when in sufficient quan-

tity, but their effect continually decreases in intensity during its continuance; they are themselves decomposed at the same time that they exert a decomposing influence; they, in short, obey the laws of chemical affinities; whereas viruses act as living beings, or as insects. The following are M. H.'s corollaries:

1st. Any heterogeneous substance that may introduce itself into a living body, remain inactive in it for a certain length of time, multiply in it, and then leave it to act in the same manner upon another body, would seem to possess a vital principle.

2d. Its mode of action resembles closely that of insects which introduce themselves into plants and animals. The small-pox, for example, appears to develop itself in the same manner as fecundated germs of insects, for, like these, it requires a foreign nidus in which it may derive nourishment, be incubated and developed.

3d. Even when it causes death it is unlike poisons, venoms and miasmata, for these, in acting, lose their own force, are decomposed, and can exert no influence until they have subdued the vital powers, whereas it, on the contrary, acquires activity, multiplies, and is unharmed by the vital forces.

4th. This matter especially resembles the itch *acarus* which multiplies its generations in our tissues. The itch might be taken as the type of all affections of virulent origin.

5th. It is indelibly characterized by contagion, incubation and reproduction. The cause of any disease which combines these three features is a virus. These features vary in each: the period of incubation is different; they do not all multiply with equal rapidity, nor are they all equally contagious.

6th. Some viruses are *persisting*, and others *temporary* or *migratory*. The former never leave the system of their own accord, as syphilis, the itch, pellagra, &c.; the latter leave the system after a certain length of time, as the small-pox, scarlatina, measles, &c. The persisting, after having been driven from the body, may return indefinitely; but the temporary do not return after leaving.

7th. There is an antipathy between certain viruses, at least for a certain time, so that a body whilst under the influence of the one is exempt from the invasion of the other, which must be owing to an opposition in their nature, and also to certain excrementitious particles left by the affecting virus. There is also a repulsion between the temporary viruses and the bodies they have left, inasmuch as they do not return to them, which must also be owing to their having left in the bodies something disagreeable to them. The cause of this repulsion is similar to that which makes all animals loathe their excrements.

8th. Each climate appears to have viruses proper to it, although these may be transported at a great distance from their origin. Yellow fever appears to have originated in Africa; syphilis, in America; variola and cholera, in India; the itch, malignant pustule, and perhaps typhus, in Europe.

9th. There are viruses proper to various species of animals, such as farcy and glanders in horses; the rot in sheep. Those that affect man do not affect brutes, and vice-versa; yet there are some exceptions to this. I have, says the author, strong reasons to believe that we have derived pellagra from the sheep, and I have seen an awful instance of the glanders communicated to man.

10th. Every cause of sickness that may travel or change climate is a virus. It could not run over great distances if it did not reproduce itself; therefore, of all causes of disease, none but the virulent have the power of reproduction. It cannot be a miasm, because all miasmata being made up of dead and decomposing substances, must also themselves be soon decomposed under the influence of the physical and chemical laws, whereas a virus is not subject to these laws.

11th. All viruses might be divided into two classes, the visible and the invisible or aerial. The author terms visible virus all liquids produced by a disease, which, it matters not how, may produce the same disease, by manifesting the three characteristics above mentioned, (contagion, incubation, and reproduction.) The liquids furnished by the itch, variola, vaccine and syphilis, are visible viruses. The invisible are those ethereal substances which manifest themselves to our senses only by the diseases they induce, but which reveal to the judicious observer the three characteristics of the virus.

Blondlot on the Use of Bile.

The *Revue Médicale* (August, 1851) contains a synopsis of a memoir presented to the Académie de Médecine, by M. Blondlot, upon the use of bile, which we think worthy of notice. The author endeavors to determine whether bile is essential to digestion or whether it is a mere excretion which serves no purpose in the function of digestion. He thinks that he established in 1846, in a thesis upon the functions of the liver, that the bile is an excretion, which passes out of the economy through the intestines, and has no chemical action upon the food with which it comes in contact. After many unsuccessful attempts M. Blondlot succeeded in forming a fistula in two dogs, which allowed the bile to pass out through it. In

these dogs the ductus choledochus was obliterated so that the bile could not enter the intestines. One of the animals was killed a month after the fistula had been established, during which time digestion had been regularly performed, although the ductus choledochus was obliterated. The second animal was preserved in order to follow up the experiments. This bitch survived five years, with good appetite, hunted with vivacity, and annually produced a litter of puppies. The flow of the bile was observed to be intermittent, it was slight when the animal was fasting, but abundant during digestion.

The autopsy showed all the organs of the chest and abdomen healthy, except the liver. This organ was hard, and its surface strewed with yellowish points; in short, it presented the appearance of a cirrhused liver. Solid adhesions united the bladder to the walls of the abdomen. The gall bladder retained its dimensions and form, though its walls were a little thickened. The cystic duct was dilated, and at the point of junction of this with the hepatic duct the origin of the ductus choledochus could be seen, this being obliterated.

The author concludes, from these experiments, that the bile is not at all essential to digestion, but is entirely an excrementitious fluid. If it is of any importance it is secondary, and may perhaps, with the mucus, form an emulsion with the fatty matters and protect the intestines against the acidity of the chyme, which it in a measure neutralizes.

NOTE.—What kind of food did the dog eat? Did the kidneys sympathize?

False Aneurism of the Brachial Artery Cured by Galvano-Puncture. By M. AMUSSAT.

M. Amussat has related the following case before the Academy of Medicine of Paris:—A butcher, aged 35, wounded the internal and lower part of the arm with a penknife; the artery was injured, compression used and seventeen days afterwards the man was sent to M. Amussat with a pulsating tumor, the size of a hen's egg, on the spot where the wound had been inflicted.

Mr. Amussat, relying on the accounts published by M. Pétrequin of Lyons, tried galvano-puncture in the following manner:—Two fine platinum needles, covered with gum lac over that portion which was to be in contact with the skin, were introduced into the tumor, and the poles of a trough of thirty compartments brought into contact with the needles for the space of five minutes. The couples were gradually increased to twelve, and after five more minutes (giving altogether ten)

the needles were withdrawn, because the patient was in great pain. Lead wash was then applied to the tumor; no unpleasant symptoms occurred, and three days afterwards the aneurism presented pulsations over an area the size of a five-shilling piece. Four needles were now introduced; this sitting lasted sixteen minutes, and the couples were increased to sixteen. The same dressings were used as before, and in four days the tumor was found smaller, harder, presented no pulsations, and had assumed a brownish color. The swelling diminished in size from day to day, the pulsations never returned, and the cure may be looked upon as quite complete, as M. Amussat presented the patient to the Academy in July, 1851, whilst the operation had been performed on the 13th of October, 1847. After almost three years the value of such a case has very naturally remarkably increased.—[*Lancet*.

Hydrastis Canadensis in Gonorrhœa. By D. M. McCANN, of Ohio.

As it has for its object the diffusion of knowledge advantageous to the Medical Profession, permit me to call the attention of the profession through the columns of your excellent Medical Journal, to the use of *Hydrastis canadensis*, (yellow root, orange root,) in gonorrhœa.

I am not aware that any of my brethren have ever used it in this affection, before myself. My experience, however, in the administration of it, though not extensive, is yet sufficient to warrant me in soliciting a trial of it by those having more opportunity of testing its curative powers than I have. I have used it in several cases in various stages of the disorder, and in every case with the most satisfactory results; more especially with males than females. I was led to its use by noticing its well-known sanative properties over inflammations of mucous and epithelial structures, such as aphthæ of the mouth, &c. The ardor urinæ, and discharge of mucus, has been entirely suspended in every case in from twenty-four to seventy-two hours. In some cases I used the balsam copaiba, in others injections of infusion of the hydrastis alone, but with about the same results, a perfect and permanent eradication of the disorder.

I have varied the strength to suit the case in its different stages, but as a general rule I have used about one drachm of the dried root to the pint of infusion—injecting a syringe full three or four times a day.

I hope that some of the profession will give this article a fair trial.—[*Ohio Med. and Surg. Jour.*

[The following cure for Tape Worm was communicated to the Boston Medical and Surgical Journal by a gentleman whose respectability is known to the Editor of that Journal:]

Cure for Tape Worm.—Procure sufficient seed of the pumpkin (those grown in the West Indies are the best) to make two ounces after removing the outside shell of the seed; put them into a mortar and add half a pint of water; pound them well up, and make a liquid orgeat of them, which strain through a cloth. Drink this mixture in the morning on a fasting stomach. If it does not operate in the course of an hour and a half, take one ounce of castor oil. Drink all the time as much fresh cold water as the stomach can bear or contain; that is, drench yourself with water. After taking the orgeat, if the stomach is well rubbed with ether, and an injection of about 60 drops of it is taken, you will find it an assistant to the orgeat, but this may not be necessary. Should the first application of the remedy not answer, repeat it the next morning, and there is no doubt your complaint will be removed. The worm will leave the patient all at once, and probably entire. This can be ascertained by finding the small end or head of it, which tapers off almost to a point.

[Since the publication of the above, the Boston Journal has been furnished with a case in which the remedy was completely and promptly successful.]

On the Histological Nervous Elements in Adhesions. By
Dr. VIRSCHOW.

The author observes that he had undertaken the examination of adhesions with the especial object of ascertaining whether nervous fibres were to be discovered in those structures. He had searched in vain a long time, but had at last twice succeeded in detecting them, by the addition of a solution of soda to the preparation. The first was in an adhesion that had been formed between the surface of the lung and the side of the thorax: here he had discovered two thick nerve-tubes running a parallel but perfectly distinct course, and following the direction of the elastic fibres. They presented a double contour: their contents were here and there aggregated, so as to impart a varicose aspect; their anastomoses were not perceptible; it was not possible to mistake them. The second occasion was in examining a flat adhesion from between the liver and diaphragm. The nerve-tubes had the same appearance as the above:

they did not traverse the entire adhesion, but terminated by pointed ends at certain distances. A short distance before it divided it sent off a branch, which proceeded for about half a line and then terminated by a pointed extremity. In this case the nerve was clearly traced from the diaphragm. In both these cases numerous other adhesions were examined, without discovering any trace of nerve.—[*Medical Gazette*, from *Verhandlungen der Physicalisch-Medicinischen Gesellschaft in Wurzburg*, 1850. *New York Journ. of Med.*

New View of the Supply of Blood to the Muscular Fibres of the Heart.

Dr. Spurgin in the Harveian oration, delivered by him this year, propounded a new view of the supply of blood to the muscular fibre of the heart. He argued that that supply could not be derived from the coronary arteries, as is universally taught; but that it flowed through certain foramina in the cavities of the heart itself, to which all the coronary vessels stood in the relation of veins. In confirmation of this view he pointed out a peculiarity in the structure of the coronary arteries, and referred to medical history, which afforded an instance of a total obliteration of the passage through those arteries, without causing a cessation of life.—[*Med. News and Library*.

Gastrotomy successfully performed for Extra-Uterine Pregnancy. By Drs. BRADLEY and ROGERS.

The patient was a negro woman, aged 28 years, the mother of seven children. In June, 1849, six weeks after conception, she began to complain of colic, attended with constipation. On the 10th of February, 1850, she supposed herself in labor. On examination, the os uteri was natural and the breasts were flabby. There had been no movement of the child felt since the middle of November, at which time there was milk in the breasts. A tumor filling the whole right lumbar region, extending above the hypochondriac, and below to the iliac region, and somewhat to the left of the umbilicus. There was also present considerable febrile disturbance. She was certain that she had felt the motions of the child, from the fourth or fifth to the seventh or eighth month, when she supposed it died. The diagnosis being extra-uterine pregnancy, the removal of the child was recommended.

On the 7th of February, 1851, the patient having been previously prepared, chloroform was administered. An incision

was made extending for two inches above the umbilicus to the pubes. The foetus was found in the right fallopian tube, fully formed and about the size of a seventh-month child. But little decomposition had taken place. It was firmly attached to the peritoneum, anteriorly and posteriorly, and latterly to the uterus. In separating the attachment, the epidermis of the child was removed at the adherent portions. After removal, the parts were carefully cleansed, and four sutures, with sufficient adhesive plaster, were used, and an opiate was ordered. Her recovery was rapid, so much so that, four weeks after the operation, it was complete.—[*N. O. Med. & Surg. Jour.*

A case of Impregnation prior to Menstruation. By HENRY RITCHIE, M. D., of Chicago, Ill.

Mrs. —, aged 21 years, a Swede, stout, apparently healthy, and well developed, came to us (with an interpreter, as she spoke our language imperfectly) about four weeks ago, stating that she had been married sixteen months, had never menstruated in her life, was anxious as well as her husband for progeny, had been under one physician in the city, but that his medicines did her no good. She said her situation made her melancholy, because she was not like other women, was afraid her husband would not love her. She frequently had pains in her loins and down her thighs, but not periodically. We saw her husband several days after, and told him what we thought was the matter; and that she must subject herself to an examination, which was cheerfully assented to by both. Found the mammary glands well developed, the person full, the abdomen well rounded and plump; per vaginam, the cervix uteri could be felt much longer apparently than natural, the end of it projecting but slightly into the vagina, with neither anterior or posterior lip. The os being a mere slit in a flat surface rather concave than convex, resembling a thin section of a cylinder three quarters of an inch in diameter, the neck from this point gradually tapering back, was lost to the sense of touch without being able to trace the body of the uterus, though in a subsequent examination it could be felt per rectum. Venereal orgasm intact. Diagnosed retained menstrual secretion from some abnormal condition of uterus. Concluded it would be perfectly justifiable to explore the uterus. Commenced with a small gum catheter and stiletto, did not succeed in introducing it into the os after repeated efforts; as we had no speculum along with us, suspended further efforts until the following day, when we introduced a speculum, found the os

and the vicinity quite vascular with ecchymosed spots about it, no doubt the result of manipulation in endeavoring to introduce the instrument the previous day. We now succeeded in introducing a small bougie, transfixed with a stillett about three quarters of an inch, which was followed by a large one, but neither with the force we thought justifiable, would the instrument penetrate further. We then concluded that the obstruction was also at the os internum, or that the cavity of the uterus was occluded. Requested her at the beginning of the following week to come to our office, for the benefit of a consultation with my partner Dr. D. Upon her response to my request, my partner examined her, coinciding with my views of the case. We concluded that it would be perfectly justifiable to use cutting instrument to get into the cavity of the uterus, conjecturing that probably, there was fibrous membrane that precluded the ingress of the instrument and egress of the menstrual fluid. We prepared a sharp pointed instrument of sufficient length, and introduced it about the same distance as we had previously done the catheter. On the application of force, the instrument appeared to be entering something very resisting and tough in its structure; when the instrument was unhanded and struck, it would vibrate. Some objections now presented themselves to a perseverance in forcing the instrument onwards. What! if there was arrest of development of the uterus, and we should now be in contact with the fundus of the organ, the next effort might send the implement through the walls of the uterus into the cavity of the abdomen; this was not a pleasant reflection. In this dilemma we dismissed her for another week, to give us time to decide what further we could do. Upon her return we used a trocar and elongated the passage so that a catheter would now pass about an inch and a quarter, but there still remained something that resisted the trocar. We still hesitated about increasing the amount of force to drive the instrument deeper. Under this state of uncertainty we concluded to have our opinions fortified by advice, before venturing further, and called in Dr. E. to see the case. From the history and examination, he concluded it was a case of retained menses, and coincided with us that interference to open a passage was justifiable. We proceeded no further at that time, but appointed the next week for her return. One day to her anticipated return, we were sent for. Upon arrival was presented with something that she had passed in the night. At first sight, without reflection, we thought it was the uterus, which had been separated by sphacelus in consequence of the violence done it. We immediately made a per vaginum examination, and our fears were appeased, for the

organ was in statu quo. We then concluded that it was a polypus, that had been detached by our interference, it never occurred to us that it was an ovum, even when the old woman in attendance, suggested in broken English, that probably it was one little baby. Sure enough the old lady was right, for on our return with it to the office, its section disclosed a fœtus about six weeks old.

We saw the patient again this morning with her husband, and both asseverate that never until we manipulated, had any sign of blood come from the part. She also positively reiterates that she never previous to marriage or since, has seen a spot of blood from the part. We have no doubt of the truth of their statements, as there can be no motive for deceiving.

[*Western Lancet.*

A Statistical Report upon Disease of the Heart, derived from a consideration of all the Cases admitted into St. George's Hospital during the last two years and a half. By Dr. BARCLAY.—("Proceedings" of the Royal Med. Chirurg. Society June 24, 1851.).

Rheumatism is first considered as one of its causes. Divided into two nearly equal classes—those really inflammatory or acute, and those less so, or sub-acute—the former class is found to contain sixty-seven cases with cardiac lesion, sixty-four without, and twenty-one doubtful. Endocardial murmur is found not to be certain evidence of disease, even in the most acute cases. Females are slightly more liable to acute rheumatism than males, but less liable to a recurrence of the disease. Females are more decidedly liable in a larger proportion to cardiac complication, and this is especially proved by the existence of friction-sound in the proportion of three females to two males. Cardiac complication exists eighteen or twenty per cent. more frequently in subsequent attacks than in primary ones. It is in the proportion of three to two of all the cases up to the age of twenty-five, and falls very rapidly after that age. The cases of sub-acute and chronic rheumatism furnish no examples of recent inflammation of the heart, but a considerable number of cases of old disease. So far as could be ascertained, these were almost all traceable to previous acute attacks, and were only about one-third of the cases which had previously suffered from acute rheumatism. The postmortem appearances of recent inflammation are found associated with acute rheumatism, with disease of the kidney, with inflammation of the peritoneum and pleura, and with old disease of the

heart, especially when hypertrophy existed, and with turbulent action during life. The cases of old disease of the heart are divided into sixty-one rheumatic, seventy non-rheumatic, and sixty-nine doubtful. They show a very considerable preponderance of males, especially among fatal cases. Up to the age of twenty, almost the whole, and even as far as thirty, more than half the cases are associated with acute rheumatism. In the next twenty years, the non-rheumatic almost doubled the rheumatic cases, and after fifty, there are scarcely any derivable from rheumatism at all. The duration of rheumatic cases, dating from the first attack of acute rheumatism to death, is generally much longer for females than for males, varying in the latter from four to six years; in the former, from twelve to sixteen years. Four out of seven fatal cases of acute rheumatism, and twelve out of eighteen of older standing, are associated with pericarditis, which is always severe and extensive; but universal adhesion is neither the constant nor even the common result of rheumatic pericarditis, and it exists in cases where the previous existence of rheumatism is altogether denied. In valvular disease, there are eighteen rheumatic cases, twenty-three non-rheumatic, and twelve doubtful. The recent cases are all examples of inflammation of the mitral valve. When old and recent disease exist together, and when old disease is seen in different stages, the mitral valve generally appears to have been first attacked, and the aortic secondarily; and hence the preponderance of double valvular lesion in rheumatic cases seems to be due to renewed inflammations at distinct periods. Inflammatory thickening occurs also in several cases in which there had been no rheumatism. Disease of the kidney is associated with two cases of simple recent fibrinous deposit on the valves, and three of recent pericarditis, in which no other cause was known to have been in operation. It seems questionable how far this can be taken as a cause of great thickening of the valves, or of an adherent pericardium. Disease of each set of valves seems to produce, in nearly equal proportions, hypertrophy and dilatation, but aortic regurgitation, especially the latter; atheroma of the aorta, more commonly hypertrophy; adhesion of the pericardium, chiefly dilatation. Disease of the kidney is associated with an immense majority of the cases of hypertrophy, and similarly of all the cases of disease of the kidney; more than a third presented on post-mortem examination more or less of hypertrophy of the heart. A table is appended, in which the post-mortem appearances are arranged, of all the cases in which clinical history threw any light on the disease of the heart found after death.—*Lancet.*

Miscellany.

To the Reader.—As the present number will complete the seventh Volume of the new series of this Journal, it is proper that we tender our thanks to those who have by their subscription, contributed to its support, as well as to those whose valuable communications have given interest to its pages. We have every reason to feel flattered, whether we turn to the respectable list of our readers or refer to that of our able correspondents. The Index appended to this volume, will show no less than forty original papers, besides a large accumulation of matter derived from foreign and domestic periodicals, and making upwards of 300 different articles in the miscellaneous and eclectic departments. We believe that this volume will be found to present a pretty faithful exposé of the advances of our Profession during the current year, and as much of the “medical news” of the day as would comport with the dignity of a scientific Journal.

The objects of this publication are the diffusion of knowledge, the establishment of a medium of communication between Southern practitioners and those of other sections, and consequently the elevation of Southern medical literature. We may therefore earnestly solicit a continuance of patronage, both pecuniary and scientific. Let each of our subscribers endeavor to obtain one more name—and let all who have any thing of interest to communicate, use our pages. We exchange with all the medical periodicals of our country and with the most valuable issues of the European press; so that whatever appears in this Journal will be as widely diffused as if contained in any other work.

We renew our pledge to spare no efforts to make the Journal acceptable to all parties, and have every reason to hope that we will be aided liberally by our professional brethren of the South.

[The unguarded sale of arsenic is so frequently attended with mischievous consequences, that we copy the subjoined law of England in the hope that the subject may be brought before our Legislature. It would, perhaps, be well for the State Medical Society to suggest the propriety of some such enactment in Georgia.]

An Act to Regulate the Sale of Arsenic, 5th June, 1851.—Whereas, the unrestricted sale of Arsenic facilitates the commission of crime: Be it enacted, by the Queen’s most Excellent Majesty, by and with the Advice and Consent of the Lords Spiritual and Temporal, and Commons, in this present Parliament assembled, and by the authority of the same, as follows:

1. Every person who shall sell any Arsenic shall forthwith, and before the delivery of such Arsenic to the purchaser, enter or cause to be entered in a fair and regular manner, in a book or books to be kept by such person for that purpose, in the form set forth in the Schedule to this Act, or to the like effect, a statement of such sale, with the quantity of Arsenic so sold, and the purpose for which such Arsenic is required or stated to be required, and the day of the month and the year of the sale, and the name, place of abode, and condition or occupation of the purchaser, into all which circumstances the person selling such Arsenic is hereby required and authorized to inquire of the purchaser before the delivery to such purchaser of the Arsenic sold, and such entries shall in every case be signed by the person making the same, and shall also be signed by the purchaser, unless such purchaser profess to be unable to write, (in which case the person making the entry hereby required, shall add to the particulars to be entered in relation to such sale, the words, "cannot write,") and, where a witness is hereby required to the sale, shall also be signed by such witness, together with his place of abode.

2. No person shall sell any Arsenic to any person who is unknown to the person selling such Arsenic, unless the sale be made in the presence of a witness who is known to the person selling the Arsenic, and to whom the purchaser is known, and who signs his name, together with his place of abode, to such entries, before the delivery of the Arsenic to the purchaser, and no person shall sell Arsenic to any person other than a person of full age.

3. No person shall sell any Arsenic unless the same be, before the sale thereof, mixed with soot or indigo, in the proportion of one ounce of soot or half an ounce of indigo at the least to one pound of the Arsenic, and so in proportion for any greater or less quantity: Provided, always, that where such Arsenic is stated by the purchaser to be required, not for use in agriculture, but for some other purpose for which such admixture would, according to the representation of the purchaser, render it unfit, such Arsenic may be sold without such admixture, in a quantity not less than ten pounds at any one time.

4. If any person shall sell any Arsenic, save as authorized by this Act, or on any sale of Arsenic shall deliver the same without having made and signed the entries hereby required on such sale, or without having obtained such signature or signatures to such entries as required by this Act, or if any person purchasing any Arsenic shall give false information to the person selling the same in relation to the particulars which said last-mentioned person is hereby authorized to inquire into such purchaser, or if any person shall sign his name as aforesaid as a witness to a sale of Arsenic to a person unknown to the person so signing as witness, every person so offending, shall, for every such offence, upon a summary conviction for the same, before two justices of the peace in *England* or *Ireland*, or before two justices of the peace or the sheriff in *Scotland*, be liable to a penalty not exceeding twenty pounds.

5. Provided, That this Act shall not extend to the sale of Arsenic

when the same forms part of the ingredients of any medicine required to be made up or compounded according to the prescription of a legally qualified medical practitioner, or a member of the medical profession, or to the sale of Arsenic by wholesale to retail dealers, upon orders in writing in the ordinary course of wholesale dealing.

6. In the construction of this Act, the word "Arsenic" shall include Arsenious Acid and the Arsenites, Arsenic Acid and the Arseniates, and all other colorless poisonous preparations of Arsenic.

THE SCHEDULE.

Day of Sale.	Name and Surname of Purchaser.	Purchaser's Place of Abode.	Condition or Occupation.	Quantity of Arsenic sold.	Purpose for which required.
1 September, 1851.	John Thomas.	Hendon Elm Farm.	Farm Laborer.	5 pounds.	To steep Wheat.

(*Purchaser's Signature.*) *Witness.* (*Seller's Signature.*)
 John Thomas. James Stone. George Wood.

Or if purchaser cannot write, *Grove Farm, Hendon.*
Seller to put here the words,
'cannot write.' [London Pharm. Journ., July, 1851.]

The State Medical Board.—In a pamphlet I published a short time since, containing the "*The Laws of the State of Georgia in Relation to the Practice of Medicine,*" there is an error on page 7, which I wish to correct through the Medical Journal. The list of members constituting the State Medical Board, is, in my pamphlet incomplete. I have, through the kindness of Dr. Thos. F. Green, of Milledgeville, been furnished by the Dean with the following list of the Medical Board as at present organized:

L. D. Ford, }	} Augusta,	W. J. Johnson, Fort Gaines,	
I. P. Garvin, }		H. K. Burroughs, Savannah,	
R. M. Moore, Athens,		M. A. Franklin, }	} Macon,
J. Branham, Eatonton,		J. M. Green, }	
B. F. Keene, Hillsboro',		T. Fort,	
E. A. Broddus, Monticello,		B. A. White,	} Milledgeville.
H. T. Shaw, Covington,		C. J. Paine,	
R. Banks, Gainesville,		T. F. Green,	
G. D. Phillips, Clarksville,		G. D. Case,	
J. Persons, Columbus,		S. G. White,	

In the above list are one or two names which have been added since the last meeting of the Legislature.

Although the error is not a very important one, *per se*, I shall, nevertheless, be obliged if you will insert this in the Journal.

Yours, most truly,

C. T. QUINTARD.

Cor. Sec'y Med. Soc. State of Georgia.

Roswell, 20th Oct., 1851.

Ichthyosis Cornea.—Dr. Austin L. Sands, of Cold Spring, N. Y., reports in the N. Y. Journal of Medicine, the case of a female 50 years of age, from whose occipital region he removed a horny excrescence which had been growing 16 years, and which measured $6\frac{3}{4}$ inches in length and 3 inches in circumference at its base.

“She took no notice of it for some time, but, becoming so large as to interfere with the proper adjustment of her cap, and obliging her to raise her head from the pillow at night whenever she wished to turn over in bed, while it also incommoded her from reposing upon her back, she was necessitated to apply to her family physician (a homœopath) for relief, who promised her that in the space of a short time he would be enabled to remove the difficulty by the use of sundry small white pills which he proceeded to furnish her with; while at the same time he assured her that he had treated successfully, to a cure, several similar cases within the past year in his own practice. For five years she continued to use the homœopath’s pills, but still the *horn* remained—still the horn continued to grow. In view of this condition of affairs her faith began to falter, but for some time it was supported with the assurance of her physician “that he had not as yet got hold of the right pills.” Reassured by this assertion, which was repeated from time to time, she kept on until the spring of the present year, when she consulted me, and finally submitted to its removal by the knife.”

Cost of the Doctorate in Paris.—The Union Médicale makes the following estimate of the cost of the Degree of Doctor of Medicine in Paris: The collegiate education requires 7 years, and to obtain the two baccalaureate degrees, 2 years more are necessary; then the medical studies, properly speaking, will average 6 years; making a total of 15 years. The 7 years at college cost 1000 francs per annum, making 7000 francs; the 2 baccalaureates, 320 francs; the 6 years at medical college, 1200 francs a year, or total 7200 francs. Private courses of study, 1000 francs; matriculations, examinations, and diploma fee, 1100 francs; instruments and books, 2000 francs. Making a grand total of 18,620 francs, or about \$3,724.

Danger of Gutta Percha Bougies.—The N. Y. Medical Times contains a notice of three cases in which Gutta Percha bougies have been broken in the bladder or urethra. Two of the cases occurred in New York and the other in England. Bougies made of this material should at once be discarded from use.

Deaths from Chloroform.—The Medical Gazette of Strasbourg furnishes the particulars of a case in which a lady suddenly died during

the extraction of a tooth under the influence of chloroform. Another death occurred last July, in London, during an operation for the removal of the testis under the influence of this agent.

BIBLIOGRAPHICAL NOTICES.

Operative Surgery, based on Normal and Pathological Anatomy. By J. F. MALGAIGNE, Professor, &c., Paris. Translated from the French, by Frederick Brittan, A.B. M.D., M.R.C.S L. Illustrated by wood engravings from designs by Dr. Westmacott. pp. 565. Philadelphia: Blanchard & Lea. 1851.

We are gratified to find this valuable work made accessible to American readers. It has been remarkably well received in France, and is there regarded as a standard production. The translator has not avoided gallicisms as much as he might have done, but this does not materially impair the value of the work.

Minor Surgery; or Hints on the every day duties of the Surgeon. By HENRY H. SMITH, M. D., Assistant Lecturer on Clinical Surgery in the University of Pennsylvania, &c., &c. 3d edition, with numerous additions. Illustrated by 247 engravings. pp. 456. Philadelphia: E. Barrington and G. D. Haswell. 1850.

This is another exceedingly useful manual, emanating from an American. The modesty of its title page should not detract from its intrinsic merit. It is as copious and complete a work of the kind as we know of.

Medical Lexicon—a Dictionary of Medical Science; containing a concise explanation of the various subjects and terms of physiology, pathology, hygiene, therapeutics, pharmacology, obstetrics, medical jurisprudence, &c., with the French and other synonymes; notices of climate, and of celebrated mineral waters; formulæ for various officinal, empirical and dietetic preparations, &c. By ROBLEY DUNGLISON, M. D., Prof. of the Institutes of Medicine, &c., in Jefferson Medical College, Philadelphia. 8th edition, revised and greatly enlarged. pp. 927. Philadelphia: Blanchard & Lea. 1851.

The fact that this Dictionary has now reached its 8th edition, shows how highly it is esteemed by the profession, and renders unnecessary to its farther success any special commendation from us.

New Remedies; with formulæ for their administration. By ROBLEY DUNGLISON, M. D., Prof. of the Institutes of Medicine, &c., in the Jefferson Medical College of Philadelphia. 6th edition, with extensive additions. Philadelphia: Blanchard & Lea. 1851.

To the present edition of the above work, Prof. Dunglison has added much valuable information hitherto scattered through the numer-

ous medical periodicals, and consequently of difficult access to the mass of practitioners. This is one of the most useful of the author's works, and will doubtless be eagerly sought after by those who wish to be posted up in therapeutical applications.

First Principles of Medicine. By ARCHIBALD BILLING, M.D., A.M., F.R.S., &c., &c. 2d American, from the revised and improved 5th London edition. Philadelphia: Lea & Blanchard. 1851.

Billing's Principles of Medicine have been so long and so favorably known as not to need any eulogy from us to secure a continuance of patronage. Suffice it to say that in the present edition the author has spared no pains to make the work even more worthy than it was before of the praise bestowed upon it.

New Medical Journals.—The New York Medical Register is defunct—and “the New York Medical Times” has appeared just in time to take its place. This is edited by J. G. Adams, M. D., and is issued in monthly numbers of 32 pages, at \$2 per annum. The New Orleans Monthly Medical Register, of 12 pages to the number at \$1 per annum, has also been received. We cordially wish them both a successful career.

We acknowledge the reception of the first volume of the “Transactions of the Medical Society of the State of Pennsylvania,” and also of the 4th number of the “Quarterly Summary of the Transactions of the College of Physicians of Philadelphia, both of which contain valuable documents to which we hope soon to be able to refer.

We have received the announcement of the intention of Dr. Frank A. Ramsay, of Knoxville, Tenn., to establish in that city a “Primary Medical School,” as recommended by the American Medical Association. The Doctor has our best wishes for his success.

Medical Colleges in New York.—The highest number counted in any of the lecture-rooms, including all the persons present during any part of the lecture, was as follows, viz:

In the College of Physicians and Surgeons,	197
In the University of New York,	179
In the New York Medical College,	69

The catalogues of each of the schools will doubtless show an increase when published. But the aggregate will be diminished from the last year, chiefly from the falling off of Southern students, very few of whom have yet reached New York.—[*N. Y. Med. Gazette*, Nov. 15.

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